Date: Mon, 2 Dec 1996 08:00:05 +1800
Subject: CSGNET Digest - 1 Dec 1996 to 2 Dec 1996

There are 22 messages totalling 1184 lines in this issue.

Topics of the day:

1. political conflict response options
2. When is behavior?
3. simultaneity (3)
4. Functional analysis (3)
5. Born to Rebel (5)
6. Bill's model (3)
7. Java (3)
8. Transfer functions (2)
9. Rewards in PCT -Reply

Date: Sun, 1 Dec 1996 09:40:25 -0500
From: Bruce Gregory <bgregory@CFA.HARVARD.EDU>
Subject: Re: political conflict response options

[From Bruce Gregory (961201.0940 EST)]

Bill Powers (961201.0345 MST)

> I appreciate Tracy Harms' picking up that quote from B:CP, but I think it
> needs some explanation. My view on the "desire to control other people" is a
> conclusion from a reasoned argument, not a moral homily.

!!!!!I find that the fundamental reorganization produced by PCT comes
from the realization that people _are_ autonomous control systems.
(Less ontologically, that the behavior of people becomes intelligible if
you perceive them as autonomous control systems.) Once this sinks in,
the next question to be addressed is "How can autonomous control
systems get along?" This you address with your customary clarity.

Bruce Gregory

Date: Sun, 1 Dec 1996 11:13:40 -0600
From: Bruce Abbott <abbott@CVAX.IPFW.INDIANA.EDU>
Subject: Re: When is behavior?

[From Bruce Abbott (961131.1210 EST)]

>Bill Powers (961201.0130 MST) --

>Bruce Abbott (961130.1055 EST)

>>>We're talking specifically about the event level, the level at which the
>>>collection of configurations of sensations going through sets of
>>>transition is recognized as a single event, and corrected if it's not
>>>right.
>
>>It's been done; they can do it.

>
>WHAT's been done? They can do WHAT? "Been there, done that" doesn't tell me
>anything. I would really like to know how "they" established that a rat can
>control an event-level perception.

Perhaps I don't have the right idea of "event control," but it is clear that rats can learn to produce a required temporal pattern (if not too complex) in order to receive a reward. Is this what you have in mind? At any rate, if we're just talking about control of relationships, it is no longer relevant to the discussion.

>I was trying to point out that what you're describing is control of a
>relationship between two sets of perceptions. The perceptions in question
>are (1) the arrival of food, and (2) the remainder of the visual, olfactory,
>kinesthetic and so on perceptions that were going on just prior to the
>arrival of food. The "suddenness" of the perceptual food-event is
>irrelevant.

Suddenness may not be a requirement, but I would suggest that a relatively rapid transition from food-absent to food-present provides a time-marker that makes it easier for the rat to identify the relevant perceptions in the stream of ongoing perceptual changes which is rapidly flowing through and out of recent memory.

>The reason that the rats fumble around at the bar for so many hours is that
>they DO NOT initially have a control system for controlling the perceptions
>relevant to depressing the bar in the right way. They don't know which
>perceptions are relevant, and the process by which they find out, and learn
>to control them, is apparently one of trial and error.

Yes, and to some extent there is a random component to the "trials" in that the reproduction of previous acts generally will not reproduce the identical set of perceptions (because of different starting points and other disturbances along the way). But I am suggesting that there is also a non-random component in the selection of "what to try," based on the rat's previous experience.

>> ... I
>>specifically do _not_ mean the actions by which the _doing_ is accomplished.
>>Doing is controlling perceptions via actions; what has to be recreated via
>>actions are certain perceptions that existed at the time of pellet
>>delivery. The problem for the rat is that it doesn't know which of those
>>perceptions need to be controlled (reestablished) to make the food pellet
>>appear; indeed, initially it doesn't know whether any of those perceptions
>>had anything to do with it.
>
>I agree. And initially it may not even know how to recreate any of those
>perceptions such as the feeling of the lever bottoming out.

Yes. However, the point I was making is that the rat does already know how to recreate most of the perceptions that put it in a position whereby it may learn how to recreate other perceptions such as the feeling of the lever bottoming out. It already knows how to approach any given location, how to place its paws on an object, how to hold onto an object (if that is possible) and exert forces against it in various directions, and so on. All the necessary control systems by which the rat can accomplish these acts are already in place and well tuned.

The problem for the rat, then, is to identify the necessary sequence of acts

that will produce the pellet. Given that it will be in contact with the lever on each occasion on which a pellet appears, it rather quickly learns the relationship between these two sets of perceptions, once the rat has had a few experiences in which contact with the lever (as, e.g., a side-effect of exploration) has been followed by pellet delivery. (One question to be addressed is how the rat "builds up" such a relationship perception.)

Having perceived this possible relationship (I say "possible" because it could be coincidental, for all the rat knows about it), the rat will now approach the lever and contact it, or if already at the lever it will simply contact it; the rat does not have to learn _how_ to perform these acts.

However, mere contact, alas, is not enough. The lever must be depressed far enough to trip the switch. From what we've seen of the rat's behavior in this situation, it may never learn that it must depress the lever. It may learn that "pawing" or "digging" at the lever works, or that allowing its weight to come down on the lever works. The particular topography of behavior used to trip the switch may depend largely on luck, on what perceptions happen to correlate with pellet delivery. The rat doesn't appear to be reasoning through the problem and drawing conclusions as a human might (such as, "oh, so _that's_ what I have to do -- press the lever down until it clicks"). [Indeed, I have seen no evidence for such a process even in dogs, whose brains are probably almost two orders of magnitude larger than those of rats).]

>>Yes, I understand that. But it _already_ knows how to create the same
>>perceptions again, except for one: making the pellet appear.

>I dispute that. Why does it take the rats so many sessions to learn to make
>the lever go down?

I hope we're not going to get into a debate about what it means to learn to "make the lever go down." Most of our rats learned enough about this requirement to be earning pellets at a rapid rate by the end of the first session. Our two cyclic-ratio animals acquired within a few minutes of being placed in the chamber.

>The problem I've stated is that of learning to control a relationship
>between two perceptions which may or may not be event-perceptions. If the
>events or transitions or configurations are already under control, then
>that's the only problem (except for how to convert an error in the
>relationship into the specific changes in lower-level reference signals that
>will tend to correct the error). If the lower-level systems for controlling
>the relevant perceptions don't exist, however, then they, too, have to be
>learned. It seems to me that what we saw early in our observations of the
>rats was that they had to learn at several levels. A rat "digging" at a
>lever without depressing it has obviously not yet figured out how to make
>the critical perception occur.

Yes, I agree. But the rat will not persist in "digging" at the lever for long if a pellet never appears during that activity. Given that one does appear at least occasionally, the animal will persist in this mode so long as there is insufficient variability in its actions to permit it to discover a more efficient approach. Rats appear to be sensitive to changes in these relationships: if, e.g., the rat allows its back to sag a bit and as a result the "digging" now produces food pellets more reliably, the rat will learn to maintain its back in this new position while digging (or to maintain more pressure against the lever, i.e., whatever perceptual state seems to be more conducive to the delivery of pellets). Thus, I see a need

for specifying a mechanism within the rat that is able to develop such perceptions from repeated samples of the perceptual input that was occurring at or just prior to the time of pellet delivery.

> . . . You're assuming that as soon as the animal has
>identified the critical perception, it already knows how to make it occur.
>The perception that the rat already knows how to control -- say, the
>"digging" motion at the bar -- is not sufficient to affect the critical
>perception, even supposing that the rat has figured out what it is. This
>motion control system must be reorganized until the ineffective aspects of
>it are changed to become effective. This requires reorganizing output
>connections as well as input functions.

See above.

Re: functional approach

This discussion isn't taking us where I hoped it would, so I'm going to drop it.

>I'm all for the modelling. But once you have such a model, what are you
>going to do? Are you going to say, "SEE? The reinforcements REALLY DO
>sustain the pressing"?

What? The very questions indicate a complete lack of understanding of my position on this issue. There is no further need for a functional approach once one has sufficient understanding of the system to propose mechanistic accounts and test them. Once I have such a model, I'm going to do with it exactly what you would do with it.

Regards,

Bruce

Date: Sun, 1 Dec 1996 09:33:36 -0800
From: David Wolsk <dow@PINC.COM>
Subject: simultaneity

011296 0915 PST from David Wolsk

I have been following the Bruce/Bill dialogue with interest ....no, fascination would describe it better. I label myself on the side of Bill except for one nagging element: as someone who years ago was involved in applying operant techniques for training a monkey to move a joystick "properly", I appreciate the power of the method to shape one's paradigms. The image which brings me to this current discussion is of my switching on a light and hearing a strange noise at the same time.
My experience is of an immediate rise in my level of alertness and a need to "process" that noise. What I'm getting at is that the brain seems programmed for simultaneity connected directly to our movements as a "special case".
In PCT terms, perhaps, it indicates the lowest level in the hierarchy. But I do feel that Bruce's defending "the event" derives from this. Perhaps it also is at the root of the mass of intelligent psychologists who love to be in control taking up operant techniques ..... mostly men I believe.

David Wolsk Victoria, BC Canada

Date: Sun, 1 Dec 1996 10:49:15 -0700
From: Bill Powers <powers_w@FRONTIER.NET>
Subject: Functional analysis

[From Bill Powers (961201.0930 MST)]

Here is a "functional analysis" that doesn't introduce any unobservable relationships:

Let B be a measure of the rate of behavior

Let B' be the observed asymptotic value reached by B.

Let C be a measure of the rate of occurrance of a (potential) consequence of the behavior.

Let f be the observed dependence of C on B.

Then during "acquisition" we observe, approximately, that

While C = f(B)

dB/dt = k*(B' - B)

and C = f(B)

and during "extinction" we observe that

While C = 0

dB/dt = - k*B

>From this we conclude that during acquisition B' is nonzero and during extinction it is zero.

One model that could explain thee observations would be

dB/dt = k1 * (C' - C) - k2*B,

where C' is a constant to be evaluated from the data, and
k1 is nonzero during acquisition and zero during extinction, and
k2 is a constant to be evaluated from the data.

During extinction, k1 would have to become zero. If there is an initial rise in behavior after the nominal start of extinction, then k1 would be set to zero after a delay.

The analysis is based on fitting a curve to the observed behavior of the data. The proposed model is introduced as a separate step.

Note that this model proposes that on initial establishment of the contingency C = f(B), the rise in B is produce entirely by the effect of the system constant C'. At t = 0, db/dt = k*C'. Then as time passes, the behavior rate rises more and more slowly as C increases.

To put this into words, it is the organism's reference setting for a certain amount of C that produces the initial rise in behavior rate; the effect of C

is to inhibit the rise in behavior rate. So the effect of the consequence is to inhibit the behavior that produces it.

This is in contrast to the proposal that the effect of C is to support or maintain the behavior that produces it. The function of "inhibition" is surely distinguishable from the function of "support."

Best,

Bill P.

Date: Sun, 1 Dec 1996 12:47:37 -0500
From: Bruce Gregory <bgregory@CFA.HARVARD.EDU>
Subject: Re: Born to Rebel

[From Bruce Gregory (961201.1245 EST)]

"This chapter has two main goals. The first is to combine all of the significant trends I have already documented about family dynamics and revolutionary personality into a single predictive model. My second goal is to do everything I can think of to break this model. I will therefore try to be my own worst critic by emphasizing the model's greatest shortcomings. By focusing on the model's mistakes -- particularly the individuals who most defy its predictions -- we can begin to appreciate the limitations of a family dynamics model _and to highlight the kinds of influences it fails to include_. It is the model's errors, not its success stories, that prove useful insights for further understanding."

Frank Sulloway
Born to Rebel: Birth Order, Family Dynamics, and Creative Lives

Bruce Gregory

Date: Sun, 1 Dec 1996 11:34:39 -0700
From: Bill Powers <powers_w@FRONTIER.NET>
Subject: Re: simultaneity

[From Bill Powers (961201.1100 MST)]

>The image which brings me to this current discussion is of my switching on a
>light and hearing a strange noise at the same time.
>My experience is of an immediate rise in my level of alertness and a need to
>"process" that noise. What I'm getting at is that the brain seems
>programmed for simultaneity connected directly to our movements as a
>"special case".

I agree. You're saying that there's a good evolutionary reason for our having event perceptions and for our ability to perceive simple temporal relationships among them. And an unexpected temporal relationship, it's reasonable to suppose (as experience confirms) is an _uncontrolled_ perception. Flipping a light switch is not supposed to be followed instantly by a loud bang. In fact, it is supposed NOT to be followed by anything but a

light turning on or off.

I can confirm all of this from my own experience. But I don't know how the same temporal relationship would seem to a rat. There have certainly been many studies of classical conditioning in which the effects of temporal lags have been well studied, but we have no way to know, on the basis of those studies alone, why this effect occurs. It could result from the perception of temporal relationships, or in a very simple animal it could be explained just in terms of the decay-time of perceptual signals following sudden changes. The problem with mechanistic models is that there is often more than one that would work, and it's not easy to think up experiments that would force us to choose one over the others.

>I do feel that Bruce's defending "the event" derives from this.

I know, and I respect the observation. But how do you distinguish between an organism with an event level of control, and an organism that simply reacts to changes in lower-level perceptions because they induce changes in error signals? This is why I have twice brought up the lever example. If you apply an event-disturbance to one end of the lever, the other end will produce an event-response to it -- but I don't think anyone would claim that a lever can perceive events. It's possible that rats CAN explicitly perceive events, and correct them while they're in progress if they're disturbed. But even a configuration control system can SEEM to respond to events, if you apply a disturbance to it that a human being can recognize as a familiar event.

If someone holds an arm steady while you give it three rapid pushes, one-two-three, the person will respond with three rapid tensings of the opposing muscles, one-two-three. But that is a _position_ control system, not an event control system.

>Perhaps it
>also is at the root of the mass of intelligent psychologists who love to be
>in control taking up operant techniques ..... mostly men I believe.

A sensitive point, because we're very short on females in PCT as well. There are certainly some who have a very competent grasp of PCT, but girls still aren't raised to believe that anything they could say on such matters could possibly impress anyone, particularly not a male. As to being in control, I think that if anything women have more reason to want to do this than men do. The main difference is that women have to find means of control that don't rely on superior physical strength, such as dangling sexual lures just out of reach in the direction they want a man to go. Control by any means, however, that doesn't avoid creating errors in another person, is a good recipe for discord and violence. I don't need to cite examples; just open a newspaper.

Best,

Bill P.

Date: Sun, 1 Dec 1996 13:16:38 -0500
From: MILLER dan <millerd@SABER.UDAYTON.EDU>
Subject: Re: Born to Rebel

[Dan Miller (961201)]

To Bruce Gregory (961130) and other CSGNETTERS:

Sorry to have been away for so long. Lots of happenings, and more work than I could do in the time allotted. I won't bore you with particulars.

Regarding the Sulloway book, "Born to Rebel", Bruce Gregory is giving us some tantalizing quotes - sort of like reading the dust cover. Actually, the book and his research is worth reading and thinking about. I have plowed through quite a bit of it and find it interesting, infuriating, right on, and downright dumb at various times.

His argument is that birth order makes THE significant difference in the development of self (he uses personality, but most psychology types do). It is not birth order per se, but rather birth order indicates that children find themselves in different interaction niches (he uses niche as do most evolutionists). That is to say, that first borns gain the full attention of parents (no sharing for a while), and thus, the reference signals of parents are most likely to be those adopted. In turn, parents themselves provide a major source of disturbances. Reorganization follows in such a context.

BTW, I hope you all don't mind if I put a PCT turn on his argument. Also, there will be a tendency for me to see his argument as a symbolic interactionist. We can'
(oops) can't all be perfect.

Later borns (2nd to nth) are born into a sociological world in which the relationship between child and parent must be shared. In addition, later borns have older siblings to contend with. The niche has changed to one of competition, conflict, and so on. First borns take their advantage - size, experience, etc. - and use them. They want to perceive themselves as "one up", privileged, prestigious, or whatever. Sulloway argues that first borns tend to favor a conforming, status quo, orientation, whereas later borns by the fact that they are competitive and conflict with their sibs must adopt a different strategy and set different reference signals. Thus, they tend to be more creative, rebellious, unconventional, and contrary with regard to the first born and, often, the parents.

Sulloway uses all kinds of data (most of it would be washed out in an MA exam at a decent university), including historical accounts of revolutionaries and scientists. Not surprising, most of these people were laterborns. Firstborns become corporate executives, accountants, and presidents. Clearly, Sulloway favors laterborns, and as I read it, I did too. It tends to fit with my biography (although the morning astrology reading does, too). I am the third born out of three. However, I most certainly like the company of laterborns as identified by Sulloway. Also, most of my friends were laterborns. The departmental chairmen in my department - since I arrived here (University of Dayton) eighteen years ago have all been firstborns - and they fit Sulloway's profile.

So what? Well, I suppose I should not try to be a departmental chairman or a dean, and get back to trying to bring about a scientific revolution. Seems like a good idea to me.

Later,
Dan Miller
miller@riker.stjoe.udayton.edu

Date: Sun, 1 Dec 1996 14:34:14 -0500
From: Bruce Gregory <bgregory@CFA.HARVARD.EDU>
Subject: Re: Born to Rebel

[From Bruce Gregory (961201.1435 EST)]

Dan Miller (961201)
>
> Sulloway uses all kinds of data (most of it would be washed out
> in an MA exam at a decent university), including historical accounts
> of revolutionaries and scientists.

You may be right that the data would be washed out in an MA exam at a decent university, but I suspect that is true of much data in the social sciences. Sulloway gave a talk at a history of science seminar here a year or so ago. The audience was _very_ skeptical. I think it's fair to say that most of us came away believing that Sulloway had done a very thorough job in _trying_ to gather meaningful data. Rankings on the scales he uses were made independently by historians of science apperently with few axes to grind. I agree that the book is worth reading and thinking about -- something I can say for fewer and fewer books I try to read.

Bruce Gregory

Date: Sun, 1 Dec 1996 13:42:24 -0600
From: Bruce Abbott <abbott@CVAX.IPFW.INDIANA.EDU>
Subject: Re: Functional analysis

[From Bruce Abbott (961131.1440 EST)

>Bill Powers (961201.0930 MST) --

>Here is a "functional analysis" that doesn't introduce any unobservable
>relationships:

O.K., maybe I'll give it one more try.

>Let B be a measure of the rate of behavior

>Let B' be the observed asymptotic value reached by B.

>Let C be a measure of the rate of occurrance of a (potential) consequence of
>the behavior.

>Let f be the observed dependence of C on B.

>Then during "acquisition" we observe, approximately, that

>While C = f(B)

> dB/dt = k*(B' - B)

> and C = f(B)

>and during "extinction" we observe that

>While C = 0

> dB/dt = - k*B

>From this we conclude that during acquisition B' is nonzero and during
>extinction it is zero.

O.K. Call B' the asymptotic rate observed during acquisition and B* the asymptotic rate observed during extinction. Taking the purely descriptive approach, what can we conclude about the necessary conditions for B' > B*? We can conclude that C must be a function of B (and, by the way, a positive function at that). Because behavior occurs at a higher rate during the contingency than in its absence, we can say that this contingency supports a higher rate of responding than the noncontingency (extinction). We are _not_ stating that C (the _rate_ of the contingent event) drives B (the _rate_ of responding). We are just describing the relationship observed between the presence/absence of the contingency and asymptotic rate of responding B', in language you prefer not to use ("supports") because you take it to be a statement of direct causality.

>One model that could explain thee observations would be
>
>dB/dt = k1 * (C' - C) - k2*B,
>
> where C' is a constant to be evaluated from the data, and
>   k1 is nonzero during acquisition and zero during extinction, and
>   k2 is a constant to be evaluated from the data.
>
>During extinction, k1 would have to become zero. If there is an initial rise
>in behavior after the nominal start of extinction, then k1 would be set to
>zero after a delay.
>
>The analysis is based on fitting a curve to the observed behavior of the
>data. The proposed model is introduced as a separate step.

Now, in this separate step, you are introducing a mechanism to explain the observed relationship between contingency/noncontingency and asymptotic rate of behavior. You have moved from the purely descriptive functional approach I have been attempting to describe to a mechanistic one. It is also a step that reinforcement theorists have sometimes taken, based on the assumption that reinforcers have a direct, causal influence in the nervous system that leads to the observed, purely descriptive relationships.

>This is in contrast to the proposal that the effect of C is to support or
>maintain the behavior that produces it. The function of "inhibition" is
>surely distinguishable from the function of "support."

The purely functional (descriptive) approach is _not_ that C (the _rate_ of contingent event C) drives the rate of behavior B, but rather, that the presence/absence of a contingency between the individual behavioral event b and event c [from which can be derived the function C = f(B)] is positively related to B'. Another way to put it, I suppose, would be to state that,

under the conditions under which these observations hold, B = f(k1).

Regards,

Bruce

Date: Sun, 1 Dec 1996 16:30:15 -0600
From: Bruce Abbott <abbott@CVAX.IPFW.INDIANA.EDU>
Subject: Bill's model

[From Bruce Abbott (961201.1730 EST)]

How about that, my watch-dial has been lying to me: it's _not_ November 31st!

>Bill Powers (961201.0930 MST) --

>One model that could explain thee observations would be
>
>dB/dt = k1 * (C' - C) - k2*B,
>
> where C' is a constant to be evaluated from the data, and
> k1 is nonzero during acquisition and zero during extinction, and
> k2 is a constant to be evaluated from the data.
>
>During extinction, k1 would have to become zero. If there is an initial rise
>in behavior after the nominal start of extinction, then k1 would be set to
>zero after a delay.
>
>The analysis is based on fitting a curve to the observed behavior of the
>data. The proposed model is introduced as a separate step.
>
>Note that this model proposes that on initial establishment of the
>contingency C = f(B), the rise in B is produce entirely by the effect of the
>system constant C'. At t = 0, db/dt = k*C'. Then as time passes, the
>behavior rate rises more and more slowly as C increases.
>
>To put this into words, it is the organism's reference setting for a certain
>amount of C that produces the initial rise in behavior rate; the effect of C
>is to inhibit the rise in behavior rate. So the effect of the consequence is
>to inhibit the behavior that produces it.

Although Bill provided this model simply as an illustration of method, it is worth pointing out that the results we (Bill and I) have been obtaining in the lab over the past several months argues against this model, at least when the particular function relating C to B is C = nB, where n is the number of responses required for one pellet delivery. It appears that our rats do not set a reference for the _rate_ at which pellets appear.

So what might be going on instead? Imagine you have two control systems, one whose reference is to perceive a food pellet in the chamber (System 1) and one whose reference is to perceive food in the belly (System 2). System 2 is the primary system in the sense that System 1 exists only so that System 2 has the means to do its job. [Note: the actual chain of systems would be longer; I've simplified it a bit for the sake of exposition.]

In the absence of a pellet in the chamber, System 2 has no pellet to swallow, so it is unable to contol the level of food in the belly. However,

System 1 is able to control its CV, and furthermore, it is experiencing error as there is currently no pellet present. It goes to work, the lever gets pressed, and a pellet appears. Now that a pellet is present, System 2 is able to take action to control its CV, so the rat grabs the pellet and swallows it. But this disturbs System 1's CV (again there is no pellet in the chamber), and System 2 takes action to restore that CV to pellet-present.

The amount of time required to complete each press-eat cycle will depend on the physical requirements imposed by the environment and the rat's efficiency, and the vigor with which the rat carries out the required activities, to name some major factors. (The latter may be determined by the gains of the systems involved.) The inverse of this time is C, the frequency of event c that is presumed to be the controlled variable in the model Bill presented as an example. In the present analysis this frequency is not controlled, but emerges as a byproduct of control by Systems 1 and 2.

Of course, this is only another proposed model; others are no doubt possible. Through experiments such as those Bill and I have been running, we are able to evaluate the merits of any given proposal by comparing its predictions to the data under various conditions. To our disappointment, the first casualty of this process was the elegantly simple model Bill described above.

Regards,

Bruce

Date: Sun, 1 Dec 1996 17:27:24 -0600
From: Bruce Abbott <abbott@CVAX.IPFW.INDIANA.EDU>
Subject: Re: simultaneity

[From Bruce Abbott (91201.1825 EST)]

>011296 0915 PST from David Wolsk --

>The image which brings me to this current discussion is of my switching on a
>light and hearing a strange noise at the same time.
>My experience is of an immediate rise in my level of alertness and a need to
>"process" that noise. What I'm getting at is that the brain seems
>programmed for simultaneity connected directly to our movements as a
>"special case".

Yes, excellent observation, David. We seem to be especially primed to take note of unexpected change, and there seem to be good evolutionary reasons why this should be so.

>I have been following the Bruce/Bill dialogue with interest ....no,
>fascination would describe it better. I label myself on the side of Bill
>except for one nagging element . . .

As Bill and I have covered a fair amount of ground, I would like to know specifically what you find yourself "on the side of Bill" with respect to and your reasoning. One reason I offer these ideas (some would less charitably call them "misconceptions"!) is _for_ the criticism -- it helps me to evaluate them.

Regards,

Bruce

Date: Sun, 1 Dec 1996 17:55:57 -0600
From: Bruce Abbott <abbott@CVAX.IPFW.INDIANA.EDU>
Subject: Re: Born to Rebel

[From Bruce Abbott (961201.1855 EST)]

>Dan Miller (961201) --

>Sulloway argues that first borns tend
>to favor a conforming, status quo, orientation, whereas later borns
>by the fact that they are competitive and conflict with their
>sibs must adopt a different strategy and set different reference
>signals. Thus, they tend to be more creative, rebellious, unconven-
>tional, and contrary with regard to the first born and, often, the
>parents.

In that case, as a small test of this proposition, I'd like to know where Bill Powers falls along the birth-order scale. Bill?

Regards,

Bruce

Date: Sun, 1 Dec 1996 17:51:26 -0700
From: "T. B. Harms" <tbh@TESSER.COM>
Subject: Re: Bill's model

>From Tracy Harms (1996;12,01.17:48)

Bruce Abbott (961201.1730 EST)

>[...]Imagine you have two control systems,
>one whose reference is to perceive a food pellet in the chamber (System 1)
>and one whose reference is to perceive food in the belly (System 2). [...]
>Now that a pellet is present, System 2
>is able to take action to control its CV, so the rat grabs the pellet and
>swallows it. But this disturbs System 1's CV (again there is no pellet in
>the chamber), and System 2 takes action to restore that CV to pellet-present.

This last sentence evaporates my willingness to believe in System 1. Whatever would generate a goal such as that? If we attribute eating the food to System 2, we must also assign the obtaining of food to be eaten to System 2. Why? Because if System 1 were *disturbed* by the removal of food from the environment, the rat would act to perceive food in its environment *even if it were not hungry*. I don't know as much about rats as you guys do, but that's too far-fetched for me.

Tracy Bruce Harms tbh@tesser.com
Boulder, Colorado
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"The saying goes that you can fool your friends, but you can't fool

yourself. Well, I believe the opposite is much closer to the truth."
-- Tyson Vaughan

Date: Sun, 1 Dec 1996 20:07:46 -0700
From: Bill Powers <powers_w@FRONTIER.NET>
Subject: Re: Born to Rebel

[From Bill Powers (961201.1830 MST)]

Bruce Abbott (961201.1855 EST)--

>In that case, as a small test of this proposition, I'd like to know where
>Bill Powers falls along the birth-order scale. Bill?

I'm the first-born, with one younger sister.

Best,

Bill P.

Date: Sun, 1 Dec 1996 20:07:49 -0700
From: Bill Powers <powers_w@FRONTIER.NET>
Subject: Re: Bill's model

[From Bill Powers 961201.1840 MST(]

Tracy Harms (1996;12,01.17:48) --

> If we attribute eating the
>food to System 2, we must also assign the obtaining of food to be eaten to
>System 2. Why? Because if System 1 were *disturbed* by the removal of
>food from the environment, the rat would act to perceive food in its
>environment *even if it were not hungry*. I don't know as much about rats
>as you guys do, but that's too far-fetched for me.

I think Bruce's model will work pretty well in terms of plausibility. Presumably system 2 (food in belly) would correct an error by sending a nonzero reference signal to system 1 (food in cup, or perhaps better, food in mouth). System 2, in turn, would get its reference signal from System 3, which controls something like the concentration of nutrients in the bloodstream. When the concentration of nutrients is less than the reference level (hunger), the error signal is translated into a non-zero setting for food in the belly. If the amount of food in the belly is less than the reference level, the error is translated into a nonzero reference level for food in the cup (mouth). If the organism isn't hungry, its reference level for food in belly will be zero, and if there's no error there, the reference level for food in mouth will be zero. These are one-way control systems: an excess of the controlled variable simply produces a zero error signal. So if the organism isn't hungry (level 3), it won't be trying to get food in the mouth (level 1) and removing the food from the environment won't make any difference.

Here's a sort of progress report, or a first cut at one.

Bruce is right in saying that my "model" in the discussion of functional

analysis is hypothetical, in that rates of pressing are not, apparently, a varible in the behavior of our rats. The rat is either pressing at some mostly constant rate, or it's not pressing (doing something else, like grooming, exploring, or sleeping).

However, we've been able to get a fairly good control model by skipping the lower levels that Bruce is trying to model now, and defining the action of the system as the food intake provided by lower-level systems with the weight as the controlled variable. "Weight" is not very closely controlled -- the loop gains we infer run from roughly 2 to 10. Both of us think that weight is probably an indirect indicator of something else, perhaps nutrient level, that is hard to measure directly. Food intake per day does vary considerably, because of changes in the amount eaten; the rats may quit pressing the lever halfway through the 1-hour session, or later, or earlier, and the collection-times lengthen during the session. The only thing that doesn't change much is the rate of lever-pressing!

Making the modeling more difficult is the fact that there are LARGE variations in intake that aren't correlated with any other system variable. One variation seems to have a 4 to 6 day cycle, which coincides with the estrus cycle of female rats. We're talking about fast variations that are equal to the total range of changes in the smoothed (5-day) rate of eating. Also, in getting the hang of this type of experiment, we began with the usual standard conditions, and have changed something important about the conditions at least half a dozen times during the 9-month run. The first thing to go was the experimenter's trying to control the rat's weight at the same time the rat was trying to control it. Our models, until very recently, were being fitted to ALL the data points through all the changes in conditions (three fixed parameters). The best results have come when we start the model run at about day 110 (of 290 so far), after the major changes in conditions have stopped. This implies that in any follow-on study we will put the experimental rats under one simple constant set of conditions, and vary only one manipulated variable (the main one being limits on food available in the home cage).

Our basic problem is that we have picked a controlled variable that is disturbed by practically everything that the rat does or that happens to it, most of which disturbances we can't measure directly and many of which originate in the rat's own biochemical cycles and maturational processes. Our means of dealing with these unknown disturbances are limited; if we calculate them exactly, we get a perfect, but meaningless, fit. It's hard to work out just what is a legitimate approximation to the disturbances and what is a fudge factor. Bruce and his hard-working lab assistants have gone to considerable lengths to record such things as water intake and waste products, including food spilled rather than eaten, but these efforts turned out to account for only about half of the observed variations. The books don't balance. I think we're pretty lucky to get as good a fit of model to data as we do.

One day I'd like to see us tackle a simpler problem involving motor control, in which the disturbances will be better observable and manipulable, and where fewer unknown processes can interfere. But what we're doing now is revealing some fascinating facts about the literature of EAB, and that is probably enough of an accomplishment for the present. Also, we're pretty sure we can now claim to have demonstrated that weight control in the face of intake disturbances does happen.

Best,

Bill P.

Date: Sun, 1 Dec 1996 20:07:56 -0700
From: Bill Powers <powers_w@FRONTIER.NET>
Subject: Re: Functional analysis

[From Bill Powers (961201.1930 MST)]

Bruce Abbott (961131.1440 EST) --

>O.K. Call B' the asymptotic rate observed during acquisition and B* the
>asymptotic rate observed during extinction. Taking the purely descriptive
>approach, what can we conclude about the necessary conditions for B' > B*?
>We can conclude that C must be a function of B (and, by the way, a positive
>function at that).

I would say that we _observe_ that C is a function of B; the form of the function is given by the contingency that is programmed into the apparatus, or that we can find by analyzing the physical environment.

>Because behavior occurs at a higher rate during the
>contingency than in its absence, we can say that this contingency supports a
>higher rate of responding than the noncontingency (extinction).

The contingency is the form of the function making C dependent on B. YOU can say that it supports a higher rate of responding (to what?), but I wouldn't say that. As you are fully aware. I have no idea why you even WANT to say that.

>We are
>_not_ stating that C (the _rate_ of the contingent event) drives B (the
>_rate_ of responding). We are just describing the relationship observed
>between the presence/absence of the contingency and asymptotic rate of
>responding B', in language you prefer not to use ("supports") because you
>take it to be a statement of direct causality.

I take it to be an unjustified statement of dependency. If it's not a statement of causality, why use words that are synonyms of "cause?" What's the purpose of using such terms?

>
>>One model that could explain these observations would be
>>
>>dB/dt = k1 * (C' - C) - k2*B,

>The purely functional (descriptive) approach is _not_ that C (the _rate_ of
>contingent event C) drives the rate of behavior B, but rather, that the
>presence/absence of a contingency between the individual behavioral event b
>and event c [from which can be derived the function C = f(B)] is positively
>related to B'.

Fine, you CAN describe this relationship without using terms like "support" or "maintain." If you're saying only that the behavior changes when the dependency of C on B changes, that's acceptable to me. We would expect the behavior of a system to change when the characteristics of one of its components changes.

>Another way to put it, I suppose, would be to state that,
>under the conditions under which these observations hold, B = f(k1).

Yes, but this doesn't tell us what k1 is a function of, so B remains undefined. I proposed that k1 is nonzero when the contingency is in effect, but that doesn't explain anything; the form of a function can't have any physical effects. To complete the model we would have to make a proposal about what variables that the organism could sense are affected in a recognizeable way when the contingency is present or absent. Perhaps the error signal in the control system would be a sufficient indicator. A large rise in the error signal would say that pressing the bar is no longer maintaining the input at its former level (and here I can use "maintaining" because we are talking about a causal relationships).

Best,

Bill P.

Date: Sun, 1 Dec 1996 20:23:08 -0700
From: Bill Powers <powers_w@FRONTIER.NET>
Subject: Re: Java

[From Bill Powers (961201.2015 MST)]

I have discovered that the Windows 3.x versions of Netscape that are available won't support Java. The only way I can see Rick's Java demos, apparently, is to upgrade to Windows 95 which runs 32-bit programs.

The main reason I don't want to switch to Windows 95 is that I've been told that there isn't any way to drop into DOS to run programs that need exclusive use of the machine. Even in a DOS session, there is still time-sharing going on, with W95 imposing an overhead, just like in a Mac. I don't know if this is true, or if it's true how serious it is. Can anyone tell me I'm right, or reassure me that I am wrong and can go ahead with W95?

Best,

Bill P.

Date: Sun, 1 Dec 1996 22:43:39 -0500
From: David <dgoldstein@P3.NET>
Subject: Re: Java

Bill Powers wrote:
>
> [From Bill Powers (961201.2015 MST)]
>
> I have discovered that the Windows 3.x versions of Netscape that are
> available won't support Java. The only way I can see Rick's Java demos,
> apparently, is to upgrade to Windows 95 which runs 32-bit programs.
>
> The main reason I don't want to switch to Windows 95 is that I've been told
> that there isn't any way to drop into DOS to run programs that need
> exclusive use of the machine. Even in a DOS session, there is still
> time-sharing going on, with W95 imposing an overhead, just like in a Mac. I
> don't know if this is true, or if it's true how serious it is. Can anyone
> tell me I'm right, or reassure me that I am wrong and can go ahead with W95?

>
> Best,
>
> Bill P.

Bill,

I have run your tracking programs in Windows 95. You know, the ones that you made for Martin. It works fine. One nice feature is that it keeps track of the commands so that one can easily go to the next trial.

Also, It is possible to print out the graphs in color using the Paint program.

Best,

David

Date: Mon, 2 Dec 1996 04:17:08 -0700
From: Bill Powers <powers_w@FRONTIER.NET>
Subject: Re: Java

[From Bill Powers (961202.0500 MST)]

John Anderson, Bill Leach, David Goldstein --

Thanks for the info about Win95. I guess I'll go ahead and do it, after a full backup. Got to see that Java stuff.

Best,

Bill P.

Date: Mon, 2 Dec 1996 13:05:26 +0100
From: "J.A. Blom" <J.A.Blom@ELE.TUE.NL>
Subject: Re: Transfer functions

(Bill Powers (961121.0930 MST))

I'm fully unable to keep up with you guys, so I'll be brief.

>I don't know why it has taken me so long -- decades! -- to find the
>following way of explaining the difference between PCT learning and
>other kinds, but here it is:

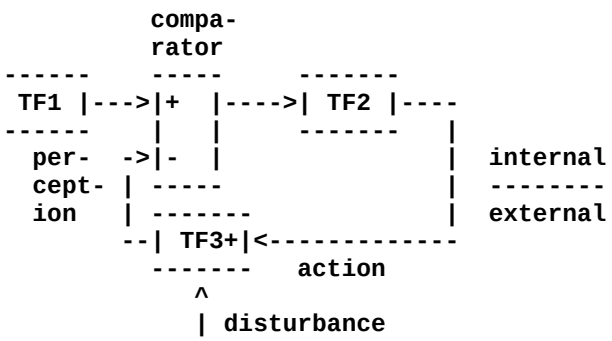
>==========
>Reorganization does not create acts or actions or behaviors; it
>creates transfer functions.
>==========

>Bruce and Rick know what transfer functions are, and I'm sure that
>Hans and Martin and a few others are nodding in agreement...

As to me: yes! But _which_ transfer functions?

The most important one is the _closed loop_ transfer function, which, on the PCT model, starts at the comparator output and ends at the comparator's perceptual input. It determines the overall stability of the closed loop, i.e.; how the controller reacts to external disturbances: oscillatory, sluggish or just right. The closed loop transfer function runs through both the organism and its environment and, assuming the latter to be fixed, the former must adjust/adapt/learn.

But there is a second important transfer function as well. Whereas the first one describes how external disturbances are handled, this one describes how internal drives are handled. It starts at the point where the reference for the comparator originates.



Given TF3, TF2 can be adjusted. Then, given TF2, TF1 can be adjusted as well. PCT usually disregards TF1 for ease of analysis, but practice shows that even a simple transfer function TF1 (often including a differentiator) can improve things dramatically.

Anyway, _two_ transfer functions are important. One that describes how we _react_ to external disturbances, and the other how we _act_ upon internal drives. These two ought to be adjusted independently for best performance. That is, at least, what control engineering tells us.

This brief analysis tells us that it makes no sense to fine-tune TF1 as long as TF2 has not stabilized (although TF1 might perhaps change concurrently with TF2). This tells us that, in an organism's learning process, accurate reactions to external disturbances will be learned at an earlier time -- or at least not at a later time -- than accurate actions to fulfill internal desires.

Doesn't that seem rather counter-intuitive?

Greetings,

Hans

Date: Mon, 2 Dec 1996 13:46:21 +0100
From: "J.A. Blom" <J.A.Blom@ELE.TUE.NL>
Subject: Re: Transfer functions

[Hans Blom, 961202b]

(Tracy Harms (961121.1446 MST))

>Why is the qualifier 'transfer' in the label transfer function?

This is control engineering jargon.

>Does it somehow denote that it applies to an input-output device?

Yes. More accurately, to any "causal" system, in particular to sensors, actuators, wires and cables, and physical computing elements. In control engineering, the term "causal" means that the system's output ("response") will not change _before_ its input changes.

The term "transfer" denotes how a device's output responds to its input at any particular frequency; the parameters of interest are, when the input is a sine wave, gain (output amplitude divided by input amplitude) and phase shift. Discovering a system's transfer function is thus easiest when offering sine waves of all possible frequencies to the input and measuring what happens at the output. A control engineer can establish the mathematical formula of the transfer function from the plots of gain and phase shift.

Usually the term "transfer function" is used in linear systems only; in non-linear systems it makes little sense. An element's transfer function carries the same information about a (linear) system as e.g. an impulse response or a step response.

Note that knowledge of a system's transfer function does not imply knowledge of the internal organization of the system. In theory, at least, different systems can have identical transfer functions.

>It seems to me that the simple term 'function' might be adequate.

This is an established term in control engineering circles. It refers to a physical device, whether existing or in the process of being designed. Thus there are some connotations: a transfer function is _physically realizable_. This means, for instance, that it cannot contain anticipation terms (which would make it non-causal; in fact, any "causal" physical system introduces some delay on the signal's way from input to output), and that it cannot have an infinite gain at any frequency.

>If not, I'd be inclined to use the term I know from data processing,
>'transform'.

A transform, such as the Fourier transform, usually has a (lossless) counter-transform. This is not true for a transfer function, if only because of the delay. If we didn't have to stick to "causal" systems, control engineering would be easy...

Greetings,

Hans

Date: Mon, 2 Dec 1996 13:56:04 +0100
From: "J.A. Blom" <J.A.Blom@ELE.TUE.NL>

Subject: Re: Rewards in PCT -Reply

[Hans Blom, 961202c]

(Tracy Harms (961124.15 MST))

>... intention to use people is a slaver mentality. It turns us
>directly beyond psychology, to questions of ethics.

In my opinion (and according to PCT, I think), we use the world and all that is in it to achieve our own personal aims. That includes people. Is it the _intention_ to use people that is bad? I cannot carry a heavy table by myself, so I intentionally ask someone's help. Is that _using_ that person? Sure!

I guess you mean something else, but I'm not quite certain what...

Greetings,

Hans

End of CSGNET Digest - 1 Dec 1996 to 2 Dec 1996
****

Date: Tue, 3 Dec 1996 08:00:35 -0600
Subject: CSGNET Digest - 2 Dec 1996 to 3 Dec 1996

There are 20 messages totalling 1093 lines in this issue.

Topics of the day:

1. Vancouver's experiment (3)
2. Bill's model
3. Descriptions, theories, and facts
4. Transfer functions, utilization.
5. Born to Rebel (4)
6. Chutzpah, thy name is Hans
7. Jumping Spiders
8. Functional relationships
9. He's a rebel and he'll never ever be any good (3)
10. Searchable Archives for CSGnet (2)
11. Stat Problem
12. simultaneity / modelling

Date: Mon, 2 Dec 1996 17:52:15 +0100
From: "J.A. Blom" <J.A.Blom@ELE.TUE.NL>
Subject: Re: Vancouver's experiment

[Hans Blom, 961202d]

(Jeff Vancouver 961125.09:55 EST)

>Below are the data from several different type of [spiral tracking]
>runs.

Thanks for the data, Jeff. I would have done something similar (blanking the screen feedback rather than closing eyes) if I had had the time. Your results are also about as I would have expected ;-).

> 6.44 visible
>115.10 Alt 10s
> 28.38 Alt 2s

>I think that the data (mine and others) clearly demonstrate that we
>use closed-loop control. If there are any open-loop only advocates
>out there, I invite a different interpretation.

My interpretation: we have a built-in "world model" that allows us to somehow predict the effect of our actions even if perceptual feedback is temporarily unavailable. The quality of the prediction will generally deteriorate after some time, and your data show how bad this deterioration is for this rather complex, dynamic task. For very short times, "tracking" is very good in tasks such as this; for times greater than a few seconds it is pretty bad.

For other tasks (mostly in a static environment), the model will not deteriorate as rapidly. Tracking a straight line, for instance, would be a lot easier ;).

>The point I was trying to make is that control with the mouse,
>screen, or whatever blanked (i.e., model-based control), is better
>than random, static (no movement), or linear trajectory performance.

Yes, that was the point I was trying to make as well.

>I think that reconciliation is in the nature of the transfer
>function _we all think_ is created to track the spiral. That given
>sufficient time to create such a function, it can create a
>controllable perception of the movement of the target even without
>the target being visible.

Yes, that is how I would say it as well. Learning installs a transfer function that we can even rely on (more or less) if there is no feedback. I have this mental model, for instance, of how I can open my front door. I need to use a key, insert it into the keyhole, turn it to the left for almost 360 degrees, push a little against the door, etc. That model is pretty reliable: no random trial and error required. Not all models are that accurate, however. And I'm pretty uncertain about the possible concomitants of accurate models, except how static/dynamic the situation is, i.e. the degree of "disturbance" that the world adds.

We humans like predictability a lot, and we pretty much organize our world to be predictable. No shape-shifting houses, cars, roads, etc. Even education/enculturation has as its main goal to get people to behave predictably.

>This function will not be as good as it would with on-line data, but
>it will be better than if no function were there (as indicated by
>comparing to random, no movement, or linear/trajectory movement).
>Any takers?

Well, slightly belated...

Greetings,

Hans

Date: Mon, 2 Dec 1996 11:05:15 -0600
From: Bruce Abbott <abbott@CVAX.IPFW.INDIANA.EDU>
Subject: Re: Bill's model

[From Bruce Abbott (961202.1205 EST)]

>Tracy Harms (1996;12,01.17:48) --

>>Bruce Abbott (961201.1730 EST)

>>[...]Imagine you have two control systems,
>>one whose reference is to perceive a food pellet in the chamber (System 1)
>>and one whose reference is to perceive food in the belly (System 2). [...]
>>Now that a pellet is present, System 2
>>is able to take action to control its CV, so the rat grabs the pellet and
>>swallows it. But this disturbs System 1's CV (again there is no pellet in
>>the chamber), and System 2 takes action to restore that CV to pellet-present.

>This last sentence evaporates my willingness to believe in System 1.
>Whatever would generate a goal such as that? If we attribute eating the
>food to System 2, we must also assign the obtaining of food to be eaten to
>System 2. Why? Because if System 1 were *disturbed* by the removal of
>food from the environment, the rat would act to perceive food in its
>environment *even if it were not hungry*. I don't know as much about rats
>as you guys do, but that's too far-fetched for me.

Your point is well taken. The problem you perceive is not the fault of the model but arises because I purposely left out certain details (including the upper level of the system) in order to focus attention on those lower-level systems that must act in sequence in order to bring the higher-level system's CV to reference. I wanted to show how these sequenced activities might lead to a given rate of reinforcement being observed without rate of reinforcement itself being the controlled variable. It was not my intention to describe the complete system.

The whole thing is being driven by a higher-level control system that sets the reference for food-in-belly (System 2), which in turn sets the reference for food-in-the-cup (System 1); the lower-level systems are the means by which this higher-level system controls its CV, which might be conceived as the nutritional state of the organism. I see that Bill has beaten me to the punch, so I defer further description of the model to his reply [Bill Powers (961201.1840 MST)].

One nice thing about this model is that completion of any step along the chain automatically sets the references for the CV's of the earlier systems in the sequence to zero, and behavior just carries on from the current point.

The exact structure of this sequence of lower-level systems remains to be worked out; it could consist of a linear chain of small components, but more likely consists of larger organized elements which themselves can be resolved into sequences. For example, one might propose a series of larger control elements (e.g., "produce pellet," "eat pellet") that each consist of a number of smaller elements ("approach lever," "move lever"), each more or

less like a subroutine in a computer program and each operating through the lower levels of the hierarchy to produce the necessary movements. Other arrangements are possible; discovering the actual organization is a matter for future research. Fortunately, these details do not matter at the levels we are currently concerned with understanding and modeling; even the simple approximation of the lower level I described (which only presses and eats) is sufficient for our purpose of understanding the major fearures of our current data.

Regards,

Bruce

Date: Mon, 2 Dec 1996 18:15:01 +0100
From: "J.A. Blom" <J.A.Blom@ELE.TUE.NL>
Subject: Re: Vancouver's experiment

[Hans Blom, 961202e]

(Bill Powers (961125.0930 MST))

>>On the second to last run the mouse ball got stuck on some dirt on
>>about the last 4s of the run so it did not move.

>That last notation shows why closed-loop control will always work
>better than model-based control when it's available. There's no way
>to predict when the mouse is going to get stuck.

Right. Models can only model predictabilities, relationships that do not vary over time -- or vary predictably. If something unexpected (unpredicted by the model) happens, the model is incorrect. So what to do? Fall back on feedback control? Even feedback control requires certain regularities (a simple model). Update the model? Discovering regularities takes time. So my guess is this: Initially, there is not even a feedback loop. Actions, sure, but with unpredictable outcomes. But the new, different outcome is immediately used to create a new model on which subsequent actions are based: control errors are the basis for learning. There's no way to predict when the mouse is going to get stuck. Yet you soon discover that this is the case: no response, no feedback loop, zero loop gain.

>Also, your sampling with different lengths of eyes-closed time gives
>us an idea of how rapidly the mental model departs from the reality
>without concurrent feedback. The longer the eyes-closed time, from 1
>second to 10 seconds, the greater the error. The error is about
>doubled after 1 second.

In this task situation.

>This says that the internal world-model, if there is one, isn't a
>very accurate model.

Predictions can be accurate only in a fully controlled experiment, i.e. in an unchanging world. If the response of the world is variable, you'd better keep your eyes open. By the way, Bill, what is your impression of the accuracy of your proposed "imagination mode"? Better? Worse? Under which circumstances would it be useful? When

not?

>Hans Blom asked about this before, and now you're also raising the
>question of how the person could continue to produce a spiral (of
>sorts) without present-time visual feedback AND without a
>world-model. Here's a rough idea, which I haven't really developed
>into a full-blown working model but which I am sure would work with
>a little tinkering. This, at least, is a little better than the
>vague solution I mentioned to Hans.

>To make life easier I'll assume that there's a level of kinesthetic
>control at which position can be controlled in angle and radius
>relative to some center, as kinesthetically sensed. ... ...

>This rough sketch shows how a system that contains no explicit
>internal world-model can do some of the things that a model-based
>control system can do -- particularly maintain an ongoing output,
>including curvatures, for some time after loss of input information.
>A different approach to adaptation would be required, of course.

Strange, but I've always though of my "internal world model" to be
the equivalent of your "imagination mode", Bill. Now you offer a
completely different solution. Where is the "imagination" is this
proposal? Or doesn't it play a role here?

Greetings,

Hans

Date: Mon, 2 Dec 1996 18:28:01 +0100
From: "J.A. Blom" <J.A.Blom@ELE.TUE.NL>
Subject: Re: Descriptions, theories, and facts

[Hans Blom, 961202f]

(Bruce Abbott (961125.1450 EST))

>1. A functional approach answers the question, what does it do?
>2. A mechanistic approach answers the question, how does it do it?

Science is concerned with discovering mechanisms. Yet, ultimately
every mechanistic approach reaches a point where nothing remains but
functional answers. Even in physics: we simply don't know what causes
an electron to circle the nucleus of an atom. We only know that it
does. Or do we?

Functional approaches are, in contrast to what some think, nothing to
be ashamed of. Even all our mechanistic models are based on those,
ultimately.

In particular, the "world model" that is the basis of adaptive
control systems is only a collection of functional relationships.
Learning is discovering reliable functional relationships. In
mechanisms, we just observe the functions of the details more
closely.

Greetings,

Hans

Date: Mon, 2 Dec 1996 11:21:57 -0700
From: "T. B. Harms" <tbh@TESSER.COM>
Subject: Re: Transfer functions, utilization.

>From (Tracy Harms (961202.07 MST)

Hans Blom, 961202b

Thank you for your explanation of transfer functions. It was very clear and very useful.

Your other message on transfer functions was very thought-provoking. I have a question. It looks to me like the diagram you provide relates to Figure 5.2 of B:CP in the following way: The box labelled "Comparator" in Figure 5.2 corresponds in your diagram to the combination of TF1, TF2, and the Comparator. Is this correct?

As for message 961202c, where you write "we use the world and all that is in it to achieve our own personal aims", I think it is more in keeping with PCT to say that we *change* the world in the pursuit of our intentions. 'Use' can be taken so broadly, but that runs counter to its main meaning. Use implies -- at least to me -- relying on a thing without consideration of the aims (control systems) of that thing. For things that are aimless this is appropriate. For things which are intentional, such as corn and cattle, complications arise. When regarding other human beings, these complications are overwhelming and the objectification of 'utilizing' breaks down. Communication must supercede it. Indeed, I imagine that such communication predates the materialist notion of utility: Our ancestors were embroiled in communication challenges before sticks and stones became 'tools' to be 'used'.

Tracy Bruce Harms tbh@tesser.com
Boulder, Colorado

"The experiences of marginality and alienation are virtually integral to the experience of a free individual--the individual free to dissolve as well as to create attachments."
William Warren Bartley, III

Date: Mon, 2 Dec 1996 08:05:07 -0800
From: Richard Marken <marken@AEROSPACE.AERO.ORG>
Subject: Re: Born to Rebel

[From Rick Marken (120296.0800)]

Dan Miller (961201) --

>Sulloway argues that first borns tend to favor a conforming, status quo, >orientation, whereas later borns...tend to be more creative, rebellious, >unconventional

Bruce Abbott (961201.1855 EST)

>In that case, as a small test of this proposition, I'd like to know where
>Bill Powers falls along the birth-order scale. Bill?

Where do you think I fall -- first, later or only?

I'm thinking first born for you, Bruce;-)

Best

Rick

Date: Mon, 2 Dec 1996 13:46:05 -0600
From: Bruce Abbott <abbott@CVAX.IPFW.INDIANA.EDU>
Subject: Re: Born to Rebel

[From Bruce Abbott (961202.1445 EST)]

>Rick Marken (120296.0800) --

>Bruce Abbott (961201.1855 EST)

>>In that case, as a small test of this proposition, I'd like to know where
>>Bill Powers falls along the birth-order scale. Bill?

>Where do you think I fall -- first, later or only?

Definitely the baby of the family ;->

>I'm thinking first born for you, Bruce;-)

Right; I have a younger sister and an even younger brother.

I wonder whether Sulliway noted the _dis_advantages of first-borns in addition to what he perceives as their advantages. These include being displaced by one's younger siblings, being expected to behave older than your age (e.g., a toddler old being told to stop "acting like a baby"), having inexperienced parents (you get to be the one they learn on), having no older sibs to serve as models or to keep you informed about what to expect next in your life, and being perceived as the bully anytime a conflict erupts between you and your sibs, to name a few. Bringing these into the picture muddies the clear relationship he perceives between birth order and personality traits a bit, don't you think?

>Bill Powers (961201.1830 MST) --

>I'm the first-born, with one younger sister.

Well, Rick, Bill and I may be more similar than you think. (Scarey thought, huh?) We're both sons of engineers, too.

Regards,

Bruce

Date: Mon, 2 Dec 1996 12:00:03 -0800
From: Rick Marken <marken@LEONARDO.NET>
Subject: Chutzpah, thy name is Hans

[From Rick Marken (961202.1200)]

Hans Blom (961202d); Hans Blom (961202e) --

Nice work, Hans. I really have to admire a guy who won't let data interfere with his lectures on how organisms control.

Best

Rick

Date: Mon, 2 Dec 1996 14:55:29 -0500
From: Scott Stirling <scstirli@ANSELM.EDU>
Subject: Jumping Spiders

Dear All,

Did any of you see the article on jumping spiders in last month's _National Geographic_? There was a genus being studied that tricked other kinds of spiders into vulnerable positions. Once there, the tricky spider would eat the curious one. The process of trickery involved tapping on other spiders' webs and lairs to arouse desired responses (or perceptions, if PCT is applied here). Apparently, the spiders could mimick the tappings of a mate or struggling prey.

I heard of PCT after reading this article. The process that these spiders use to achieve mimicry is not understood. And it seems to be open-ended, in that they can adapt their trickery to virtually any kind of spider. This sounds like a text book case of PCT to me.

Anyone have any thoughts on this example?

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Date: Mon, 2 Dec 1996 14:12:49 -0600
From: Bruce Abbott <abbott@CVAX.IPFW.INDIANA.EDU>
Subject: Re: Functional relationships

[From Bruce Abbott (961202.1510 EST)]

>Bill Powers (961201.1930 MST) --

>>Bruce Abbott (961131.1440 EST)

>The purely functional (descriptive) approach is _not_ that C (the _rate_ of
>contingent event C) drives the rate of behavior B, but rather, that the
>presence/absence of a contingency between the individual behavioral event b
>and event c [from which can be derived the function C = f(B)] is positively
>related to B'.

Fine, you CAN describe this relationship without using terms like "support"
or "maintain." If you're saying only that the behavior changes when the
dependency of C on B changes, that's acceptable to me. We would expect the
behavior of a system to change when the characteristics of one of its
components changes.

Good. But "support" simply means "to hold something up." One can say,
based on these observations, that the contingency is supporting the behavior
(the rate of the behavior is higher with the contingency than without it),
without saying anything about _how_.

Why would I want to speak this way? I don't. I'm simply trying to show
that when others _do_ speak this way, that they are not necessarily doing
anything more than stating an observed relationship.

>>Another way to put it, I suppose, would be to state that,
>>under the conditions under which these observations hold, B = f(k1).

>Yes, but this doesn't tell us what k1 is a function of, so B remains
>undefined. I proposed that k1 is nonzero when the contingency is in effect,
>but that doesn't explain anything; the form of a function can't have any
>physical effects. To complete the model we would have to make a proposal
>about what variables that the organism could sense are affected in a
>recognizeable way when the contingency is present or absent. Perhaps the
>error signal in the control system would be a sufficient indicator. A large
>rise in the error signal would say that pressing the bar is no longer
>maintaining the input at its former level (and here I can use "maintaining"
>because we are talking about a causal relationships).

Another approach would be to manipulate other variables (e.g., deprivation
level) and observe how they and k1 are related. This would simply expand
the functional relationship to include these other factors, without
proposing any mechanism through which these variables might come to be
related in the ways observed. This would amount to an expanded description
of the conditions affecting the relationship between the contingency and the
asymptotic rate of behavior, B'. A model identifying the internal variables
and their connections would provide the mechanism through which these
empirical relationships emerge.

Clearly, having such a model is superior to having only these emperically
derived descriptions. I have not been arguing otherwise. Yet these
empirical relationships are precisely what the model must explain if it is
to be deemed successful.

Regards,

Bruce

Date: Mon, 2 Dec 1996 16:29:37 EST
From: Bruce Gregory <gregory@CF4.HARVARD.EDU>

Subject: Re: Born to Rebel

[From Bruce Gregory (961202.1630 EST)]

Bruce Abbott (961202.1445 EST)

> I wonder whether Sulliway noted the _dis_advantages of first-borns in
> addition to what he perceives as their advantages.

Sulloway focuses on the competition of siblings for parental
resources. His analysis includes interactions of several kinds
(including degree of introversion). Nothing you said is
inconsistent with that analysis.

Bruce Gregory
(First of three)

"You can pick your friends, and you can pick your nose. But you
can't pick your friend's nose." Anonymous

Date: Mon, 2 Dec 1996 16:35:47 -0500
From: Jeff Vancouver <jeffv@PSYCH.NYU.EDU>
Subject: Re: Vancouver's experiment

[from Jeff Vancouver 961202.16:15 EST]

> [From Bill Powers (961125.0930 MST)]
>
> Jeff Vancouver 961125.09:55 EST --
>
> >Below are the data from several different type of runs.
>
> Very nice, Jeff. I appreciate simple direct solutions to problems. Blank the
> screen? Close the eyes!

Thank you. This follows years of training and teaching research methods.

I have included for comparison purposes.

>
> jeff
> > 6.31 visible
> > 6.21 visible
> > 6.01 visible
> >117.72 Alt 10s
> > 6.44 visible
> >115.10 Alt 10s
> > 28.38 Alt 2s
> > 36.67 Alt 2s
> > 32.29 Alt 2s
> > 29.16 Alt 2s
> > 11.66 3,1,3
> > 10.41 3,1,3
> > 11.87 3,1,3
> > 11.38 3,1,3
> > 32.79 3,1,3 Mouse got stuck

> > 12.19 3,1,3
> >
> >On the second to last run the mouse ball got stuck on some dirt on about
> >the last 4s of the run so it did not move.
>
> That last notation shows why closed-loop control will always work better
> than model-based control when it's available. There's no way to predict when
> the mouse is going to get stuck. If your eyes had been open when that
> happened, there still would have been more error, but it would have been a
> much smaller increase and it wouldn't have lasted four seconds..
>

Agree

> >It is perhaps interesting to note that I did substantially better in the
> >Alt 2s then the Alt 10s even though my eyes were closed half the time in
> >Alt 2s and only a third in Alt 10s, ...
>
> Um, Jeff ... equal time open and closed is closed half the time, isn't it?
> Regardless of the cycle time?

Recall that a run lasts 30s, I begin with eyes open. Think about it.

>
> >the ability to correct more often once
> >my eyes were opened accounts for the better performance (given that when I
> >opened my eyes I was always making a substantial correction -- esp. in
> >the Alt 10s condition).
>
> Also, your sampling with different lengths of eyes-closed time gives us an
> idea of how rapidly the mental model departs from the reality without
> concurrent feedback. The longer the eyes-closed time, from 1 second to 10
> seconds, the greater the error. The error is about doubled after 1 second.
> This says that the internal world-model, if there is one, isn't a very
> accurate model. You might try repeating this experiment about 50 times and
> seeing if your eyes-closed performance improves substantially. I haven't
> improved much over about 100 trials.

Below are the data from several more trials. This time alternating 2s
open, 2s closed.

Jeff2
  6.63 visible (eyes open)
 21.85 visible (2s alt for rest)
 23.26 visible
 25.59 visible
 17.10 visible
 19.81 visible
 15.18 visible
 19.98 visible
 21.63 visible
 17.81 visible
 21.16 visible
 23.54 visible
 17.42 visible
 17.18 visible
 31.74 visible
 28.25 visible
 17.15 visible

I did not get a lot better and I don't have the patience for 100 trials.
I did improve slightly, just does beg the question: what is improving?
Precision?

I must say, the task is much more difficulty than I thought it would be
(as Hans 961292d observed).

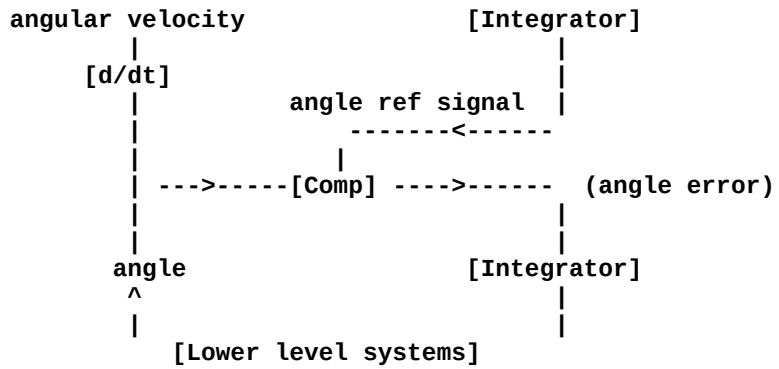
> >The point I was trying to make is that control with the mouse, screen, or
> >whatever blanked (i.e., model-based control), is better than random,
> >static (no movement), or linear trajectory performance. Thus, as a
> >comparison, the average RMS from those models needs to be added to the
> >output.
>
> I think you're saying that we need a baseline against which to compare the
> performance. I agree; that would be a fairer comparison. However, it still
> doesn't answer the basic question of how control is carried out with the
> eyes closed (if "control" is the word).

To get some comparison data I did a few other types of runs.

288.06 target in corner throughout run
105.51 target in center throughout run
 87.43 target frozen for 2s, tracking for 2s
 82.24 target frozen for 2s, tracking for 2s
107.37 target with linear trajectory for 2s, tracking for 2s
 91.83 target moving "randomly" for 2s, tracking for 2s
122.65 target moving "randomly" for 2s, tracking for 2s

Clearly my attempt to follow the target with my eyes closed was much
better than any of the comparison conditions. The trajectory and random
of course are subject to many parameters that will change those numbers,
but I suspect we do not need to get into that. The rest of your post to
me was very interestings.

>
> Hans Blom asked about this before, and now you're also raising the question
> of how the person could continue to produce a spiral (of sorts) without
> present-time visual feedback AND without a world-model. Here's a rough idea,
> which I haven't really developed into a full-blown working model but which I
> am sure would work with a little tinkering. This, at least, is a little
> better than the vague solution I mentioned to Hans.
>
> To make life easier I'll assume that there's a level of kinesthetic control
> at which position can be controlled in angle and radius relative to some
> center, as kinesthetically sensed. Any other coordinate system could be
> assumed, but this one is easiest. I'll just show the kinesthetic angle
> control system.
>
>   from integrating output function
>   in visual system
>
> |
>   velocity ref
> |
> |
>   ------->----[Comp] --->----- (velocity error)
> | |
> | |



> Note that to move the mouse at a constant angular rate, all you need is a
> _constant_ velocity reference signal. The integrator in the upper system
> would turn a constant (small) error signal into a _constantly changing_
> angle reference signal. The derivative (d/dt) in the input function cancels
> the lag in the integrating output function, so the overall response is
> proportional, as far as stability is concerned. The lower system makes the
> perceived angle follow the changing reference signal. Passing this constant
> rate of change through the first derivative in the upper system yields a
> perceptual signal of angular velocity proportional to the rate of change of
> angle.
>
> Suppose now that there is a visual system that is detecting position error,
> and converting it, through an integrator, into the reference angular
> velocity for this kinesthetic control system. If the visual system
> experiences a blackout, the proper response is to clamp its error signal to
> zero (the model-based control system also needs a special response to loss
> of input). The assumed integrator in the output of the visual system
> receives a zero error-signal input, so its output remains fixed. This means
> that the angular velocity reference signal for the kinesthetic system also
> becomes constant, and the kinesthetic system continues to move the mouse (as
> kinethetically detected) at a constant angular velocity. When the visual
> input returns, the angular velocity reference signal will be increased or
> decreased as necessary to correct the visual error.
>

Wow, I wish I had a degree in engineering. If I understand this model (which I am sure I do not), it seems that you are saying that a control system can perform the tracking task without data _or_ without setting itself up so that it can operate without data. Control systems just set themselves up that way.

This does seem to be different from your imagination mode, which Hans (961202d) and I conceived of as the model mode (although, not necessarily with any firing of output functions).

Bottom line, we must come to some mutual understanding of how to interpret the data. Your "model" suggests that my comparison data (e.g., frozen cursor) is of no value.

I have another experiment in mind, but before we discard this one is there any response to my data?

Jeff

Date: Mon, 2 Dec 1996 16:26:34 -0500
From: MILLER dan <millerd@SABER.UDAYTON.EDU>
Subject: Re: Born to Rebel

[Dan Miller (961202)]

To Bruce Gregory, Rick Marken, and the rest of you:

OK, so Bill Powers is a first born. This fact not only disconfirms Frank Sulloway's hypothesis, it confirms the first law of sociology - namely, "Some do, some don't."

The use of descriptive or inferential statistics is problematic for the social sciences. First, the model of behavior is most likely some form of the behaviorist model - behavior is caused, released, influenced, and so on. Then, there is the law stated above. Let me flesh it out a bit.

For example, in the sociological study of crime, there is a bias so widely shared and seldom criticized that to point it out is seen as a breach of faith. It is the simple notion that low income (or poverty) causes (influences) crime. No amount of factual information disconfirming this hypothesis will detract the true believers - who are legion. To them, crime is materially based. The havenots are forced into a life of crime (and, I suppose) ruin.

The evidence looks more like this. (Excuse me for not getting the actual statistical facts.)

1. Many of those people who share the pertinent qualities do not commit crimes. In fact, most do not.
2. Many of those people who do not share the pertinent qualities (falling outside the low income category) do commit crimes.
3. Some who have the pertinent qualities commit the crime once (or twice) and never do it again.

Only a few actually share the pertinent qualities and commit crime. The variance "explained" on the dependent variable (crime) is always very low (10 - 20%).

So, why do they keep it up? As I noted above, Faith may be the case. Maintaining a reference signal no matter what the perceptual input. Also, this type of research works. It gets published, gets people tenure, promotion, and approval from their fellow travellers. The fact that neither science nor understanding have been served may be irrelevant.

As for Frank Sulloway's book, he falls into the trap of doing this type of research - or at least falling back on such forms of statistical reasoning, and, thus, leaving himself open for the instance that casts great doubt. Once again, Bill Powers has totally messed-up the good intentions of mainstream social scientists. Sulloway's book is better than I have suggested in this post. Still, he can be easily dismissed. Too bad, there might be a decent idea worth addressing.

BTW, Bill Powers falls under number two (above). He does not have

the pertinent qualities (being a firstborn), yet he fulfills the necessary requirements for Sulloway's dependent variable - he is a scientific revolutionary. Perhaps there was something about his relationship with his mother ...

Later,
Dan Miller
miller@riker.stjoe.udayton.edu

Date: Mon, 2 Dec 1996 15:10:41 -0800
From: Rick Marken <marken@LEONARDO.NET>
Subject: He's a rebel and he'll never ever be any good

[From Rick Marken (961202.1510)]

Me:

>Where do you think I fall -- first, later or only?

Bruce Abbott (961202.1445 EST) --

>Definitely the baby of the family ;->

I'm first of two, actually. I have a younger (and far more rebellious) brother. Maybe birth order just determines rebelliousness relative to other people in your family;-)

I think this guy Sulloway's main problem isn't his statistics (though they are bad enough). His main problem is that he just doesn't know what behavior _is_. He seems to think that behavior is what one person sees another person doing (like "being rebellious"). In fact, behavior is the perceptions that a person controls. Some of what Sulloway sees as "rebellious" behavior may be a side effect of a person's efforts to control one kind of perception (like principles and system concepts) and some may be a side effect of a person's efforts to control other kinds of perceptions (like food and water). Sulloway's mistake is that he is trying to determine why people cause _his_ perceptions (of rebelliousness, for example) rather than trying to determine what perceptions people control _for themselves_ -- and why they control those perceptions.

Best

Rick

Date: Mon, 2 Dec 1996 20:26:36 -0500
From: Gary Cziko <g-cziko@UIUC.EDU>
Subject: Searchable Archives for CSGnet

[from Gary Cziko 962304.0120 GMT]

I recently received the following notice about a searchable archive for CSGnet.

I checked it out and it is pretty nifty. The catch is that you will also get some advertising as you search, but since you don't have to look at it,

it doesn't seem too much of a price to pay.--Gary

Date: Mon, 2 Dec 1996 19:25:10 -0700
From: "T. B. Harms" <tbh@TESSER.COM>
Subject: Re: He's a rebel and he'll never ever be any good

>From Tracy Harms 1996;12,02.19

Rick Marken (961202.1510)

>[...] Some of what Sulloway sees as
>"rebellious" behavior may be a side effect of a person's efforts to
>control one kind of perception (like principles and system concepts)
>and some may be a side effect of a person's efforts to control other
>kinds of perceptions (like food and water).

So, if I follow this, a PCT-informed analysis would sort observable tendencies in other's actions (what most people call behavior) into a set of intentions (i.e. reference levels). My initial guess for such an undertaking is that it would involve at least three major categories: "deep" goals (organic heritage such as warmth and food), "high" goals (cultural heritage such as principles and standards), and conflicting supersystems.

The label "rebellious" is weak because it describes a convention which occurs among observers, and invites the assumption that the single label corresponds to a particular thing. In fact, however, any variety of factors within an actor may provoke the use of this label by others. PCT directs us to look for consequential patterns (such as reference levels) within the actors, and whether these map reliably to public labelling cannot be presumed in advance. Thus a thesis such as Sulloway's is crippled from the start because he takes a public label as his touchstone, then goes on to examine personal experience and motivation presuming that the systemics interior to personalities reflect these public labels. But PCT suggests that the presumption must be that they do *not* match; at the very least, allegation of a match must be argued and defended.

What I've written above is, I think, nothing which Rick Marken did not already say. I'm just writing this out as an excercize to see whether I can state this same idea in my own words, and to get corrective criticism insofar as I have missed the boat.

Overall, I think the lesson here is that the words which are used to categorize people are no help in sorting them by *behavior*, for these categories usually sort by external effect and impression rather than by personal intention. This may even suggest one insight into why people are

uniformly annoyed by the labels which pidgeon-hole them, no matter how accurate those may be: for the labels identify things which are not important to the actors, but instead provide a sort of public utility.

I'm crawling out on a limb, there, but I have to test my understanding of PCT somehow.

Tracy Bruce Harms tbh@tesser.com
Boulder, Colorado

"It is not the noisy revolutions of politics, but the silent revolutions of skill that change the course of man's destiny."
Kenneth Boulding

Date: Mon, 2 Dec 1996 22:39:54 -0500
From: "John E. Anderson" <jander@OSPREY.UNF.EDU>
Subject: Re: Searchable Archives for CSGnet

[From John E. Anderson (961202.2240 EST)]

> [from Gary Cziko 962304.0120 GMT]
>
> I recently received the following notice about a searchable archive for CSGnet.
>
> I checked it out and it is pretty nifty. The catch is that you will also
> get some advertising as you search, but since you don't have to look at it,
> it doesn't seem too much of a price to pay.--Gary
>
> ---
> - Reference.COM has begun archiving this list as of:
> Nov. 26, 1996
>
> - Searchable archives for the lists are available at:
>
> http://www.reference.com/cgi-bin/pn/listarch?list=CSGNET@postoffice.cso.uiuc.edu

Wow! It is definitely cool. Unfortunately it doesn't go back very far, only to last week. So I guess there's no way to search the archives before then?

John
--
John E. Anderson
jander@unf.edu

Date: Tue, 3 Dec 1996 04:36:25 -0500
From: Bruce Gregory <bgregory@CFA.HARVARD.EDU>
Subject: Re: He's a rebel and he'll never ever be any good

[From Bruce Gregory (961203.0430 EST)]

Tracy Harms 1996;12,02.19

Rick Marken (961202.1510)

I'm at a definite disadvantage in this exchange, since I am actually reading what Sulloway says... I can see that having no knowledge of the man's work provides a freedom that I can only envy. I can only fall back on Cromwell's plea to the Irish bishops, "I beseech you, in the bowels of Christ, to think it possible that you might be wrong."

I suspect that those who reject PCT have done so on the basis of an equally careful reading ;-)

Bruce Gregory

Date: Tue, 3 Dec 1996 06:58:10 -0500
From: Nickols@AOL.COM
Subject: Re: Stat Problem

[Fred Nickols (961202.1900 EST)]

> In response to [Bill Powers (961126.1200 MST)], who wrote in part:
>
> >I have a problem that people on CSGnet might be able to
> >help me solve. How do we objectively evaluate the goodness
> >of fit between a model's behavior and real behavior?

Bill:

I'm no statistician (actually, I'm mathematically challenged), but I do hang around with a bunch of the world's best here at ETS. So, I passed along to some researchers here at ETS the entire message in which the snippet aboveappeared. That elicted the following responses. I hope you find them helpful ... but you'll have to be the judge of that as I am in no way competent to make that evaluation.

Regards,

Fred Nickols
nickols@aol.com

<Begin Response #1, from Bruce Kaplan>

[T]he standard way of comparing models is to look at the residuals. So first calculate the residuals (observed - predicted), the mean square(the sum of the squared residuals divided by the degrees of freedom) error tells how much variance is not explained. That is a good way of comparing which of two models fit better. There are various F-tests that can be computed. Also it would be a good idea to look at the following:

1) plot residuals against the observed dependent variable.
    See if there is still some pattern left in the data. Also
    plot the residuals against each of the independent
    variables, it might tell if a squared term will fit better.

2) Also usually the researcher is concerned about fitting

a model for a whole population of something (e.g. rats),
not just the ones in his/her sample. Cross-validation,
then can be an important tool. Either do more observations,
and see how well the previous fit model fits to the new data.
Or Initially randomly split your observations into 3/4 of the
data to run the initial model on and 1/4 to see how it fits.
You could even do this repeatedly to get a better model.

<Begin Response #2, from Don Rock>

I completely agree with [Bruce's] suggestions -- a slight variation
is doing a bootstrap on the cross-validation sample. In this case
the model(s) parameters from the validation sample are considered
fixed and the variability (stability) of the estimates can be evaluated
using the bootstrap on the cross-validation sample. This does
assume a reasonable sample size on the cross-validation sample.

Date: Tue, 3 Dec 1996 14:43:09 +0100
From: "J.A. Blom" <J.A.Blom@ELE.TUE.NL>
Subject: Re: simultaneity / modelling

[Hans Blom, 961203]

(011296 0915 PST from David Wolsk)

>The image which brings me to this current discussion is of my
>switching on a light and hearing a strange noise at the same time.
>My experience is of an immediate rise in my level of alertness and
>a need to "process" that noise.

The notion "strange" can be "explained" from the point of view of
model-based control, where the model "predicts" which perceptions we
will experience in the immediate future. That is, when the model is
correct and complete. But it never is. Things are "surprising" or
"strange" when our perceptions don't match our expectations. This
implies that the model is incorrect and needs to be adjusted. Our
highest priority seems to be to have a model that is accurate and is
in tune with our perceptions. If it is not, we start a search for
what causes the "strangeness" in order to adjust the model and
include the new thing into our mental paradigms. This can be done in
a large variety of ways, of course, dependent upon what we (think we)
know already. Anyway, the end effect will be that our "explanation"
now is part of the new model, and that the surprise is surprising no
more.

This is everyday science: to reduce "miracles" to "understood"
relationships.

By the way: In an adaptive controller "surprise" can be implemented
by an "alertness mechanism" which tests whether a new observation is
in- or outside the two or three standard deviation boundaries of the
(internally available) prediction error estimate ("my estimate of how
certain I am of my knowledge"). In case of a "surprise", the adaptive
controller can be forced to discard the old knowledge -- which has
been demonstrated to be incorrect because it did not correctly
anticipate the perceptions -- and to restart learning from scratch.
But that is a bit drastic and recommended only when the "surprise" is

huge -- and completely unfeasible in humans. A more gradual approach continually enlarges the internal prediction error estimate variance ("I am less certain of what I know than I thought") in order to keep the subjective prediction error uncertainty in line with the actual prediction errors.

A different, but far less successful, approach is not to process outliers at all, to assume that you did not really perceive them or that you cannot trust your perceptions ("I could not believe my eyes!"), and to keep the model as it is -- but at the risk of maintaining illusions and growing ever more apart from reality.

My two cents...

Greetings,

Hans

End of CSGNET Digest - 2 Dec 1996 to 3 Dec 1996
****

Date: Wed, 4 Dec 1996 08:00:03 -0600
Subject: CSGNET Digest - 3 Dec 1996 to 4 Dec 1996

There are 18 messages totalling 1043 lines in this issue.

Topics of the day:

1. simultaneity
2. Bill's model
3. Transfer functions, utilization
4. Chutzpah, thy name is Hans
5. Born to Rebel (4)
6. Vancouver's experiment (4)
7. Rebels with chutzpah (3)
8. Learning, Control of others (3)

Date: Tue, 3 Dec 1996 15:03:02 +0100
From: "J.A. Blom" <J.A.Blom@ELE.TUE.NL>
Subject: Re: simultaneity

[Hans Blom, 961203c]

(Bill Powers (961201.1100 MST))

>Flipping a light switch is not supposed to be followed instantly by
>a loud bang. In fact, it is supposed NOT to be followed by anything
>but a light turning on or off.

I would call that a mental model: in my experience, flipping a light switch does not cause anything but turn a light on or off (this may not be true for some electrical engineering students).

>I can confirm all of this from my own experience. But I don't know
>how the same temporal relationship would seem to a rat.

The temporal relationships that we incorporate into our internal model arise not only when events occur simultaneously or nearly so. I remember reading about studies where rats reliably established the source of food poisoning, although the unpleasant perceptions of eating poison may take hours to become pronounced. The survival value of such a mechanism is obvious.

How does such a mechanism work? Something like this, I guess. Some internal structure can accommodate knowledge of the type "specific unpleasant perceptions [which we humans call being caused by food poisoning] must have been caused by eating a new food stuff X at approximately two hours before the perceptions become noticeable". After identification/instantiation of X, the resulting behavior will be to avoid eating that X and to communicate the badness of X to the other members of the nest. I leave it up to you to rephrase this into PCT terminology...

Greetings,

Hans

Date: Tue, 3 Dec 1996 15:14:15 +0100
From: "J.A. Blom" <J.A.Blom@ELE.TUE.NL>
Subject: Re: Bill's model

[Hans Blom, 961203c]

(Tracy Harms (1996;12,01.17:48))

>... if System 1 were *disturbed* by the removal of food from the
>environment, the rat would act to perceive food in its environment
>*even if it were not hungry*. I don't know as much about rats as
>you guys do, but that's too far-fetched for me.

Admittedly, Bruce's model is too simple. Yet, Bruce's mechanism is _almost_ right. Wild rats (and all other higher animals) go to great lengths to ensure the _possible_ (imaginary? anticipated?) perception of food in their environment even if they are not hungry; they control for easy access to food. Hence migration to a new environment when the food supply at the old spot gets exhausted, storage of food for harsher times, etc. Thus, removal of food from the environment _does_ cause a desire to perceive new food, though less directly than in Bruce's model.

By the way, I like your quotes a lot.

Greetings,

Hans

Date: Tue, 3 Dec 1996 15:38:45 +0100
From: "J.A. Blom" <J.A.Blom@ELE.TUE.NL>
Subject: Re: Transfer functions, utilization

[Hans Blom, 961203d]

(Tracy Harms (961202.07 MST))

>Thank you for your explanation of transfer functions. It was very
>clear and very useful.

You're welcome.

>Your other message on transfer functions was very thought-provoking.
>I have a question. It looks to me like the diagram you provide
>relates to Figure 5.2 of B:CP in the following way: The box
>labelled "Comparator" in Figure 5.2 corresponds in your diagram to
>the combination of TF1, TF2, and the Comparator. Is this correct?

Yes. I just added some detail, and I don't quite understand what
makes it so thought-provoking. The "comparator" in my diagram is
simply a subtractor; this clarifies the positions of the transfer
functions, I hope. The addition of TF1 changes little in Bill's basic
story, which remains fully intact.

>As for message 961202c, where you write "we use the world and all
>that is in it to achieve our own personal aims", I think it is more
>in keeping with PCT to say that we *change* the world in the pursuit
>of our intentions.

When we change (or use) the world, we also change the world of
others. Thus we cannot help but change the behavior of others who
inhabit that same world. In some cases, that changed behavior will be
helpful for us. In other cases not. In some cases, _our_ changed
behavior will be helpful for others. In other cases not. And it seems
as if we have a choice...

>'Use' can be taken so broadly, but that runs counter to its main
>meaning. Use implies -- at least to me -- relying on a thing without
>consideration of the aims (control systems) of that thing.

Oh. Word usage problems. I took the perspective of considering
everything outside of me as a "tool" to be used to achieve my goals.
I'm fully aware that others do the same. But also that some tasks are
more easily achieved -- or can only be achieved -- if we cooperate,
to the advantage of all concerned.

>Communication must supercede it [utilization]. Indeed, I imagine
>that such communication predates the materialist notion of utility:
>Our ancestors were embroiled in communication challenges before
>sticks and stones became 'tools' to be 'used'.

Yes. The world of a social animal consists, to a large extent, of
other animals. Thus, others are our prime tools. Think this through,
however you may object to my use of language...

Greetings,

Hans

Date: Tue, 3 Dec 1996 15:45:21 +0100

From: "J.A. Blom" <J.A.Blom@ELE.TUE.NL>
Subject: Re: Chutzpah, thy name is Hans

[Hans Blom, 961203d]

(Rick Marken (961202.1200))

>Nice work, Hans. I really have to admire a guy who won't let data
>interfere with his lectures on how organisms control.

There is a saying amongst control engineers: "One man's noise is
another man's data." You get an equivalent when you turn it around:
"One man's data is another man's noise."

In world-model terms: You live in a world very different from mine.

By the way, what is chutzpah? Sounds nice, though...

Greetings,

Hans

Date: Tue, 3 Dec 1996 15:49:46 +0100
From: "J.A. Blom" <J.A.Blom@ELE.TUE.NL>
Subject: Re: Born to Rebel

[Hans Blom, 961203e]

(Dan Miller (961202))

>For example, in the sociological study of crime ...

Huh? Crime is a sociological notion? I thought it was a legal one.
One society's crime may be another society's virtue. Abolish crime:
abolish all laws!

:-) :-) ;-)

Greetings,

Hans

Date: Tue, 3 Dec 1996 10:49:55 -0500
From: Jeff Vancouver <jeffv@PSYCH.NYU.EDU>
Subject: Re: Vancouver's experiment

[from Jeff Vancouver 961203.0940 EST]

> [Hans Blom, 961202e]
>
> (Bill Powers (961125.0930 MST))
>
> >>On the second to last run the mouse ball got stuck on some dirt on
> >>about the last 4s of the run so it did not move.
>

> >That last notation shows why closed-loop control will always work
> >better than model-based control when it's available. There's no way
> >to predict when the mouse is going to get stuck.
>
> Right. Models can only model predictabilities, relationships that do
> not vary over time -- or vary predictably. If something unexpected
> (unpredicted by the model) happens, the model is incorrect. So what
> to do? Fall back on feedback control? Even feedback control requires
> certain regularities (a simple model).

I am not sure I agree with the "simple model" part. I would agree that actions must have some impact on the environmental variable with some regularity, but neither the environment nor the array of control systems need be simple.

> Predictions can be accurate only in a fully controlled experiment,
> i.e. in an unchanging world. If the response of the world is
> variable, you'd better keep your eyes open. By the way, Bill, what is
> your impression of the accuracy of your proposed "imagination mode"?
> Better? Worse? Under which circumstances would it be useful? When
> not?
>
> Strange, but I've always though of my "internal world model" to be
> the equivalent of your "imagination mode", Bill. Now you offer a
> completely different solution. Where is the "imagination" is this
> proposal? Or doesn't it play a role here?
>

For the most part, this is what I am thinking as well. The difference is that the imagination mode involves outputs triggering memories (but not actions). This does not account for my prediction that the sun will rise every morning despite absolutely no action on my part. But that is not the issue.

The issue as I see it is to reconcile the theories. It seems that the options are:

1) closed-loop control only
2) model-based control only
3) some combination of 1 & 2
4) neither

Lets eliminate 4. Science is about finding better models/theories. Without a contestant, 4 is not an option.

Now it appears that Rick and sometimes Bill P. advocate 1. Yet, Bill P. often acknowledges that model-based control sometimes occurs. Hans and I appear to believe he has even included it in PCT via imagination mode. Nonetheless, it appears that we have not developed an experiment or observation set that refutes 1 to the satisfaction of the players. More later on how this could be.

I am not sure if anyone believes 2, although Bill and Rick keep telling me that is what the motor control people and Hans believe. I keep seeing closed loop control in the work I read. Clearly, everyone (of us) agrees that on-line data improves performance. Thus, the data (as Rick so delicately points out) refute 2.

That leaves us with 3. THe question then becomes "what is the nature of

the combination?" I think that the answer lies in what one calls a model. I think of a model as a mechanism for making predictions of future or current states. For me an example of such a model is a regression equation. A regression equation can be used to predict future performance based on current inputs like test scores or past performance. Regression equations are simply transfer functions. Unfortunately, regression equations tend to be linear and sensitive to missing data. Those are some reasons I prefer the neural network model. But the basic idea is the same. Inputs are weighted and combined to create outputs (let us focus on outputs from input transfer functions, i.e., perceptions). If a neural net model is used, some of those inputs to the input function can be missing.

The inputs can be either from the environment or internally generated (redirected from output functions as in imagination mode). In either case, an output results in a "perception." The accuracy of that perception is likely to be much greater if the inputs are from the environment rather than internally generated or generated with missing data.

This picture, as it turns out, can be described as either 1 _or_ 2! It is 1 if one focuses on the notion that the output of the input function is a perception with is controlled, regardless of the source of the inputs into the input function. The problem with this interpretation is that we are trying, I believe, to distinguish between inputs directly from the environment v. ones where at least some of that input cannot be directly from the environment because it has been cut off. Thus, it is not the output of the input function and subsequent attempts at control, but the source of the input into the input function that is the issue.

One can also argue that what I describe is 2. That is, the input transfer function is a model. Again, regardless of the source of the input into that input transfer function, the nature of the function (i.e., model) determines perceptions, which as it happens, is part of control. Thus, like the previous paragraph the source of the input is ignored so that the theory can be called model-based control. And like the previous paragraph I would argue that that is missing the point.

Thus, 3 _is_ 1 and 2.

Now Bill P.s post to explain the Vancouver Experiment results seems to have added a wrinkle to my speculation. Specifically, if I understand it, he is incorporating the ability of the control system to response to missing data via more than just the nature of the input transfer function, but in the nature of the entire set of control systems and all their transfer functions. Intriguing. I am wondering how many possible configuration of control systems can be set to accomplish the spiral tracking task? Then, how many would be able to continue spiralling without direct input from the environment? THen, why would the control system set itself in a way that allows the able to operate without input? Finally, does this "choice" constitute a model for prediction?

THus, Bill P. is suggesting that the model is not simply in the input function, but in the entire configuration. If it is in the entire configuration and that is the only reasonable configuration for on-line control, than perhaps the concept of model-based control is trivial. If, on the other hand, the configuration has be adapted to deal with missing data directly from the environment, that would make it a model. The difference between this model and the one I proposed is that the output is

actions, not the perceptions. That is a big difference as well, hence the word "intriguing."

I personally find it hard to believe that the only reasonable configuration of control systems would be able to handle non-direct environmental input for all tasks in which we do that. Further, I find it hard to believe that the input transfer function type of model does not exist. My ability to imagine (perceive) the sun rising in the morning, to dream, etc. suggests that perceptions can be created without direct input.

So where do we disagree? Can we devise an experiment to resolve the disagreement? Answer the first question and I will think about the second.

Later

Jeff

Date: Tue, 3 Dec 1996 08:39:19 -0800
From: Richard Marken <marken@AEROSPACE.AERO.ORG>
Subject: Rebels with chutzpah

[From Rick Marken (961203.0830)]

Me:

> Some of what Sulloway sees as "rebellious" behavior may be a side effect of
>a person's efforts to control one kind of perception...

Tracy Harms (1996;12,02.19) --

>So, if I follow this, a PCT-informed analysis would sort observable
>tendencies in other's actions (what most people call behavior) into a set
>of intentions (i.e. reference levels).

Very close. A PCT informed analysis tries to see _past_ observable behavior to the perceptions being controlled by the behaving system. PCT tries to see behavior _from the perspective of the behaving system_. This is done by testing to determine what perceptual variables the system is controlling. What I see as "rebellious behavior", for example, may be actions that are defending a controlled perception from disturbance. The same person, controlling the _same_ variable in a different environnment (different disturbances) might not appear "rebellious" at all. So a person can be doing the same thing (controlling the same perception) yet appear to be doing different things from the point of view of an observer. Conversely, a person can doing different things(controlling different perceptions) at different times yet appear to be doing the same thing (being "rebellious") becuase the actions used to protect these different perceptions from disturbance happen to be similar . PCT takes us beyond the superficial _appearances_ of behavior to what people are actually _doing_ when they behave -- to the perceptions they are controlling.

>The label "rebellious" is weak because it describes a convention which
>occurs among observers, and invites the assumption that the single label
>corresponds to a particular thing.

I'd say that the label "rebellious" is weak because it describes behavior from the point of view of the observer rather than from the point of view of

the behaving system. For example, I might appear "rebellious" to you because I consistently trash posts from Hans Blom. But I may just be controlling for perceiving PCT relevant information on CSGNet. If I were actually controlling for being rebellious, I would trash Hans' posts no matter what in order to rebel against him. But you could _test_ to see if I were actually controlling for "rebelliousness" by having Hans post something relevant to PCT occasionally; in those cases, there would be no disturbance to the variables I _am_ controlling and you would see no "rebellious" response.

>PCT directs us to look for consequential patterns (such as reference levels)
>within the actors

Yes. The consequential patterns are not patterns of observable behavior; they are controlled _perceptions_, which can be objectively determined by the use of The Test for the Controlled Variable.

>Thus a thesis such as Sulloway's is crippled from the start because he takes
>a public label as his touchstone, then goes on to examine personal
>experience and motivation presuming that the systemics interior to
>personalities reflect these public labels. But PCT suggests that the
>presumption must be that they do *not* match; at the very least, allegation
>of a match must be argued and defended.

I think that's a pretty darn good statement of the situation. From the PCT perspective, the notion that a person is "rebellious" is, if anything, a hypothesis about a perceptual variable that that person is controlling. This hypothesis must be _tested_ before one starts trying to explain _why_ the person is controlling this perception. It makes no seens to explain why a person controls for being rebellious if that person is _not_ controlling for it -- but just _appears_ (to an observer) to be "rebellious".

> This may even suggest one insight into why people are uniformly annoyed by
>the labels which pidgeon-hole them, no matter how accurate those may be:
>for the labels identify things which are not important to the actors, but
>instead provide a sort of public utility.

Very close. I think the labels are annoying when they impute to you control of a variable that you are not (and possibly would not) control. If a person classifies my behavior as "rebellious" when what I am doing (from my perspective) is controlling for, say, "ideas based on an understanding of PCT", it is annoying because that person is imputing an intention to me (to be "rebellious") that I don't have (or don't _want_ to think I have;-))

>I'm crawling out on a limb, there, but I have to test my understanding of
>PCT somehow.

In my opinion, this was a sensational early effort.

Bruce Gregory (961203.0430 EST) --

>I'm at a definite disadvantage in this exchange, since I am actually
>reading what Sulloway says... I can see that having no knowledge of
>the man's work provides a freedom that I can only envy. I can only fall
>back on Cromwell's plea to the Irish bishops, "I beseech you, in the
>bowels of Christ, to think it possible that you might be wrong."

A very good point, although I did read about Sulloway's work in a New Yorker article and saw no evidence there to suggest that he tested to determine what perceptual variables were controlled by first-, second-, etc borns. I guess I

could be wrong about this. Does Sulloway explain how he tested to determine whether people were actually controlling for "rebelliousness"?

Hans Blom (961203d) --

>In world-model terms: You live in a world very different from mine.

Well, you sure got THAT right!!

>By the way, what is chutzpah? Sounds nice, though...

It's a Yiddish word that refers to a person who continues to _unashamedly_ teach the virtues of a model- based control theory of behavior in the face of overwhelming evidence that behavior is the CONTROL OF PERCEPTION.

Best

Rick

Date: Tue, 3 Dec 1996 12:00:59 EST
From: Bruce Gregory <gregory@CF4.HARVARD.EDU>
Subject: Re: Rebels with chutzpah

[From Bruce Gregory (961203.1200 EST)]

Rick Marken (961203.0830)

> A very good point, although I did read about Sulloway's work in a New Yorker
> article and saw no evidence there to suggest that he tested to determine what
> perceptual variables were controlled by first-, second-, etc borns. I guess I
> could be wrong about this. Does Sulloway explain how he tested to determine
> whether people were actually controlling for "rebelliousness"?

Far be it from me to claim that Sulloway is a closet-PCTer. My point was simply that Darwin is a rebel, not because of his behavior (this is true of Bill as well; at least as far as I can tell from Dag's tapes ;-))but because of his ideas. In fact, reading Darwin's biography does suggest some of the things we was controlling for and "rebelliousness" is not one of them! Your discussion would be right on target if Sulloway were labelling Darwin a rebel on the basis of his behavior, rather than on the basis of his intellectual impact.

> >By the way, what is chutzpah? Sounds nice, though...
> >
> It's a Yiddish word that refers to a person who continues to _unashamedly_
> teach the virtues of a model- based control theory of behavior in the face of
> overwhelming evidence that behavior is the CONTROL OF PERCEPTION.

Incredible. And they say the Greeks had a word for it...

Bruce Gregory

Date: Tue, 3 Dec 1996 12:03:04 -0500

From: Martin Taylor <mmt@HERMES.DCIEM.DND.CA>
Subject: Re: Learning, Control of others

[Martin Taylor 961203 11:40]

> Rick Marken (961130.2210) to Bruce Abbott

I've been wading through several hundred messages, and have saved a few for later (I hope) comment. Among them is a thread in which Bruce Abbott's modest proposal is elaborated and rejected, including the following quote:

> If what is learned is how to _vary_ a reference signal, then
> what your system is learning is transfer functions -- and everything is
> fine. Again, why not program it up so we can see how it works.
>

My understanding of Bruce's proposal is obviously different from yours. I understood Bruce to be arguing that the organism (rat) is developing a transfer function by table lookup, rather than as a functional description. I don't see any incompatibility between the two approaches, and indeed one can merge into the other if the lookup table values are in a perceptual space that has a continuous topology (i.e. is analogue rather than symbolic).

I could be misinterpreting Bruce, but if I am, I propose the above as another possibility for learning.

In the same post, Rick continues a thread on controlling others, started (?) by Tracy Harms (961124.15 MST)

>But
>intention to use people is a slaver mentality. It turns us directly beyond
>psychology, to questions of ethics.

Rick:
> Controlling your neighbor means getting him to behave in a particular
> way. If you want to make him _not want to hurt you_ or if you want to
> make him _have the right values_ then you would be trying to control him.
>

Don't we control people most of the time, every day, in the sense that we get them to act in ways that help us to control our perceptions? When I want an item from a shop, I control the salesperson so that he/she provides the desired information, or the item, or takes my money in exchange for the item. If I want to travel on a bus, don't I control the driver so that he/she makes the bus stop at the place I want to get off. Isn't it the very _basis_ of society that we control each other in ways that are, overall, to our mutual benefit (meaning that by such mutual control we, on balance, have better control over our _own_ perceptions than we otherwise would have)?

Social conventions are based around this acceptance of mutual control. The salesperson takes on a reference to see that the customer appears pleased with the service (or at least a good salesperson does). The customer's controlling actions don't disturb perceptions the salesperson is trying to control (at least not much, if the customer is a good customer, filling the conventional role). The salesperson has this reference value because bringing the perception of the customer to that "pleased" value is an aspect of controlling the salesperson's perception of a large paycheque. Everybody is improving their own control by allowing themselves to be controlled in "conventional" ways, by other people. That's why we have

a society at all, and are not solitary hunters in the forest.

The "political" problem comes when people try to control others in ways that reduce the others' control over their own perceptions--the usual N. American business management practices, and our present Ontario government, are examples that spring readily to mind. But to control others is not intrinsically bad. The "badness" comes when the actions that are used to effect control are, to the other, disturbances to their own controlled variables, to be resisted by their own actions. Therein lies conflict, or "hurt" or "enslavement."

Only 150 more messages to go...

Martin

Date: Tue, 3 Dec 1996 10:34:14 -0800
From: Richard Marken <marken@AEROSPACE.AERO.ORG>
Subject: Re: Rebels with chutzpah

[From Rick Marken (961203.1030)]

Bruce Gregory (961203.1200 EST) --

>Your discussion would be right on target if Sulloway were labelling Darwin a
>rebel on the basis of his behavior, rather than on the basis of his
>intellectual impact.

This is _really_ strange. You mean Sulloway is saying that birth order has an effect an something over which one has absolutely no control -- the intellectual impact of their ideas? So if Bill Powers were not first born, PCT might have had an intellectual impact. But it hasn't (which it certainly hasn't) because Bill is first born?

Just my luck; hitching my wagon to the ideas of a fellow first born;-)

Best

Rick

Date: Tue, 3 Dec 1996 14:49:59 -0500
From: MILLER dan <millerd@SABER.UDAYTON.EDU>
Subject: Re: Born to Rebel

[Dan Miller (961203)]

To: Hans Blom (961203):

Yes, it's true, sociologists study the social and legal aspects of crime. I'm not a criminologist, so don't hold this against me. However, several people down the hall devote their academic careers to these phenomena.

I'm not sure how you meant it, but I think I agree with you when you note that crime is a legal notion. Actually, it is relative by society, and is unevenly administered. My main

criticism of criminology is that sociologists are accepting without much thought the concepts offered them by this sector of society. I had a professor once who, when interviewing a candidate for a job, interrupted the fellow's remarks and asked him if he was a sociologist in Salem, Massachussetts in the 17th century would he use the concept of witchcraft as a sociological concept. He said that he would. The poor soul did not get the job. My professor thought that sociologists should study the concepts and notions that they derived through their work. I couldn't agree more.

Later,
Dan Miller
miller@riker.stjoe.udayton.edu

Date: Tue, 3 Dec 1996 14:18:30 -0600
From: Bruce Abbott <abbott@CVAX.IPFW.INDIANA.EDU>
Subject: Re: Born to Rebel

[From Bruce Abbott (961203.1520 EST)]

>Rick Marken (961202.1510) --

>Rick:

>Where do you think I fall -- first, later or only?

>>Me:

>>Definitely the baby of the family ;->

>I'm first of two, actually. I have a younger (and far more rebellious)
>brother. Maybe birth order just determines rebelliousness relative to
>other people in your family;-)

Hmmmm. Bill's a first-born, I'm a first-born, you're a first-born; there seems to be a pattern developing here. No wonder we're all so stubborn . . . (;->

I.Q. is higher in first-borns, too, only-children excepted. (;->

Regards,

Bruce

Date: Tue, 3 Dec 1996 13:05:05 -0800
From: Richard Marken <marken@AEROSPACE.AERO.ORG>
Subject: Re: Vancouver's experiment

[From Rick Marken (961203.1300)]

Jeff Vancouver (961203.0940 EST) --

>Now it appears that Rick and sometimes Bill P. advocate 1 [closed-loop
>control only]. Yet, Bill P. often acknowledges that model-based control

>sometimes occurs. Hans and I appear to believe he has even included it in
>PCT via imagination mode.

The PCT imagination connection can be _called_ "model-based control" but it is not the same as Hans' "model based control" model. In Hans' model, a model of the environmental function relating system output to controlled variable is continuously adjusted so that the model can continue to control by _generating output_ "in the blind" when no perceptual representaion of the controlled variable is available. Hans' "model-based control" model is always "controlling" by generating output based on the model of the environmental feedback function; it is _not_ a control of perception model. That's why Hans' model fails when there are unpredictable disturbances to the controlled variable.

The PCT imagination connection has nothing to do with control "in the blind"; it is a process of controlling _in imagination_. The imagination connection accounts for the fact that we _think_ and _remember_. You are "controlling" via the imagination connection when you _think about_ controlling the cursor in the "spiral" program - WITHOUT ACTUALLY DOING IT.

>Nonetheless, it appears that we have not developed an experiment or
>observation set that refutes 1 to the satisfaction of the players.

The data hasn't clearly or quantitatively refuted either model because subject performance has not been compared to model performance. But it has certainly qualitatively refuted the model-based control model. We would have expected the _same_ level of performance with eyes open _or_ closed if you are controlling via a Hans style model-based control system. Hans' model produces the same performance with eyes open or closed once it has adjusted the model properly. But no subject did equally well in the visual and blind conditions of the experiment. Bill proposed a plausible hierarchical control model to explain the fact that the subjects were able to move the mouse in a spiral pattern that kept the error lower than expected from chance movements in the blind condition. So the results don't rule out a closed loop control explanation until Bill's model is tested.

I think what you should do now is write a computer simulation of a model-based and a PCT controller doing the "spiral" task in the visual and blind conditions. You might be able to find versions of both models that handle the data fairly well. The next step would be to do a manipulation of the "spiral" experiment that produces vastly different behavior from the two models. Then test subjects in this situation and see if they behave like the model based controller or the PCT controller. I think this would be a VERY valuable research project.

Best

Rick

Date: Tue, 3 Dec 1996 16:10:36 -0500
From: Martin Taylor <mmt@HERMES.DCIEM.DND.CA>
Subject: Re: Born to Rebel

(Unannotated and undated message--I decline to acknowledge it)

>Bruce Abbott (961203.1520 EST)]
>

> I.Q. is higher in first-borns, too, only-children excepted. (;->
>

But the siblings of an only child, since they operate in imagination mode, are usually less rebellious than the second and later children of a multi-child family, aren't they? What does Sulloway have to say about them ?-)

Martin

Date: Tue, 3 Dec 1996 14:43:28 -0700
From: "T. B. Harms" <tbh@TESSER.COM>
Subject: Re: Learning, Control of others

>From Tracy Harms (1996;12,03.14)

Martin Taylor 961203 11:40 asks

>Don't we control people most of the time, every day, in the sense that we get
>them to act in ways that help us to control our perceptions?

To get technical, I think the answer must be NO. I, myself, am inclined to say that we do not control people because we cannot control people. I think PCT implies that we shoul constrain our use of the word to the control of perception. Since even Bill Powers talks about control of behavior I'll not presume to come in at this late date and chide people for doing the same. I do offer that we may always translate 'control of behavior' to mean 'control of perception of particular actions'. I further suggest that one important problem with control of behavior is that it involves *overspecification*: It involves focusing on means instead of ends. So, in addition to the larger problem that it will disrupt perception and thus provoke counterreaction, it also is hazardous in that it shifts attention from a more significant motivation to a lesser--perhaps irrelevant--one.

>When I want an item from a shop, I control the salesperson so that he/she
>provides the desired information, or the item, or takes my money in
>exchange for the item.

Hmmm. I don't think that's what *I* do. I think I control for getting information I desire, or for buying something, or for owning something. Only incidentally do I pay attention to the *actions* of the merchant. True, there seems to be fairly limited means by which they may do their side of the deal, but that is not my concern! To get distracted by their motions and mistake those motions for my goals seems like a quintessential example of non-PCT thinking.

>[...]Isn't it the very _basis_ of
>society that we control each other in ways that are, overall, to our mutual
>benefit[...]?

But the focus is usually (and properly) on *ends*, not *means*. If I'm an airline passenger I don't want to control the pilot landing at my destination airport. I am indifferent as to whether it is the pilot, co-pilot, or auto-pilot which maintains the aircraft's flight at any given time, just so long as between them they do a good job.

>Social conventions are based around this acceptance of mutual control.

Envisioning social order in terms of controlling people is what Bill Powers has called "the major premise of civilization". He is correct that this premise has been proven wrong.

>[...]
>The "political" problem comes when people try to control others in ways
>that reduce the others' control over their own perceptions--the usual N.
>American business management practices, and our present Ontario government,
>are examples that spring readily to mind. But to control others is not
>intrinsically bad. The "badness" comes when the actions that are used to
>effect control are, to the other, disturbances to their own controlled
>variables, to be resisted by their own actions. Therein lies conflict,
>or "hurt" or "enslavement."

I'm in agreement with these concluding remarks. This may indicate that our opinions are not so far apart as the rest of my comments suggest.

Tracy Bruce Harms tbh@tesser.com
Boulder, Colorado

"How far more interesting when we regard every production of nature as one which has had a history."
Charles Darwin

Date: Wed, 4 Dec 1996 13:51:51 +0100
From: "J.A. Blom" <J.A.Blom@ELE.TUE.NL>
Subject: Re: Vancouver's experiment

[Hans Blom, 961204]

(Jeff Vancouver 961203.0940 EST)

>> Right. Models can only model predictabilities, relationships that
>> do not vary over time -- or vary predictably. If something
>> unexpected (unpredicted by the model) happens, the model is
>> incorrect. So what to do? Fall back on feedback control? Even
>> feedback control requires certain regularities (a simple model).

>I am not sure I agree with the "simple model" part.

Let me explain. The feedback loop of a well-behaving, stable controller requires certain properties. First, the loop gain must be high enough (but, as practice shows, not too high). Given the transfer function gain of the outside (world) part of the loop, the transfer function gain of the inside part of the loop must be chosen such that the product of both is high enough. Second, the loop phase shift must be less than 180 degrees or the controller will turn into an oscillator. Thus, the controller (the internal part of the full control loop)'s design -- whether engineered or natural -- must be based on (must "know") certain properties of the environment. If the properties (gain, phase shift) of the outside part of the loop can change dramatically and unpredictably, no reliable controller will be possible. It is the fact that some pieces of "knowledge" of the

environment (gain and phase shift) must be incorporated into the controller that makes me say that every controller is based on a simple "model" (of the environment). This "model" need not be a clearly discernable mechanism, however. In a controller with fixed parameters, its gain and phase shift will be due to hard-wired "material" properties.

>I would agree that actions must have some impact on the
>environmental variable with some regularity, but neither the
>environment nor the array of control systems need be simple.

That is right. Yet, practice shows that even a simple low order model of a complex high order environment works well as the basis of a good controller. I don't know whether you are familiar with the tuning (optimal adjustment of the parameters) of PID controllers, which find frequent employ in all kinds of tasks. The knowledge that tuning requires of a (not too badly behaving) environment are just three aspects of the environment's transfer function: the gain, the delay time, and the dominant time constant. It is, amongst other things, the success of PID-type controllers that demonstrates that simple models of complex environments usually allow good control, if the environment is not too capricious. Not perfect or even optimal control, of course, although a criterium for optimality is usually difficult to formulate. And it is not that I don't strive for better models. The point is that simple models already can do the job well.

>The issue as I see it is to reconcile the theories. It seems that
>the options are:

>1) closed-loop control only
>2) model-based control only
>3) some combination of 1 & 2
>4) neither

I don't agree with the exclusion that you seem to discern between closed-loop control (1) and model-based control (2). Although I have not always expressed myself as clearly as I should in this respect, I consider model-based control to be A FORM OF closed-loop control (that is why I am here ;-), be it with some additional properties. The difference is that in model-based control the feedback loop includes an additional model, which one can think of as a transfer function with adjustable parameters. Two of the additional properties of model-based controllers are:

- robustness in the face of missing inputs (such as the "eye blinks" that you experimented with);

- a built-in learning mechanism that provides continuous adjustment of the controller's parameters to keep the controller operating stably in the face of changes of the environment.

The former, which actually emerges as a side product of the latter, has received the most attention of the PCT crowd, because it does not fit the standard PCT diagram where the comparator always needs both a reference input and a perceptual input; the latter MUST be present and cannot be dispensed with, even for a short time. Hence, in PCT, perceptions are necessarily thought of as always present. This, however, is hardly interesting and easily remedied. A simple sample-and-hold circuit (akin to episodic or short term memory?) in

the perceptual pathway could "hold" the last valid perception during e.g. eye blinks. In model-based control the mechanism is similar in principle to this crude zero order hold scheme, but slightly more complex (and much better ;-).

This problem of missing samples is not really (much of) a problem. Have you ever tried to visually fit a curve through a set of data points? Of course. Humans are very good at such tasks. Remarkably, however, many computational curve fitting procedures assume a number of properties that humans do not require. The simplest procedures require that the samples are obtained at regular intervals (at a fixed sampling frequency) and that there are no missing samples. Compare this to the PCT approach. A less simple procedure can tolerate missing samples, but still requires a constant sampling frequency. This is usually my approach. An even more complex (but readily available) procedure can also process non-equidistant data points. A real-time version of such a procedure, however, is rather difficult to implement; the code is complex and the computational load is large. A solution is to fall back on the constant sampling frequency method with a high frequency but only an occasional (valid) sample, which is an almost-equivalent.

The latter additional property of model-based controllers (real-time learning) is the more interesting aspect for me. Although Bill Powers has demonstrated that this can be done in specific cases (e.g. his Artificial Cerebellum), he has not come up with a general mechanism and the "laws" according to which such a mechanism could or should operate. That may be due to the fact that probability theory and statistical procedures (the implementation of real-time discovery of correlation/transfer functions) are the normal approach in this type of learning.

What I am looking for is a combination of the basic PCT scheme of things (with some reservations on minor points) with a generally applicable real-time learning mechanism.

One major problem is to discover, through theory and simulations, whether learning can be local (a la Hebb) or must necessarily be global. I suspect (regrettably) the latter. It then is a research theme whether and how global learning can be supported by the type of hierarchy that HPCT proposes. It has been shown that if an organism is modelled as a collection of cooperating "agents", learning can take place, but the chance of getting stuck in a local extremum is high. Is this true for a hierarchical approach as well?

Well, this is a lot of work. Don't expect results any time soon...

Greetings,

Hans

Date: Wed, 4 Dec 1996 14:31:30 +0100
From: "J.A. Blom" <J.A.Blom@ELE.TUE.NL>
Subject: Re: Vancouver's experiment

[Hans Blom, 961204b]

>The PCT imagination connection can be _called_ "model-based control"
>but it is not the same as Hans' "model based control" model. In
>Hans' model, a model of the environmental function relating system
>output to controlled variable is continuously adjusted so that the
>model can continue to control by _generating output_ "in the blind"
>when no perceptual representaion of the controlled variable is
>available. Hans' "model-based control" model is always "controlling"
>by generating output based on the model of the environmental
>feedback function; it is _not_ a control of perception model.

>The PCT imagination connection has nothing to do with control "in
>the blind"; it is a process of controlling _in imagination_. The
>imagination connection accounts for the fact that we _think_ and
>_remember_. You are "controlling" via the imagination connection
>when you _think about_ controlling the cursor in the "spiral"
>program - WITHOUT ACTUALLY DOING IT.

Now think of this: What is the use of the imagination connection and
of controlling in imagination if it does not somehow lead to actions?
Why have the mechanism at all? Just for fruitless daydreaming that
never is translated into actions?

The term "controlling in imagination" is absurd; if it did not lead
to improved reality connected real life control, "imagination" would
never have arisen. WHY do we think and remember? It's all in the
service of how we control, how we interact with the real world.

In my opinion, the "imagination connection" (alias "world model" or
"acquired knowledge") operates ALL THE TIME and AT ALL LEVELS,
tapping information and filling in details that are not momentarily
available to our perceptual apparatus. That goes for the spiral
tracking experiment as well. We _know_ just about where that dot on
the computer screen should have been -- if the delay is not too long.

We may fight about the quality of a "world model" or its equivalent
and its implementation, but it is certain to me that my present
actions are based on much more than the information that my sensors
present to me right now.

Greetings,

Hans

Date: Wed, 4 Dec 1996 14:42:57 +0100
From: "J.A. Blom" <J.A.Blom@ELE.TUE.NL>
Subject: Re: Learning, Control of others

[Hans Blom, 961204c]

(Tracy Harms (1996;12,03.14)) responding to Martin Taylor 961203

>>Don't we control people most of the time, every day, in the sense
>>that we get them to act in ways that help us to control our
>>perceptions?

>To get technical, I think the answer must be NO.

This contrasts somewhat with what you say later:

>>When I want an item from a shop, I control the salesperson so that
>>he/she provides the desired information, or the item, or takes my
>>money in exchange for the item.

>Hmmm. I don't think that's what *I* do. I think I control for
>getting information I desire, or for buying something, or for owning
>something. Only incidentally do I pay attention to the *actions* of
>the merchant.

Or his/her intentions, I guess. That way, you treat another person much like you would treat an inanimate object such as a rock or a hammer. That comes close to what I said about "using" others (as tools to achieve our aims).

Greetings,

Hans

End of CSGNET Digest - 3 Dec 1996 to 4 Dec 1996
****

Date: Thu, 5 Dec 1996 08:00:32 -0600
Subject: CSGNET Digest - 4 Dec 1996 to 5 Dec 1996

There are 27 messages totalling 1809 lines in this issue.

Topics of the day:

Date: Wed, 4 Dec 1996 08:05:46 -0800
From: Richard Marken <marken@AEROSPACE.AERO.ORG>
Subject: Spider Tricks, Social Control

[From Rick Marken (961204.0800)]

Scott Stirling (2 Dec 1996) --

>Did any of you see the article on jumping spiders in last month's
>_National Geographic_? There was a genus being studied that tricked
>other kinds of spiders into vulnerable positions...I heard of PCT after
>reading this article...This sounds like a text book case of PCT to me.

All purposeful behavior is a textbook case of PCT;-) The question is what are the spiders' _purposes_ in this case. I think it's highly unlikely that these spiders have the purpose of "tricking" another spider: I don't think spiders can perceive the complex relationship called a "trick" in the way we perceive it.

The "tricky" spider is probably controlling for perceiving a "tap" on a weblike structure. This tap happens to be a disturbance to a perception (like the perception of food) that the "tricked" spider is controlling for. The "tricked" spider acts to compensate for the "tap" disturbance, which brings it into the jaws of the "tricky" spider. So the "tricky" spider is controlling (for food) by taking advantage of a control system in the "tricked" spider.

This is a very nice example of a control system (the "tricky" spider) that controls by taking advantage of the controlling nature of another control system (the "tricked" spider).

Martin Taylor (961203 11:40) --

>Don't we control people most of the time, every day, in the sense that we get
>them to act in ways that help us to control our perceptions? When I want an
>item from a shop, I control the salesperson so that he/she provides the
>desired information, or the item, or takes my money in exchange for the item.

Tracy Harms (1996;12,03.14)

>To get technical, I think the answer must be NO.

I agree. But I think it is important to understand why Martin _thinks_ social interaction involves mutual control. When I want an item on the shelf, I _assume_ that the salesperson is controlling for providing it to me; I am _using_ the salesperson's controlling as part of the means for doing my controlling (getting the item). If the salesperson has adopted this goal, then it _looks like_ I have controlled the salesperson (by saying "can I have that item, please"). In fact, I am not controlling the salesperson; I am just capitalizing on the autonomous controlling being done by this person in order to achieve my goals.

If the sales person doesn't get the item I request, I might be inclined to try to _control_ the salesperson. That is, I could try to get the salesperson to behave in a way I think is appropriate. People often do try to control service people who don't behave "properly". They usually try to control using threats ("I'll call your manager") or violence ("get it, or I'll punch you in the nose"). Sometimes this kind of control works (the salesperson behaves as you want) and sometimes it doesn't. But once you get into this kind of control regime, conflict is a likely result.

Society doesn't depend on mutual control (you control me and I'll control you); it depends on cooperative control (I'll voluntarily control for this -- which will help you-- if you will control for that -- which will help me).

>I, myself, am inclined to say that we do not control people because we
>cannot control people.

Actually, people _can_ control each other; they can control the actions other people use to control their perceptions. For example, I can make you bend down (control my perception of the angle of your body) if I know you want money (your controlled perception) and I throw a dollar on the ground.

But my ability to exert this kind of control is limited. I can only get you to take actions (like bending down) that don't disturb other variables you are controlling. For example, I can't get you to bend down for the dollar if you have a bad back and such bending would create a pain perception for which you have a reference of zero.

So we can control other people; we just can't control them _arbitrarily_, which means that we can't control them in a way that does not take into account their entire structure of goals. Since we can never really know a person's entire structure of goals, it is best _not_ to even _try_ to control other people since it is almost certain to create intra- or interpersonal _conflict_.

Hans Blom (961204b) --

>In my opinion, the "imagination connection" (alias "world model" or
>"acquired knowledge") operates ALL THE TIME and AT ALL LEVELS,

Hans, if you had anything more than an opinion (like some data, for instance) I might be interested. Hopefully, Jeff Vancouver will be doing the research that shows just how much "model-based control" is involved in ordinary controlling. Once we have the data, then we can talk. But based on what we already know from the simple spiral demo, the idea that your kind of "model-based control" is working "ALL THE TIME and AT ALL LEVELS" appears to exist only in your "imagination connection".

Best

Rick

Date: Wed, 4 Dec 1996 11:00:49 -0500
From: Martin Taylor <mmt@HERMES.DCIEM.DND.CA>
Subject: Re: Control of others

[Martin Taylor 961204 10:30]
>Tracy Harms (1996;12,03.14)
>
> Martin Taylor 961203 11:40 asks
>
> >Don't we control people most of the time, every day, in the sense that we get
> >them to act in ways that help us to control our perceptions?
>
> To get technical, I think the answer must be NO. I, myself, am inclined to
> say that we do not control people because we cannot control people.

To get technical is not to get tautological, nor to appeal to articles of faith.

> I
> think PCT implies that we shoul constrain our use of the word to the
> control of perception.

Exactly as I did, in the usual sense that when we say "control the position of a cursor" we always mean "control our perception of the position of a cursor." You are doing exactly as I did when I was new to this group and chided Bill P. for talking about controlling an aspect of the outer world, when he was just using the (as I now know) accepted shorthand form.

> Since even Bill Powers talks about control of
> behavior I'll not presume to come in at this late date and chide people for
> doing the same.

Having done so.

>I do offer that we may always translate 'control of
> behavior' to mean 'control of perception of particular actions'.

As I explicitly did, in the sentence you quoted.

>I further
> suggest that one important problem with control of behavior is that it
> involves *overspecification*: It involves focusing on means instead of
> ends.

Possibly, but you have to evaluate this in each case. If I want an item in a shop, I have to perceive the item to come into my hands. If this requires that the salesperson get it off the shelf and hand it to me, I must control the salesperson ('s actions) so that the salesperson does so. This will work only if the salesperson has no reference perceptual levels to the contrary.

An essential element of mutually beneficial control of other people is that the actions desired by the controller do not interfere with actions the controllee uses to implement control of his/her own perceptions. There's a degrees of freedom issue.

Overspecification can indeed be an issue, and it can be deadly. In other contexts, it is called "micromanagement" and it seldom fails to cause problems.

>So, in addition to the larger problem that it will disrupt
> perception and thus provoke counterreaction, it also is hazardous in that
> it shifts attention from a more significant motivation to a lesser--perhaps
> irrelevant--one.

I don't understand a word of this.

> >When I want an item from a shop, I control the salesperson so that he/she
> >provides the desired information, or the item, or takes my money in
> >exchange for the item.
>
> Hmmm. I don't think that's what *I* do. I think I control for getting
> information I desire, or for buying something, or for owning something.
> Only incidentally do I pay attention to the *actions* of the merchant.

Yes, but when you are driving to your house, only incidentally do you pay attention to controlling your perceptions of the correct turns and avoiding the other cars; but if you fail to control those perceptions, you don't perceive yourself getting home. So, when you control for getting information, and so forth, but fail to control your perceptions of the actions of the salesperson, you don't get the information--the salesperson having gone for coffee while you were not asking for the service.

> True, there seems to be fairly limited means by which they may do their
> side of the deal, but that is not my concern! To get distracted by their
> motions and mistake those motions for my goals seems like a quintessential
> example of non-PCT thinking.

So, you don't believe in HPCT, but only in single-level perceptual control? Goals (perceptual reference values) exist only at one level, and should not be mistaken for non-goals that are non-existent reference levels for perceptions at non-existent lower levels?

> >[...]Isn't it the very _basis_ of
> >society that we control each other in ways that are, overall, to our mutual
> >benefit[...]?
>
> But the focus is usually (and properly) on *ends*, not *means*. If I'm an
> airline passenger I don't want to control the pilot landing at my
> destination airport. I am indifferent as to whether it is the pilot,
> co-pilot, or auto-pilot which maintains the aircraft's flight at any given
> time, just so long as between them they do a good job.

One of the fundamental aphorisms of PCT is "many means to a given end." You, presumably, would like to control the pilot and crew to the extent that they don't parachute out of the plane (if you had the ability to do so). That they don't do so is at least in part because one of their goals is to get you to where you want to go in good time and in a mood that will not get you to reorganize so that you take a different airline next time! They are controlling you, too. Of course you are controlling for your own ends, but you can't do so without the means.

>
> >Social conventions are based around this acceptance of mutual control.
>
> Envisioning social order in terms of controlling people is what Bill Powers
> has called "the major premise of civilization". He is correct that this
> premise has been proven wrong.
>
Nonsense. He has commended this view of the development of social convention, and quite recently, too. It is based on the minimization of conflict among the controlled perceptions both between ECSs within individuals and between ECSs in different individuals. It's mutually beneficial.

>... The "badness" comes when the actions that are used to
> >effect control are, to the other, disturbances to their own controlled
> >variables, to be resisted by their own actions. Therein lies conflict,
> >or "hurt" or "enslavement."
>
> I'm in agreement with these concluding remarks. This may indicate that our
> opinions are not so far apart as the rest of my comments suggest.
>
Possibly, but I propose further study of _Hierarchic_ PCT before you say so with assurance. Your agreement with this part cannot be searated from your disagreement with the earlier part of my posting, just because the conclusion agrees with your prejudice. Both parts are of a seamless whole, based on the same analysis of the interaction of control hierarchies in an environment of limited dimensionality.

Martin

Date: Wed, 4 Dec 1996 10:59:18 EST
From: Bruce Gregory <gregory@CF4.HARVARD.EDU>
Subject: Re: Rebels with chutzpah

[From Bruce Gregory (961204.1100 EST)]

Rick Marken (961203.1030)

> Just my luck; hitching my wagon to the ideas of a fellow first born;-)

We first-borns are not natural rabble rousers. It's no doubt
due to our inordinate modesty and lack of chutzpah ;-)

Bruce Gregory

Date: Wed, 4 Dec 1996 11:25:30 EST
From: Bruce Gregory <gregory@CF4.HARVARD.EDU>
Subject: Re: Control of others

[From Bruce Gregory (961204.1125 EST)]

Martin Taylor 961204 10:30

I found this and your earlier post both clear and persuasive.

Bruce Gregory

Date: Wed, 4 Dec 1996 11:34:24 EST
From: Bruce Gregory <gregory@CF4.HARVARD.EDU>
Subject: Re: Spider Tricks, Social Control

[From Bruce Gregory (961204.1135 EST)]

Rick Marken (961204.0800)

> I agree. But I think it is important to understand why Martin _thinks_ social
> interaction involves mutual control. When I want an item on the shelf, I
> _assume_ that the salesperson is controlling for providing it to me; I am
> _using_ the salesperson's controlling as part of the means for doing my
> controlling (getting the item). If the salesperson has adopted this goal,
> then it _looks like_ I have controlled the salesperson (by saying "can I have
> that item, please"). In fact, I am not controlling the salesperson; I am
> just capitalizing on the autonomous controlling being done by this person in
> order to achieve my goals.

This seems like obfuscation to me. I control my perception of
the salesperson's actions by asking for something. The fact
that the salesperson is also a control system limits the ways I
can control that perception, but that doesn't mean that I only
_appear- to be controlling. If the salseperson ignores me, I
resist the disturbance and ask to speak to his supervisor. You
apparently mean something more by control than PCT means by
control.

Bruce Gregory

Date: Wed, 4 Dec 1996 12:03:09 EST
From: Bruce Gregory <gregory@CF4.HARVARD.EDU>
Subject: Billy Graham and PCT

[From Bruce Gregory (961204.1215 EST)]

It occurs to me that one way to look at "born again" Christianity is that it begins with the message "Look at what a mess you have made trying to exercise control in your life. If you would only surrender control to an omniscient and beneficent controller, things will go much better for you." Indeed, for many people the notion that "everything happens for a reason" and "God's will be done" seems to dramatically reduce the anxiety associated with being an autonomous controller in a world filled with other autonomous controllers.

Bruce Gregory

Date: Wed, 4 Dec 1996 10:25:04 -0700
From: Bill Powers <powers_w@FRONTIER.NET>
Subject: Re: Vancouver's experiment

[From Bill Powers (961204.0800 MST)]

Hans Blom, 961202d--

>(Jeff Vancouver 961125.09:55 EST)
>
>>Below are the data from several different type of [spiral tracking]
>>runs.
>
>Thanks for the data, Jeff. I would have done something similar
>(blanking the screen feedback rather than closing eyes) if I had had
>the time. Your results are also about as I would have expected ;-).
>
>> 6.44 visible
>>115.10 Alt 10s
>> 28.38 Alt 2s
>
>>I think that the data (mine and others) clearly demonstrate that we
>>use closed-loop control. If there are any open-loop only advocates
>>out there, I invite a different interpretation.
>
>My interpretation: we have a built-in "world model" that allows us to
>somehow predict the effect of our actions even if perceptual feedback
>is temporarily unavailable. The quality of the prediction will
>generally deteriorate after some time, and your data show how bad
>this deterioration is for this rather complex, dynamic task. For very
>short times, "tracking" is very good in tasks such as this; for times
>greater than a few seconds it is pretty bad.

Your "interpretation" is simply your model-based control model, as usual, rather than a fresh look based on the experimental results. It seems to me that your interpretation implies a very large expenditure in neurons and

computing power to achieve a very small advantage, while giving up the ability to compensate immediately for unexpected (unpredicted) disturbances like Jeff's stuck mouse.

We can't let a particular engineering approach, developed under conditions that are, perhaps, appropriate for the construction of artificial systems where we have extensive knowlede of the environment and where the objectives are external to the system, dictate a model of the human or animal system. I think we should use the concept of model-based control sparingly, where it is clearly required to explain phenomena, and keep the simpler model where it seems adequate (and where dealing with unexpected disturbances is taken care of without an encyclopedic model of the entire physical universe).

A case in point is your recent post on transfer functions. You propose putting a transfer function in series with the reference input. This means that the actual reference signal is modified so that the _effective_ command to the control system includes precompensations of various kinds. How would a living system perceive that the direct reference input is giving a wrong result, and adjust the compensation until the result is better? I can understand how an engineer would do it, with the control system laid out on the lab workbench and with oscilloscope probes and recording devices attached to various parts of the system and its environment, but a living system, that system on the lab workbench, has to accomplish this result without external aid. Somehow the living system must know what a "good" result is, and compare the actual result with this "good" result, and make the adjustments accordingly. But the apparatus for doing that is absent from your model, just as the apparatus for generating the basic world model with adjustable parameters is missing from your model. The nearest you have come to supplying these missing smart systems is to say that "evolution" has taken care of all that. That's not the kind of answer I'm looking for.

You asked where the world-model is in the system I sketched in for mimicking the behavior in Vancouer's "spiral" experiment. There isn't one, unless you want to stretch the definitions and say that it's implicit in the various leaky integrators and in the relationships among the levels of systems. That, of course, makes any control system into a sort of model of its environment, but I don't think that's what you mean by a world-model. My main purpose in showing that model was to show how a system could be built without a world-model that would still be able to continue control of the derivatives for some time after loss of the position perceptual signal. I could have gone further and made each system a pair of one-way control systems, so that loss of input signal would automatically create a composite error signal of zero rather than some large value, without any need for switching modes. The point is that if you start with a preconceived model, you shut the door on other, simpler, solutions to the problem and force yourself into ever-increasing complexities for no better reason than to avoid giving up the initial model. The fixed point in our investigations is the organism, not any particular model.

>Yes, that is how I would say it as well. Learning installs a transfer
>function that we can even rely on (more or less) if there is no
>feedback. I have this mental model, for instance, of how I can open
>my front door. I need to use a key, insert it into the keyhole, turn
>it to the left for almost 360 degrees, push a little against the
>door, etc. That model is pretty reliable: no random trial and error
>required.

Except when you're just starting to learn how to open doors with keys.

Your description of how you open a door is a good illustration of where mental models probably come in, and what their nature is. What you have given us is a list of the symbolic NAMES of certain results that need to be accomplished in a certain sequence: use key, insert in keyhole, turn left, push against door. I think that at this level we are very likely to use world-models, but they are models cast in logical or verbal form and converting symbolic inputs into symbolic outputs. Once this kind of world-model has produced the symbolic specification "key in lock," it is up to lower-level systems to create a real situation that can be interpreted as fitting this symbolic description of the desired perception. And that process requires being able to adjust the key in relation to the lock in the analog world, taking into account waverings in the hand holding the key, the position of the body, any loads being carried in the arms, the stiffness of the wards inside the lock (dependent on the state of lubrication and the ambient temperature inside and outside the house), and all the other truly unpredictable aspects of the world that exist at the moment the described result is to be physically created.

>We humans like predictability a lot, and we pretty much organize our
>world to be predictable. No shape-shifting houses, cars, roads, etc.
>Even education/enculturation has as its main goal to get people to
>behave predictably.

Speak for yourself, Hans. I think that a person who really loves predictability will also love your world-model approach. But I think that such a person will also tend to minimize the variability of the world, because in fact we all manage to behave successfully by _varying_ our actions as appropriate to the immediate environment, and this doesn't fit a conception in which everything is understood and planned out in advance. Planning is a highly overrated activity; if you put too much faith on planning, you are likely to say that whatever happens is what you intended, whether it actually was or not.

RE: world-model vs closed-loop multilevel model:

>>This function will not be as good as it would with on-line data, but
>>it will be better than if no function were there (as indicated by
>>comparing to random, no movement, or linear/trajectory movement).
>>Any takers?

Are you going to make me program an example of my model? Exactly the same description would apply to it. I really don't want to get into such a hypothetical model if you get the idea; we could go off into imaginary worlds of ever-increasing detail and ever-diminishing importance. But if I didn't explain my model adquately, I suppose I'll have to come up with a working example. Not that I would be suggesting for a moment that the real system works that way (which is why I'm not terribly interested in doing this).

Best,

Bill P.

Date: Wed, 4 Dec 1996 10:25:10 -0700
From: Bill Powers <powers_w@FRONTIER.NET>
Subject: Re: Vancouver's experiment

[From Bill Powers (961204.0930 MST)]

Jeff Vancouver 961202.16:15 EST--

>> Very nice, Jeff. I appreciate simple direct solutions to problems. Blank
>>the screen? Close the eyes!
>
>Thank you. This follows years of training and teaching research methods.

You would have made a good engineer.

>I did not get a lot better and I don't have the patience for 100 trials.
>I did improve slightly, just does beg the question: what is improving?
>Precision?

Depends on what model you assume. In Hans' model, he would say that the parameters of the world-model are being adjusted on the basis of the difference between the real performance and the world-model's performance. In my model, I would say that the accuracy of real-time control is improving, and that this leads to a slight improvement in eyes-closed performance.

>Wow, I wish I had a degree in engineering.

Well, Hans has a degree in engineering and he doesn't buy this model, so what would a degree do for you?

>If I understand this model
>(which I am sure I do not), it seems that you are saying that a control
>system can perform the tracking task without data _or_ without setting
>itself up so that it can operate without data. Control systems just set
>themselves up that way.

Remember that we're trying to account for the fact that you can move your hand in an approximate spiral when you close your eyes, but only for a short time. I proposed a control system design that would also behave this way when its visual input is lost. It would not really continue controlling, but its output actions would, for a short time, follow the same trajectory they were following at the moment of loss of input. Equally important is that after just a few seconds it would start wandering away from the right trajectory, eventually getting very far off.

Here's a simple experiment you can do without a computer. Get a friend to stand facing you and track your finger while you move it smoothly around in a largish vertical circle at a moderate speed. Keep doing this until the tracking is pretty good, then ask your friend to close his, her, or its eyes and continue tracking as long as possible. You'll see what happens the instant the eyes are closed.

Now ask your friend to move the finger in the same circle at the same speed with eyes closed, but without your finger present. You'll see that it is perfectly possible to generate a cyclic movement of this kind on the basis of kinesthetic perceptions alone. So consider that behavior as the product of a subsystem already designed to produce circular motions, as perceived kinesthetically. The mass of the arm prevents any extreme or sudden departures from the circle, and the control system does maintain a roughly circular trajectory.

Now, if you start moving your finger in a circle again and ask the other

person to look again, you'll see a quick correction, after which tracking will be good again. What has the person adjusted? Obviously, the gross circular motion doesn't need to be generated from scratch; the subsystem that was moving the finger in a circle with the eyes closed takes care of those details. What the visual system has to do is add slow corrections of the phase (position around the circle), amplitude (radius of the circle), and center (center of revolution). These slow adjustments represent what the visual system is doing. If you suddenly lose visual input, these adjustments will start to drift, and the circle's phase, amplitude, and center will start to drift -- but there will still be that basic circular motion, which will remain almost correct for a short time, and then become more and more in error. As you were saying, even when the eyes close the result will be better than a random motion or a linear motion.

>This does seem to be different from your imagination mode

You're right, it is. All that's happening is that you continue to control those perceptions you can still receive (kinesthetic), but with respect to reference signals for phase, amplitude, and center that have started to drift.

Best,

Bill P.

Date: Wed, 4 Dec 1996 10:25:14 -0700
From: Bill Powers <powers_w@FRONTIER.NET>
Subject: Re: Stat Problem

[From Bill Powers (961204.0950 MST)]

Fred Nickols (961202.1900 EST)--

Thanks to all for thinking about my problems!

>[T]he standard way of comparing models is to look at the residuals.
>So first calculate the residuals (observed - predicted), the mean
>square(the sum of the squared residuals divided by the degrees of
>freedom) error tells how much variance is not explained. That is a
>good way of comparing which of two models fit better.

What do you mean by degrees of freedom? The number of data points? In the tables for F-tests and Chi-squared that I've seen, the number of "degrees of freedom" seem to go only up to about 30 or 40, hardly enough for one second of data in a tracking experiment, or one month of the rat experiments. What I use now is simply RMS error between predicted and actual behavior, divided by the range of the behavioral variable.

What is an F-test?

>1) plot residuals against the observed dependent variable.
> See if there is still some pattern left in the data. Also
> plot the residuals against each of the independent
> variables, it might tell if a squared term will fit better.

That's a good idea; I'll probably try it.

>2) Also usually the researcher is concerned about fitting

>   a model for a whole population of something (e.g. rats),
>   not just the ones in his/her sample.

Not in control-system experiments. We expect individual differences in the model parameters, although not in the _form_ of the model. It wouldn't make any sense to look for the parameters that would fit a whole population; they wouldn't predict what _any_ subject does. If any population studies were done, they would be done _after_ the individual parameters had been determined. We might be interested in the range of loop gains, or reference levels, or output sensitivities. We test the validity of parameters by putting the same individuals in a new experiment (with different disturbance patterns and even added kinds of disturbances) and using the model with parameters determined from other experiments to predict the behavior. In tracking experiments such predictions fit one individual's behavior far more accurately than they would fit the "mean behavior" of a population.

>   Cross-validation,
>   then can be an important tool. Either do more observations,
>   and see how well the previous fit model fits to the new data.
>   Or Initially randomly split your observations into 3/4 of the
>   data to run the initial model on and 1/4 to see how it fits.
>   You could even do this repeatedly to get a better model.

That's what we do already. In fact, Tom Bourbon is conducting a long-term experiment in which, once every five years, he does another run on a tracking experiment, with a new disturbance pattern, which was predicted back in about 1985. So far the model is predicting handle movements within about 3% of actual handle movements.

><Begin Response #2, from Don Rock>
>
>I completely agree with [Bruce's] suggestions -- a slight variation
>is doing a bootstrap on the cross-validation sample. In this case
>the model(s) parameters from the validation sample are considered
>fixed and the variability (stability) of the estimates can be evaluated
>using the bootstrap on the cross-validation sample. This does
>assume a reasonable sample size on the cross-validation sample.

Uh, that's not very helpful to a statistical dimwit.

Best,

Bill P.

Date: Wed, 4 Dec 1996 10:25:18 -0700
From: Bill Powers <powers_w@FRONTIER.NET>
Subject: Re: simultaneity

[From Bill Powers (961204.1010MST)]

Hans Blom, 961203c--

>>Flipping a light switch is not supposed to be followed instantly by
>>a loud bang. In fact, it is supposed NOT to be followed by anything
>>but a light turning on or off.
>
>I would call that a mental model: in my experience, flipping a light

>switch does not cause anything but turn a light on or off (this may
>not be true for some electrical engineering students).

You can see that as a mental model, or simply as a perceptual signal. The perception would be that of a particular sequence: switch, light. The reference condition is to perceive that sequence. If something else happens when you flip the switch -- for example, the light doesn't go on -- there is an error in the sequence. There's a sort of model involved, in that you're prepared to perceive a particular sequence as matching the reference conditions and all others as not matching it. But this isn't a world-model in the same position as in your model, running from output to input via an imagination connection. It involves real-time perceptions from the outside world, not imagined perceptions from the inside model. Of course in your model you still have to explain how it is that the person perceives sequence as a variable that can be correct or incorrect.

Best,

Bill P.

Date: Wed, 4 Dec 1996 12:18:23 -0500
From: Martin Taylor <mmt@HERMES.DCIEM.DND.CA>
Subject: Re: Social Control

[Martin Taylor 961204 12:10]
>Rick Marken (961204.0800)]
>... I am
> _using_ the salesperson's controlling as part of the means for doing my
> controlling (getting the item). If the salesperson has adopted this goal,
> then it _looks like_ I have controlled the salesperson (by saying "can I have
> that item, please"). In fact, I am not controlling the salesperson; I am
> just capitalizing on the autonomous controlling being done by this person in
> order to achieve my goals.
>
> If the sales person doesn't get the item I request, I might be inclined to
> try to _control_ the salesperson. That is, I could try to get the
> salesperson to behave in a way I think is appropriate. People often do try
> to control service people who don't behave "properly". They usually try to
> control using threats ("I'll call your manager") or violence ("get it, or
> I'll punch you in the nose"). Sometimes this kind of control works (the
> salesperson behaves as you want) and sometimes it doesn't. But once you get
> into this kind of control regime, conflict is a likely result.

I am not clear why controlling our perception of the position of a cursor can be talked about with the shorthand "controlling the cursor position," but controlling our perception of other people's control actions cannot (should not?) be talked about with the shorthand "controlling the other person."

If the reason is that "controlling the other person" _has_ to imply getting the other to do something that conflicts with their own control, then how is it that one can say "controlling the cursor position" when the cursor has nothing with which my control can conflict?

It seems to me much more natural to use the same terminology as a short form in both cases. It's much too longwinded always to say "controlling my perception of the actions or the other person." I'd prefer to use

"controlling the other person" in just the same way as "controlling the cursor position." The question of whether this control involves conflict is an independent issue.

Anyway, I think (or rather, I hope) that the issue is one of terminology, not of what is happening when two or more people take on roles for their mutual advantage.

Martin

Date: Wed, 4 Dec 1996 09:53:34 -0800
From: Richard Marken <marken@AEROSPACE.AERO.ORG>
Subject: Re: Social Control

[From Rick Marken (961204.1000)]

Bruce Gregory (961204.1135 EST)]

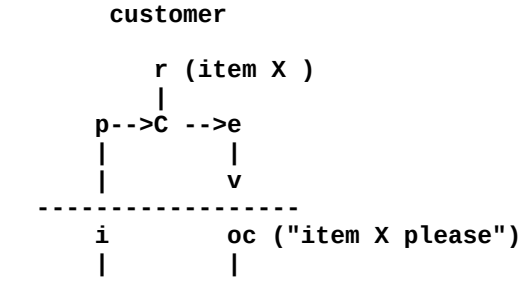
>This seems like obfuscation to me. I control my perception of
>the salesperson's actions by asking for something.

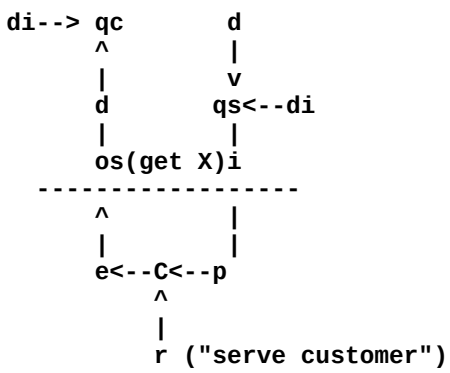
Not necessarily. I was assuming that what you are controlling for is getting the item. You don't care exactly "how" the salesperson gets it to you -- carries it by hand, brings it in a basket, etc.

>If the salseperson ignores me, I resist the disturbance and ask to speak to
>his supervisor. You apparently mean something more by control than PCT means
>by control.

This occurs only if you do, indeed, want to control the salesperson's actions as the means of getting the item. But there are other ways of controlling for getting the item _without_ trying to control the salesperson. If the salesperson ignores you, you might control for getting the item by just reaching for it yourself or asking another sales person for it or leaving for a different store. Controlling your perception (of the item) does not _require_ that you control your perception of the salesperson's actions.

I agree that it is annoying when people don't cooperate and set the references that we assume they _should_ set in order for us to be able to achieve our social goals. But we don't _have to_ control other people in order to achieve our goals. My point was simply that we can _usually_ achieve our goals by asking others to do something for us because we can count on the fact that those others have goals that make our request a disturbance to a perception that can only be controlled by their taking actions that help us. We are not really _controlling_ for the people taking those actions. Perhaps a diagram of the situation will help:





The dashed lines are person/environment boundaries. The customer is controlling a perception of qc, which is having item X in hand. The server is controlling for qs, which is giving service on request.

Note that the customer is _not_ controlling the server's actions (os). The server's actions are just an influence on (disturbance to) the variable controlled by the customer (qc). The customer is "using" the server as part of the control loop that allows him to control his perception of item X. The customer is _not controlling_ the server: he is not controlling either qs (the variable controlled by the server) or os (the server's actions).

If the server does not have a reference to "serve the customer" the customer will find that asking the server for item X is an ineffective way to control for item X. If the customer is _also_ controlling for seeing a particular type of behavior from the server, then he _will_ take action to control for that behavior (either os or qs). But it is not a _necessary_ aspect of the customer/server relationship. (Note: The di's are disturbances to the variables controlled by server and customer other than those produced by the actions of the server and customer).

Does this help?

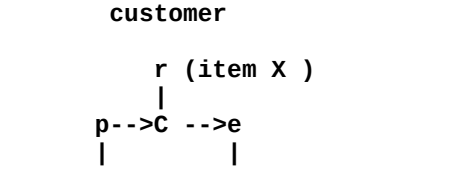
Best

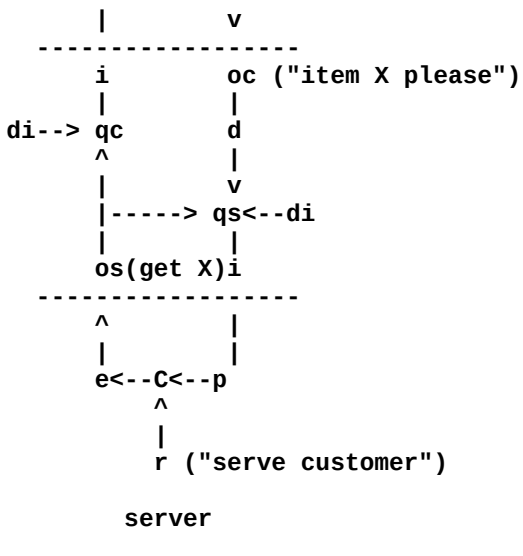
Rick

Date: Wed, 4 Dec 1996 11:30:21 -0800
From: Richard Marken <marken@AEROSPACE.AERO.ORG>
Subject: Re: Social Control --a minor correction

[From Rick Marken (961204.1130)]

I forgot to draw the connection from the server's output (os) to the server's controlled variable (qs). Here's a better version of the diagram:





Now it's clear that the server is controlling his own perception of qs ; as a side effect, the server's actions influence the customer's controlled variable (qc). The only effect the customer has on his own controlled variable (qc) is via the server; the customer has no direct effect on his own controlled variable. So, if the server doesn't have the "correct" reference (to serve the customer) the customer cannot control for getting item X.

Martin Taylor (961204 12:10) --

>I think (or rather, I hope) that the issue is one of terminology, not of
>what is happening when two or more people take on roles for their
>mutual advantage.

As you can see from this and my previous post, the issue is _not_ one of terminology. You [Martin Taylor (961203 11:40)] told Tracy Harms (1996;12,03.14) that people control one another all the time when they take on roles for their mutual advantage. I am showing (once again) that you're wrong;-)

Best

Rick

Date: Wed, 4 Dec 1996 13:51:10 -0500
From: Martin Taylor <mmt@HERMES.DCIEM.DND.CA>
Subject: Re: Social Control

[Martin Taylor 961204 13:30]
>Rick Marken (961204.1000)]

Your posting makes it quite clear (to me at least) that the issue is only one of terminology, as to whether or not we ordinarily control other people in a mutually satisfactory intraction. Your description of what happens is exactly as I understand it to be. (Including that one may well achieve the perception of having an item by grabbing it off the shelf, as well as by "controlling" the salesperson to get it).

Bruce Gregory (961204.1135 EST)
>>If the salseperson ignores me, I resist the disturbance and ask to speak to
>>his supervisor. You apparently mean something more by control than PCT means
>>by control.

> This occurs only if you do, indeed, want to control the salesperson's actions
> as the means of getting the item. But there are other ways of controlling
> for getting the item _without_ trying to control the salesperson. If the
> salesperson ignores you, you might control for getting the item by just
> reaching for it yourself or asking another sales person for it or leaving for
> a different store. Controlling your perception (of the item) does not
> _require_ that you control your perception of the salesperson's actions.

No, but it's ordinarily a convenient way. Just as getting to work doesn't
require you to control your various perceptions of driving a car. You might
instead control perceptions of riding a bike, or getting a bus, or ...
But controlling the (perception of the result of) the salesperson's actions
is one way of getting the item, and it is the way we ordinarily use. Why
not use the word "control" where it is warranted? To control something
does not require its active non-cooperation. Not if it is a cursor, and
not if it is a person.

> Perhaps a diagram of the situation will help:
>
>   customer
>
>   r (item X)
> |
>   p-->C -->e
> | |
> | v
>   ------------------
>   i   oc ("item X please")
> | |
> di--> qc   d
>   ^ |
> | v
>   d   qs<--di
> | |
>   os(get X)i
>   ------------------
>   ^ |
> | |
>   e<--C<--p
>   ^
> |
>   r ("serve customer")
>
>   server
>
> The dashed lines are person/environment boundaries. The customer is
> controlling a perception of qc, which is having item X in hand. The server
> is controlling for qs, which is giving service on request.
>
> Note that the customer is _not_ controlling the server's actions (os). The
> server's actions are just an influence on (disturbance to) the variable
> controlled by the customer (qc).

True, but in the same sense, one does not control the cursor in a tracking task. One influences the cursor and along with other influences on the cursor, its motions affect one's perception. Whether there is a computer or a human (Wizard of Oz) behind a screen simulating a computer is of no import. One's perceptual control is the same either way.

So I'd say that the customer _is_ controlling the server's actions; not the muscle movements, not the choice of whether to get a ladder to reach the item. not the words the server uses, but the high-level action that implements the server's control of the server's perception of having satisfied the customer's requirements. For if the customer is not perceived to be satisfied, the salesperson's (assumed) reference value for that perception is not met, and the salesperson still has error in the corresponding ECS.

Of course, you can look at it in the other direction if you want, but then (in this assumed situation) the only actions of the customer that are being controlled by the salesperson are those contributing to the salesperson's perception of the state of the customer's satisfaction. (Though one can guess that the salesperson is also controlling for a high perceived value of commissions or sales receipts).

Shall We Dance? Who then controls whom?

Martin

Date: Wed, 4 Dec 1996 14:58:14 -0700
From: Bill Powers <powers_w@FRONTIER.NET>
Subject: Re: Billy Graham and PCT

[From Bill Powers (961204.1410 MST)]

>From Bruce Gregory (961204.1215 EST)

]
>It occurs to me that one way to look at "born again"
>Christianity is that it begins with the message "Look at what a
>mess you have made trying to exercise control in your life. If
>you would only surrender control to an omniscient and
>beneficent controller, things will go much better for you."
>Indeed, for many people the notion that "everything happens for
>a reason" and "God's will be done" seems to dramatically reduce
>the anxiety associated with being an autonomous controller in a
>world filled with other autonomous controllers.

Interesting. Even more interesting is what happens if you turn control over to this beneficent controller and there is no such controller. To what are you turning over the control? And what is doing the turning-over? I think there can be no doubt that born-again people often find peace of mind and happiness and show greatly improved behavior toward others as a result of their experience. We would be foolish to underestimate the reality and the value of this sort of conversion process. It is quite real.

We are, of course, free to offer a different interpretation if we don't accept the God hypothesis. If we accept the phenomenon, but look for a PCT interpretation, I think we can find a plausible one.

I've mentioned before that people seem to operate as if their awareness were

focused on, or participated in, some particular level in their hierarchy of control. Levels both higher and lower operate automatically, out of conscious experience. Of course the level of consciousness changes, and is sometimes more narrowly focused at one level than at other times. But I think that we can see roughly where a person operates by looking at the things that person finds most interesting and worthy of effort. Some people seem to live for logical thinking, not worrying much about why they want to do logic or how (or even if) the results of their logical cogitations are to be accomplished through action. Others might prefer working with shapes, or with movement, or may be interested in dividing the world into events or studying relationships among events and other things. They are little interested in why they do these things, and much more interested in how to do them well or better.

If there are levels in your own organization higher than those at which you most commonly operate, then the reference signals you receive from these higher levels are not themselves objects of attention. Instead, you simply know that some perceptions are in the states they are supposed to be in, and others, occasionally, are "wrong" and require correction. This is particularly true when you find yourself in conflict. Conflicts seem to attract attention to themselves, so you focus down on the warring efforts to the exclusion of other points of view. It doesn't occur to you that each conflicting goal was originally adopted to accomplish something for yourself.

This is how you get your life into a mess. One conflict leads to attempts to let one or the other side win, and that leads to more conflicts, until life seems just a network of things you would do except that you also want or need to do the opposite. Unless you take expert advice from a psychotherapist or a Zen master or a preacher or someone else who has at least a broad idea of what's going on, you will simply dig yourself deeper with each attempt to get out of the hole.

When, under whatever theory you are lucky enough to adopt, you finally discover how to move your awareness up a level or two, the result is just as if you had given up control of those conflicting goals and turned the whole mess over to someone else. The "you" that was down there immersed in the conflicts simply ceases to be, and is replaced by another "you" that is in a position to see that they are conflicts and to change the goals that were causing the conflicts.

If the conflicts are of an intellectual or cognitive sort, as they are for many of us, going up a level takes us into what seems ill-defined territory. We have left behind a rational or calculating way of looking at things and replaced it by fuzzy principles. Since these principles are the level from which we're operating, it seems that they simply exist in the world of logic and lower-level experiences, as givens. And the reasons for choosing one principle instead of another lie at a still higher level which we haven't yet visited (since it was first formed), much less transcended. So it would be easy to attribute the peace of mind which comes from putting aside the intellectual conflicts to a Higher Power -- one level higher, that is, than the level where we're aware. If we grew up under a Christian system concept, then it will be that concept that chooses the principles, whether we're aware of choosing them or not. And attributing our peace of mind to God then goes with the territory.

I prefer the HPCT story, but when you come down to it there's no scientific evidence yet for any of these stories, and if you don't know HPCT you're likely to find some other story to explain the experience. Fine by me, as long as we can agree on the general outline of the experiences we call being

Born Again, or Enlightened, or Fully Functioning, or whatever term in English or Sanskrit you prefer. Whatever is going on, it seems like a good thing to do.

Best,

Bill P.

Date: Wed, 4 Dec 1996 14:58:10 -0700
From: Bill Powers <powers_w@FRONTIER.NET>
Subject: Re: Spider Tricks, Social Control

[From Bill Powers (961204.1345 MST)]

Bruce Gregory (961204.1135 EST)--

> I control my perception of
>the salesperson's actions by asking for something. The fact
>that the salesperson is also a control system limits the ways I
>can control that perception, but that doesn't mean that I only
>_appear- to be controlling. If the salseperson ignores me, I
>resist the disturbance and ask to speak to his supervisor. You
>apparently mean something more by control than PCT means by
>control.

If you want to speak precisely about controlling the behavior of others, break it down into actions, intended consequences, and side-effects. "Behavior" can mean any of these things in different contexts.

If you're controlling the actions of the salesperson, you will say "I would like a pack of gum taken from the top of that stack right there and delivered to me with your right hand while you look me in the face and say thank you nicely." If you're controlling low-level side-effects of the other's actions you will just say "Pack of gum, please." If you're controlling the other's intended consequences of action, you will say (or somehow convey the message) "You'd better try to keep me happy, because I'm a real bastard when I don't get what I want right away."

In all these cases, the control works only if the other person decides to cooperate -- in other words, if that person weighs all the effects of the requested action, side-effect, or intention and finds that no preferences or needs are seriously violated, or that not complying would lead to some serious violation. This applies even if you shove a gun in the salesperson's face and say "Gimme your wallet."

Also, please keep in mind the exact definition of control. Most of the problems in understanding "what PCT says" come from ambiguous uses of language. If you stop to ask what the technical definitions of terms are in PCT, most often you will see the answer right away. But if you use terms like "control" when what you mean is "affect" or "influence," you will have a hard time applying the theory.

A is said to control B if, for every change in B away from the controlled state, A takes action to restore B to that particular state. This is why perceptions don't control behavior. If the behavior changes in some way, the perception does not take action to restore the behavior to a particular state. The perception, by itself, is not a control system and can't control

anything. A perception, or the physical situation on which it depends, can be USED BY a control system to control behavior, because it can be varied in such a way as to make your behavior match some form desired by the other control system, given that doing so causes you no serious inconveniences that would end your cooperation.

Best,

Bill P.

Date: Wed, 4 Dec 1996 17:03:54 -0500
From: Martin Taylor <mmt@HERMES.DCIEM.DND.CA>
Subject: Re: Social Control --a minor correction

[Martin Taylor 961204 17:00]
> Rick Marken (961204.1130)]

It's a strange, but not unfamiliar position to be in, to be saying to Rick "yes I agree with you" and for Rick to be saying "Oh no, you don't".
> So, if the server doesn't have the "correct"
> reference (to serve the customer) the customer cannot control for
> getting item X.

Quite so. There are other situations, but they all involve the server not controlling _against_ the customer.
>
> Martin Taylor (961204 12:10) --
>
> >I think (or rather, I hope) that the issue is one of terminology, not of
> >what is happening when two or more people take on roles for their
> >mutual advantage.
>
> As you can see from this and my previous post, the issue is _not_ one of
> terminology. You [Martin Taylor (961203 11:40)] told Tracy Harms
> (1996;12,03.14) that people control one another all the time when they take
> on roles for their mutual advantage. I am showing (once again) that you're
> wrong;-)

Actually, you are _saying_ I'm wrong, while _showing_ I'm right:-)

Thank you.

Martin

Date: Wed, 4 Dec 1996 14:36:08 -0500
From: Martin Taylor <mmt@HERMES.DCIEM.DND.CA>
Subject: Re: Stat Problem

[Martin Taylor 961204 13:50]
> Bill Powers (961204.0950 MST)]

> What do you mean by degrees of freedom? The number of data points?

Not at all. This is a point I've tried to get across to you in many ways over several years. It was the main technical problem I had with the

"Models and their Worlds" paper in its original version.

In a noise waveform or a filter that has a purely rectangular bandshape of bandwidth W Hz, the number of degrees of freedom is 2WT, where T is the duration of observation. If the bandshape is non-rectangular (as is always the case) an equivalent rectangular bandwidth can be computed. If the objective is to deal with goodness of fit for a control system, the appropriate bandwidth is probably that of the disturbing waveform (unless that is greater than the bandwidth of the perceptual input function or some other element of the control loop). That there are two degrees of freedom means that the value of one measure can be altered arbitrarily without altering the value of the other. In a tracking run with samples every 1/60 second, this is not true. The handle position at sample N will always be close to the handle position at sample N+1. The two samples do not represent separate degrees of freedom.

In the sleep studies, we used 50-second runs and various rectangular bandwidths of disturbance. For the Gaussian disturbances, at a bandwidth of 0.5 Hz the run would have 50 df. For the Uniform disturbances the run would have only 25 df (since half the degrees of freedom in the disturbance were lost in making the amplitudes equal at each frequency when the disturbance was constructed).

However, it can be fruitful to look not only at the disturbance waveform that was presented, but at the disturbance waveforms possible over the bandwidth for which control is possible. Then there could be more degrees of freedom--it would depend on the equivalent bandwidth of the whole control loop--and it would catch any micro-oscillations due to near-critical phase lags around the loop.

The problem you posed is much more interesting, though. You are trying to find a function that projects all the degrees of freedom for the run onto one single degree of freedom in some way that satisfies your intuition as to what constitutes a "good fit." You want one measure. But in different postings, you have identified more than one degree of freedom that matters to you in the deviations between two tracks. It is you, not the automatic (standard, accepted, conventional) analysis technique, who must decide how important these different degrees of freedom are. Is the mean more important than the correlated variation? Do spurious frequencies that occur in the CEV motion but not in the disturbance mean something or should they be ignored? Are you trying to compare within similar models, and look only for the most plausible barameter values, or are you trying to see whether different kinds of model (e.g. with and without time-delays) fit differently? You can't make general _a priori_ statements about the correct analysis methods without saying something about what the different degrees of freedom mean _to you_.

If I understand you well, you want to look at models that all have the same structure, and you want to vary three or four parameter values to see what value set gives the best fit. But you don't know what you mean by "best". I suggest that you have to think about the behaviour of the models, not about the statistical techniques. To do otherwise is analogous to thinking about the actions that affect the world rather than about the perceptions that the actions affect. The models have three degrees of freedom (or four?), and so you cannot profitable look at more than that number of degrees of freedom in the fitting of the data (mean, correlation and amplitude variation might be three, off the top of my head).

Firstly, is it true that small changes in parameter values _always_ generate small changes in model behaviour? I suggest that it is not. There are regions of the parameter space in which the model is not a controller but an oscillator (or so I guess). So, you are interested in parts of the parameter space where the behaviour is at least qualitatively like the data.

Now think about the data. Is it true that your model is intended or expected to account for _every_ variation that happens in the data? In a tracking study, does the model account for moments when the subject looks away or blows his nose? If not, then one has to provide in the model or in the fitting procedure some way to reduce or eliminate the effects of such "extraneous" effects on the data.

So now you have a set of edited data that you think ought to be entirely fitted by your model with the right parameters, if the model in fact describes the experimental subject. Now ask yourself another question: Do you want to know whether _any_ such model is adequate to describe the subject, or do you assume that one is, and want to find the correct parameter values? These are quire different questions, though to answer the first may require optimizing the parameters.

If you want to find whether any model is adequate, rather than assuming that your model is OK in principle, then you really have to have some kind of alternative. In what way might it show its inadequacy? If you don't know, then you have to assume that one degree of freedom is as important as another, and do an RMS deviation as a measure of fit. But if you have some idea about other models or about the significance of different kinds of deviation, then you should not use RMS deviation. For example, if you are concerned about the possibility of whether a better model might incorporate transport lag, you might be interested in phase shift and the gain-frequency curve (are disturbance variations at one frequency more strongly controlled than at another?). So you might analyze the disturbance and the output waveform in frequency and phase, and compare those, rather than looking at mean and mean-square deviation.

I guess what all this long-winded stuff is about is that there is no such thing as a proper criterion for "best fit." It depends on what you are looking for. And that's why I was trying to use a different criterion in looking at the sleep-study tracking results. I assumed that a control system was operating, and tried to compare the model results with those of an unspecified ("randomly chosen") control system that had the same RMS error, to see how much of the data fit could be attributed simply to "control" and how much to the specific characteristics of the fitted model.

I'm sure this leaves you less satisfied than ever with your analysis of the rat data, but I hope it helps you to see some of the issues that get swept under the rug by appeals to "standard" or "conventional" procedures and RMS error measures.

Martin

Date: Wed, 4 Dec 1996 15:01:09 -0800
From: Richard Marken <marken@AEROSPACE.AERO.ORG>
Subject: Re: Social Control

[From Rick Marken (961204.1500)]

Martin Taylor (961204 13:30) --

>But controlling the (perception of the result of) the salesperson's actions
>is one way of getting the item, and it is the way we ordinarily use.

Right. We don't control the behavior (actions or controlled consequences) of the salesperson; we just control one side effect of those actions.

>Why not use the word "control" where it is warranted?

I agree. You were saying that we ordinarily _control_ other people's actions in social interactions. I showed the more common situation where we _don't_ control anything about the person; the person is just part of the feedback path between us and the variable we are controlling

>To control something does not require its active non-cooperation. Not if it
>is a cursor, and not if it is a person.

Correct. Non-cooperation has nothing to do with it. You control when you perceive a variable and act to bring that variable to a reference state and protect it from disturbance. In my diagram, the customer controls item X; he does _not_ control any aspect of the behavior of the server.

Me:

> Note that the customer is _not_ controlling the server's actions (os).

Martin:

>True, but in the same sense, one does not control the cursor in a tracking
>task.

Not true at all. The position of the cursor is unquestionably controlled in a tracking task. The means used to control cursor position (like variations in mouse position) are not necessarily controlled.

>So I'd say that the customer _is_ controlling the server's actions;

Look at the diagram, Martin. The server's actions are _not_ controlled. This is not a verbal thing; it's a fact. The server's actions would _not_ be protected from disturbance by the actions of the customer -- at least not in the situation as I've modeled it. The server's actions are _not_ controlled (see Bill Powers (961204.1345 MST)).

Martin Taylor (961204 17:00) --

>Actually, you are _saying_ I'm wrong, while _showing_ I'm right:-)

You're hallucinating, Martin. Look at the diagram again. What in the world gives you the impression that any of the server's behaviors (actions or controlled consequences) are under control. There is no behavior of the server that is being controlled by the customer. The customer is not controlling the server in any way. If that was your point in saying that people control one another all the time when they take on roles for their mutual advantage then I suppose that I have, indeed, shown that you were right.

Best

Rick

Date: Wed, 4 Dec 1996 15:04:17 EST
From: Bruce Gregory <gregory@CF4.HARVARD.EDU>
Subject: Re: Social Control

[From Bruce Gregory (961204.1500 EST)]

Rick Marken (961204.1000)

> Note that the customer is _not_ controlling the server's actions (os). The
> server's actions are just an influence on (disturbance to) the variable
> controlled by the customer (qc). The customer is "using" the server as part
> of the control loop that allows him to control his perception of item X. The
> customer is _not controlling_ the server: he is not controlling either qs
> (the variable controlled by the server) or os (the server's actions).
>
> If the server does not have a reference to "serve the customer" the customer
> will find that asking the server for item X is an ineffective way to control
> for item X. If the customer is _also_ controlling for seeing a particular
> type of behavior from the server, then he _will_ take action to control
> for that behavior (either os or qs). But it is not a _necessary_ aspect of
> the customer/server relationship. (Note: The di's are disturbances to the
> variables controlled by server and customer other than those produced by the
> actions of the server and customer).
>
> Does this help?

Yup. Thanks.

Bruce Gregory

Date: Wed, 4 Dec 1996 18:21:29 -0500
From: Martin Taylor <mmt@HERMES.DCIEM.DND.CA>
Subject: Re: Social Control

[Martin Taylor 961204 18:15]
> Rick Marken (961204.1500)]

This gets tiresome.
>
> >So I'd say that the customer _is_ controlling the server's actions;
>
> Look at the diagram, Martin. The server's actions are _not_ controlled. This
> is not a verbal thing; it's a fact. The server's actions would _not_ be
> protected from disturbance by the actions of the customer -- at least not in
> the situation as I've modeled it. The server's actions are _not_ controlled
> (see Bill Powers (961204.1345 MST)).

Does the customer not act so that the salesperson delivers the desired item?
Does the customer not say things like "No, the other one" if the salesperson
acts so as to deliver the wrong item? Does the customer not continue to

affect the salesperson's perceptions until the salesperson's actions result in the correct item being delivered (or until a conflict arises with some controlled perception in the salesperson)?

The server's action (not "actions"--the customer presumably doesn't much care _how_ the salesperson delivers the item, so long as it is within customary limits of courtesy) is clearly being controlled by the customer.

>
> Martin Taylor (961204 17:00) --
>
> >Actually, you are _saying_ I'm wrong, while _showing_ I'm right:-)
>
> You're hallucinating, Martin. Look at the diagram again. What in the world
> gives you the impression that any of the server's behaviors (actions or
> controlled consequences) are under control.

See above.

>There is no behavior of the
> server that is being controlled by the customer.

Except the one of interest to the customer--the delivery of the item.

> The customer is not
> controlling the server in any way.

Do you see yet how this is wrong? I sure hope so:-)

Martin

Date: Wed, 4 Dec 1996 16:37:56 -0700
From: Bill Powers <powers_w@FRONTIER.NET>
Subject: Windows 95, Microsoft, and computer progress

[From Bill Powers (961204.1520 MST)]

I need to get a tale of woe off my chest before asking for advice from our wise List members.

For the past two days I've been working on the problem of being able to run Rick Marken's WWW Java programs. Sounds simple enough.

To run those programs, I need a Web Browser that can implement Java. The only ones I know anything about are Netscape Navigator and Internet Explorer (Microsoft's answer to Netscape). I have both of these, but they are both 16-bit versions, and the Java feature is implemented only in the 32-bit versions which run only under 32-bit operating systems, which on a PC means Windows 95 and not Windows 3.11 which I have. They will NOT run with Win32s, as far as I can tell.

So. Having transferred everything from my old machine to the new fast capacious laptop, I took the old machine down to a nice computer store in Farmington, NM, where the Sam gave me Windows 95 and a bunch of cash for it, and even had his Navajo technician named Jarvis install Win95 for me.

Back home, I found that both Internet Explorer and Netscape, the old 16-bit

versions, still worked fine, as did everything else. In fact the multitasking is much better; I had my orbital simulator running while I was downloading the 32-bit version of Netscape from the Web, and it all worked fine.

So, fire up Netscape and try Rick's program, right? Wrong. Netscape fired up all right, but it said it couldn't establish something called a "socket." I had been using Trumpet Winsock, and all I had to do was put it in the path or in the right directory, and both Netscape(16) and IE(16) found it and used it. But not Netscape(32).

I realized that I hadn't removed the 16-bit Netscape as recommended, and being rather intimidated by this time I backed all the way out, removed Windows 95, and started over. The one really nice feature of Windows 95 is that it saves the entire old environment, and when you tell it to go away it removes itself and restores everything the way it was. Pretty much. At least my free disk space went up by 40 Megabytes.

I deleted the old Netscape and reinstalled Windows 95, an hour's process. Reinstalled 32-bit Netscape. Same result. No connection. I fooled around a bit with Win95 to see if I could set it up with its own winsock connection, but didn't know enough about it. Called my Service Provider. Advice: get Internet Explorer(32) and use it to set Win95 up using a built-in Wizard. Two hours of dowloading later I installed IE, and tried it.

This is Microsoft, remember? Sure enough, IE came up running fine, except that of course it couldn't connect to the internet. So I found the recommended program and ran the Wizard, letting it do everything automatically. This entailed another hour of swapping the Win95 installation disks around as requested by the program; disks 1,2,3,7,11,13, 12, 7 again, 8, 10, 12, and 13 (or some such godawful sequence that just went on and on as the afternoon wore on). I was thinking "_I_ was supposed to know how to do _that_?" And I ended up with a screen on which I was invited to subscribe to the Microsoft Network, ATT, and about six other of the biggies, and NO MORE. No way to use the provider I am already using.

Just for fun I tried Netscape (32). It didn't work at all -- I had read rumors that Microsoft has set up IE so it disables Netscape if it finds it. Seems to be true.

After going away and fuming for a while, I remembered one screen where an option of manual installation was offered. I had assumed this was for experts, and it was, but I tried again using that one. This time I ended up with a screen asking for the technical details of my provider's PPP connection, which after a while I found and entered. I entered the ones for Fort Lewis because I can use that one free. Then I waited to be asked for another, so I could also use frontier.net, but that was it. One connection to one provider.

Finally! I entered the phone number and password and fired up IE. Nothing -- connection refused. After trying this unbelievingly for a while, I realized that the new improved program couldn't get past the two-tier logon procedure that Fort Lewis uses, once for the peripheral computer and again for the main one. A script in Trumpet Winsock handled that just fine, but there doesn't seem to be a script facility for either Netscape or IE, or in Win95's network connection. At that point, exhausted and disgusted, and after wasting a long day and a half, I removed Netscape, IE, and Windows 95, reinstalled my 16-bit versions of the browsers, and with a huge sigh of relief got back to where I started. Good old Windows 3.11 looked pretty friendly to me.

As to Windows 95, it's a pain in the neck. Consider trying to run one program, like Eudora, and then dropping out of it to run another one, like my word processor, to pick up an archived file to include in a post. In 3.11, all I have to do is hit alt-tab to get the program manager back, then click on the word processor to do the rest. In Win95, you have to hit the Start button every time, and then trace through four levels of menus to get back to the group of programs you were using before, so as to pick another one. If you find that it's not the one you want, you have to go all the way back to the Start button again. I suppose there are ways around this, like cluttering up the desktop with a lot of shortcut icons, but in my three days with Windows 95 I haven't been inclined to give the programmers any benefit of the doubt. Anyway, this much continuous reorganization HURTS.

Well, for those of you who are still with me, thanks for letting me dump that load of accumulated frustration. I'm presently feeling about Microsoft programmers much the way I feel about lawyers (non-PCT lawyers, that is), but I'm still willing to try one more time if there's anyone out there who can give me a clue about how to read Rick Marken's Java programs, the highest-order goal in this cognitive tree. Or maybe I should just wait for Windows 2001.

Grumpily,

Bill P.

Date: Wed, 4 Dec 1996 16:52:08 -0800
From: Richard Marken <marken@AEROSPACE.AERO.ORG>
Subject: Re: Social Control

[From Rick Marken (961204.1650)]

Martin Taylor (961204 18:15) --

>This gets tiresome.

Learning (reorganization) is not easy.

Me:

> Look at the diagram, Martin. The server's actions are _not_ controlled.

Ye:

>Does the customer not act so that the salesperson delivers the desired
>item?

Yes. There is control of the desired item; NOT of the salesperson's actions or controlled results.

>Does the customer not say things like "No, the other one" if the
>salesperson acts so as to deliver the wrong item?

Yes. There is control of the desired item; NOT of the salesperson's actions or controlled results.

>Does the customer not continue to affect the salesperson's perceptions until

>the salesperson's actions result in the correct item being delivered (or
>until a conflict arises with some controlled perception in the salesperson)?

Yes. There is control of the desired item; NOT of the salesperson's actions
or controlled results.

Again, look carefully at my diagram. The customer does say things that are
a disturbance to a perception that is controlled by the server. But the
customer is not _controlling_ the server's actions or the perception
controlled by the server. The customer does nothing to protect a perception
of the server's actions from disturbance. The server can produce the
desired item for the customer using any actions that will produce that
item. The customer doesn't care what actions the server uses to produce
the item; he only wants the item. If the wrong item is forthcoming the
customer may yell ("No, the other one") and swear ("You stupid dufus")
at the server. But these are just disturbances to the server's controlled
variable (to give good service); they don't control the server's actions or
the server's controlled variable.

>The server's action...is clearly being controlled by the customer.

OK. Let's look at Bill Powers' (961204.1345 MST) definition of control:

>A is said to control B if, for every change in B away from the controlled
>state, A takes action to restore B to that particular state.

So you are saying that for every change in the server's action (os) away
from the controlled state the customer takes action (oc) to restore the
server's action to that particular state. Can you please show me where
that is happening in my diagram of the interaction between customer and
server? Where is the control loop in which the server's action (os) is the
controlled variable.

Me:

> The customer is not controlling the server in any way.

Ye:

>Do you see yet how this is wrong? I sure hope so:-)

No. I don't see how this is wrong. I sure hope you will explain how this
is wrong using my diagram.

Best

Rick

Date: Wed, 4 Dec 1996 19:42:04 +0000
From: Richard marken <marken@AERO.ORG>
Subject: Re: Social Control

[From Rick Marken (961204.1830)]

Well, I hate to do this but:

Me:

> The customer is not controlling the server in any way.

Martin Taylor (961204 18:15) --

> Do you see yet how this is wrong? I sure hope so:-)

Yes! I admit it. I am wrong!

The customer is unquestionably controlling a side effect of the server's actions. So the customer is controlling the server in _some way_. The customer is _not_ controlling the server's action;the customer is not controlling the server's controlled variables. The customer _is_ controlling an immediate side effect of theserver's controlling.

So I think you are absolutely correct. Martin: all cooperative interactions between living control system seem to involve control of _some aspect_ of one person's behavior (action, controlled variable or side effect) by another person.

You know I _hate_ to be wrong, Martin, old man. But at least I can now go to my square dancing class with a clear conscience.

Contritely

Rick

Date: Wed, 4 Dec 1996 21:41:53 -0500
From: Bruce Gregory <bgregory@CFA.HARVARD.EDU>
Subject: Re: Windows 95, Microsoft, and computer progress

[From Bruce Gregory (961204.2140 EST)]

Bill Powers (961204.1520 MST)

Courage. Once you get Win 95 working, you'll love it... trust me. I highly recommend _Windows 95 is Driving Me Crazy: A Practical Guide to Windows 95 Headaches, Hassles, Bugs, Potholes, and Installation Problems_ by Kay Yarborough Nelson, Peachpit Press.

It took me quite a while to get my Internet connections set up, but now everything runs smoothly. I'm using I.E. 3.01 and it runs Rick's JAVA routines very nicely.

Good luck.

Bruce Gregory

Date: Thu, 5 Dec 1996 05:34:02 -0500
From: Bruce Gregory <bgregory@CFA.HARVARD.EDU>
Subject: Re: Windows 95, Microsoft, and computer progress

[From Bruce Gregory (961205.0535 EST)]

Bill Powers (961204.1520 MST)]
.
"If you're using the 32-bit version on something like
Netscape, you'll need a 32-bit Winsock.dll. Window's
95 32-bit Winsock is called Wsock32.dll. That's the one
want to use instead of Winsock.dll. so make sure your
program can find it and it hasn't been renamed
something else. It should be in your Windows 95\
System folder.

"If you get the error message "Netscape was unable
to create a network socket connection (Reason 10047),"
that means you're trying to use the 16-bit .dll (Winsock.dll).
Make sure its not in your path. If Windows 95 encounters
the Trumpet Winsock.dll file, it can crash."

-Windows 95 Is Driving Me Crazy

Bruce Gregory

By the way. Thanks for the very nice posts responding to me
yesterday. I'm taking them very much to heart.

Date: Thu, 5 Dec 1996 14:45:53 +0100
From: "J.A. Blom" <J.A.Blom@ELE.TUE.NL>
Subject: many/one means to an end

[Hans Blom, 961205]

(Martin Taylor 961204 10:30)

>One of the fundamental aphorisms of PCT is "many means to a given
>end."

Two remarks.

1. Yes. Although we control for a lot of simultaneous goals, there
are vastly more things that we don't need to, indeed don't want to
control. Certain things happen "automatically" or "ballistically",
due to the laws of nature or cultural convention, and they don't
require my control; I just count on it that they happen (I have a
good "internal model" of such normally occurring sequences). If I ask
the sales person for a pack of gum, I just want to get the gum and
don't care (within limits) how. By no means would I need to/want to
control _all_ aspects of the sales person's behavior. If I just want
the ball to hit the floor, I just drop it; it would require much more
effort on my part if I also were to control the ball's position from
letting go to impact through time in a particular way. And I do not
want to exert that effort if it serves no goal. Basic economics. And
if the sales clerk becomes nasty, I can go to a different shop.

2. No, if I apply the aphorism to myself. In that case, I can reach a
certain goal only through one path through time. In my imagination I

can try to consider all "solution paths", as AI calls them, from where I am now to where I want to be, in order to discover the "best" one, but reality constrains me to follow one single path. Yet, even though I have to choose one particular means, many of its aspects may still be kind of irrelevant. I can either ride a bicycle or drive a car to get to the university. Sometimes I have a good reason to do either, sometimes it does not matter. But even in the latter case, I _have to_ choose.

So the validity of the aphorism depends upon what we talk about.

By the way, Martin, our (all too brief) conversation on your visit last week prompted me to rethink the issue of a single top-level goal in a hierarchical controller. As I indicated, a single mathematically equivalent top-level goal can always be computed given a number of lower level goals. As you indicated, we might indeed not be able to recognize such a construct as a PCT-type of goal, and its utility may be doubtful for practical purposes. Or maybe not. I'm working on a post.

Greetings,

Hans

End of CSGNET Digest - 4 Dec 1996 to 5 Dec 1996
****

Date: Fri, 6 Dec 1996 08:00:00 -0600
Subject: CSGNET Digest - 5 Dec 1996 to 6 Dec 1996

There are 16 messages totalling 709 lines in this issue.

Topics of the day:

1. Spider Tricks, Social Control
2. Billy Graham and PCT (3)
3. Jehovah's Hans (2)
4. Dark Ages II (2)
5. Social Control (3)
6. Lessons from PCT
7. Reflections on Java (2)
8. Windows 95, Microsoft, and computer progress
9. Ahhh, epistemology at last!

Date: Thu, 5 Dec 1996 15:00:29 +0100
From: "J.A. Blom" <J.A.Blom@ELE.TUE.NL>
Subject: Re: Spider Tricks, Social Control

[Hans Blom, 961205b]

(Rick Marken (961204.0800))

>>In my opinion, the "imagination connection" (alias "world model" or
>>"acquired knowledge") operates ALL THE TIME and AT ALL LEVELS,

>Hans, if you had anything more than an opinion (like some data, for
>instance) I might be interested.

Regrettably, Rick, I function on the principle that there are no facts or truths, just opinions. You'll never get more from me than an opinion. You're not interested in data, I assure you. I could send you lots and lots of data and you wouldn't know what to do with them.

>... based on what we already know from the simple spiral demo, the
>idea that your kind of "model-based control" is working "ALL THE
>TIME and AT ALL LEVELS" appears to exist only in your "imagination
>connection".

Regrettably, Rick, in my theory the "imagination connection" plays a role in anything I (and you :-) perceive: there are no theory-free observations; internal pre-existing knowledge or prejudice always colors the data, and the more so the more someone is convinced of the "factness" of his/her "knowledge". You are a prime example of that ...

Greetings,

Hans

Date: Thu, 5 Dec 1996 15:57:19 +0100
From: "J.A. Blom" <J.A.Blom@ELE.TUE.NL>
Subject: Re: Billy Graham and PCT

[Hans Blom, 961205c]

(Bruce Gregory (961204.1215 EST))

>It occurs to me that one way to look at "born again" Christianity is
>that it begins with the message "Look at what a mess you have made
>trying to exercise control in your life. If you would only surrender
>control to an omniscient and beneficent controller, things will go
>much better for you."

I read this as follows, translated into PCT language. First is the realization that we are frequently pretty bad controllers, who are often confronted with lack of control, especially "unintended side effects" that later show up to seriously interfere with some of our own dearest goals. [In model-based controller terminology that would be: we are confronted with the imperfections of our model and, given an imperfect model, control cannot be perfect either]. In short, we make a lot of errors. That is a fact, although in my view these errors occur mainly at the lower levels of the hierarchy; that we are still alive shows that the higher level goals always remained in control. But that may be just my heretical view ;-).

Second is the realization that these "errors" or "disturbances" are not "bad" but actually do not harm us; they may even be considered benign. Only from such "disturbances" can we learn, after all, and what we learn is that we can, notwithstanding our fears, cope with them (remain in control) if we just let them be. We can even better cope with them if we welcome them as benign, as "the best that could ever happen to us". This "transformation" eliminates fear (of the

unknown, of "error", of "disturbance", of what was earlier thought of as uncontrollable). Not only that: not avoiding disturbances but dealing with them makes better controllers of us, who have a wider span of control.

This explains:

>Indeed, for many people the notion that "everything happens for a >reason" and "God's will be done" seems to dramatically reduce the >anxiety associated with being an autonomous controller in a world >filled with other autonomous controllers.

Yes, this strategy truly works. Reik, one of Freud's lesser known pupils, said: "Love is the absence of fear". If the world and the things in it do not induce fear (of the unknown/"uncontrollable") anymore, we can start to feel their attraction, their positive qualities, their "love" (for now defined as a feeling of being attracted to). Instead of being "disturbed" by the world, we start to feel that we live in harmony with it, that it even takes care of us.

"God" is a reification/personification of the source of this feeling. In the PCT model, both our own actions and "disturbances", often created by other persons, act on the world, and both our actions and these extraneous disturbances create similar perceptual effects. It is thus easy to think that an unseen personal actor creates the "disturbances" that we cannot attribute to a visible person.

I hope I didn't offend anyone with this personal view.

Greetings,

Hans

Date: Thu, 5 Dec 1996 08:04:46 -0800
From: Richard Marken <marken@AEROSPACE.AERO.ORG>
Subject: Jehovah's Hans

[From Rick Marken (961205.0800)]

Hans Blom (961205b) --

>Regrettably, Rick, I function on the principle that there are no >facts or truths, just opinions.

No facts?

>You're not interested in data, I assure you. I could send you lots and >lots of data and you wouldn't know what to do with them.

Try me. Send me some data that requires a model-based control explanation.

>there are no theory-free observations

That's your theory;-)

>internal pre-existing knowledge or prejudice always colors the data

That opinion certainly explains why you spurn the scientific approach to the study of living systems. If your statement above is true, then how do you explain the remarkable success of the scientific method?

Since the comparison of experimental data to model performance (ie. the scientific method) seeme to have no influence on your opinion about how people work, your view of behavior as model-based control is simply a religious convinction. This makes your contributions to this list rather like the hand-outs I get from Jehovah's Witnesses who occasionally come by the house to set me straight about the coming apocalypse.

Opinions are like assholes, Hans. Everybody's got one. If you're not interested in whether or not your opinion (model) is consistent with observation, then please take your hand-outs to the next house; maybe they care about your opinion.

Best

Rick

Date: Thu, 5 Dec 1996 17:20:05 +0100
From: Oded Maler <Oded.Maler@IMAG.FR>
Subject: Re: Jehovah's Hans

[From Oded Maler 961205]

Rick Marken (961205.0800)

(Hans):
>internal pre-existing knowledge or prejudice always colors the data

That opinion certainly explains why you spurn the scientific approach to the study of living systems. If your statement above is true, then how do you explain the remarkable success of the scientific method?

Which sucess, can you remind us?

>From time to time I am astonished by your naive scientificism, Rick. Take the most convincing experimental data that confirms your thesis, or that you use in order to track the so-called controlled variable, and I can easily show you on how many assumptions and prejudice it is built.

In fact I just wanted to say happy Chanukka,

--Oded

Date: Thu, 5 Dec 1996 08:36:52 -0800
From: Richard Marken <marken@AEROSPACE.AERO.ORG>
Subject: Dark Ages II

[From Rick Marken (961205.0830)]

Oded Maler (961205) --

>From time to time I am astonished by your naive scientificism, Rick.

Gosh. It's just what I learned in chem lab;-)

>Take the most convincing experimental data that confirms your thesis,
>or that you use in order to track the so-called controlled variable,
>and I can easily show you on how many assumptions and prejudice it is
>built.

So what? It still confirms my thesis (that variable X is under control).

What, are you and Hans trying to be the vanguard of the new Theocratic Age? Rest assured that you are winning. Welcome to "Dark Ages II: This Time There's No Excuse".

>In fact I just wanted to say happy Chanukka,

I rest my case.

Best

Rick

Date: Thu, 5 Dec 1996 11:45:05 -0500
From: Martin Taylor <mmt@HERMES.DCIEM.DND.CA>
Subject: Re: Social Control

[Martin Taylor 961205 11:15]
>Rick Marken (961204.1830)]

I think we are in agreement. Mostly.

> The customer is unquestionably controlling a side effect of the
> server's actions.

Comment below.

> So the customer is controlling the server
> in _some way_. The customer is _not_ controlling the server's
> action;the customer is not controlling the server's controlled
> variables.

Right. However, the customer is acting as part of the environmental feedback path for one of the salesperson's controlled perceptions: the reference value for this perception is to see the customer as being satisfied. And the customer assumes (perceives, believes) that the salesperson has such a reference value as part of the perception of the person playing the role of "salesperson." What happens when we mistake another customer for a salesperson in a shop? Think about it;-)

> The customer _is_ controlling an immediate side effect of
> theserver's controlling.
>

Now I'm unclear on how you see the situation. And I think this unclarity(!) has nothing to do with the interpersonal situation, but with the relationship among the variables in a control hierarchy. Handing the customer the

item is a means--an action by the salesperson--whose effects propagate through the environmental feedback path (consisting largely of the customer) to influence the perception the salesperson has of the cusomer's level of satisfaction. If that level is the same as the salesperson's reference level for that perception, then the salesperson has no further error and no further action is required. An observer would say that the transaction has been completed.

Now, is an action that is a (partial) means of bringing a perception to its reference value a side-effect? Ordinarily, one would (or at least I would) say "no." My use of the term "side-effect" is an effect on the environment that does not influence the controlled perception, at least not within the time-scale of the control loop's normal action. (There's a technical issue here, relating to stability and delayed effects, but I don't want to further "declarify" the issue by going into it).

As I would use the term, the provision of the item to the customer is an influence on the customer's controlled perception (reference level: to have the item) and a part of the environmental feedback path for the salesperson's controlled perception (reference level: to see the customer satisfied). And I think that's the way you drew it, unless I badly misread your diagram. And I wouldn't call that a side-effect of the salesperson's control. But you might, and I might be persuaded to change my usage, given adequate reason.

> So I think you are absolutely correct. Martin: all
> cooperative interactions between living control system seem
> to involve control of _some aspect_ of one person's behavior (action,
> controlled variable or side effect) by another person.

Sorry, but I'd rather replace "all" in the first line by "one class of". There are cooperative interactions of a quite different kind that don't have this characteristic, whether you use the term "side-effect" or not.

The phrase "pouring oil on troubled waters" characterizes another class, in which the actions of one ECU reduce the coupling between the disturbing variable and the CEV of another ECU, and vice-versa in a reciprocal arrangement (or in a network of many ECUs). In this class, the important interactions may well be by way of true side-effects. Each member of the cluster controls more readily by virtue of the existence of other members of the cluster, but there's no need for any of them to have overlapping CEVs (i.e. their actions don't directly influence each other's perceptions).

Yet another class (rare, I think) occurs when one person physically manipulates another in ways that do not disturb controlled variables; for example "Let me show you a better way to hold this golf club/tennis racquet/bowling ball". There may be yet more classes, including ones in which the controlled variable is indeed a true side-effect of the actions of another person.
>
> You know I _hate_ to be wrong, Martin, old man. But at least
> I can now go to my square dancing class with a clear conscience.
>
Very happy for you. Who doesn't hate to be wrong? It's a disturbance to an important controlled perception in most people (including me, and you say, you). But clearing the conscience is a statement of error reduction in an even more important controlled perception, isn't it. And isn't it a nice feeling?

Martin

Date: Thu, 5 Dec 1996 16:58:24 EST
From: Bruce Gregory <gregory@CF4.HARVARD.EDU>
Subject: Lessons from PCT

[From Bruce Gregory (961205.1700 EST)]

In the country of the blind, the one-eyed man is ignored.

Bruce Gregory

Date: Thu, 5 Dec 1996 15:17:00 -0800
From: "Marken, Richard S." <Marken@COURIER3.AERO.ORG>
Subject: Reflections on Java

[From Rick Marken (961205.1520)]

This may not be the best time (given Bill's problems with Win95) but I'd like to make some comments about the Java demos. First, I'd like to thank everyone who did try them and gave me feedback. I've learned that Java is not nearly as "platform independent" as one might hope -- especially when it comes to interactive animations. I hope that those of you who tried them will keep going back to them occasionally. I plan to revise (and, hopefully, improve) the demos regularly and (_slowly_) add new demos to the collection. The demos can currently be accessed by going directly to

http://www.leonardo.net/Marken/demos.html

The main thing I've learned so far (to my despair;-)) is that these demos run somewhat more smoothly on a PC than on a Mac. This has been my experience here at work too. Even on a fast Mac (Martin used a 200MHz Power PC I believe) there are brief "freezes" or "jags" where the animation seems to skip ahead suddenly. A slow Mac (Gary Cziko used an LC I believe) will work (I'm amazed) but the quality is very poor. I use a 120MHz Power PC and the animation is a bit rough but not that bad. The jaggyness of the animation is worst (on the Mac) when the programs are run in Netscape; the programs can be run as stand alone applications and they run quite a bit more smoothly that way. So the problem on the Mac seems to be with Netscape and/or with the way Apple does the network layer. Maybe things will improve or the Mac will get so fast that these problems will no longer be noticeable.

The feedback from PC users is generally good. I think the programs look better on the faster Pentium processers but I'm not sure. There are a couple of folks out there using Suns. I think the demos run pretty well on these machines but there were some problems with the HTML for the "Test for the Controlled Variable" demo as originally written. I have no idea what the problem was but I changed the HTML and everything seems to be OK now (is that right Rupert?).

Although faster machines seem to improve the animation they also make the control tasks more difficult. Java lets you put in a time delay between each animation iteration, which should determine the _fastest_ possible

refresh rate (and the greatest degree of difficulty of the control task); if this is true (and I'm not sure it is yet) then timing of these programs should be close to standard when 1) everyone is on minimally fast computer (say a 166 Pentium or better) or when browsers (like Netscape) come equipped with a _just in time_ Java compiler (so that computational overhead during each animation cycle is minimum).

I am trying to design the demo displays so that it is possible to read about the demo _while_ it is happening. This means that the demo displays have to be designed to fit the minimal screen size. When you try these demos I would appreciate any suggestions about better ways to display them. I would also appreciate suggestions for improving the existing demos or for developing new demos and experiments.

Best

Rick

Date: Thu, 5 Dec 1996 10:44:59 -0800
From: Richard Marken <marken@AEROSPACE.AERO.ORG>
Subject: Re: Social Control

[From Rick Marken (961205.1100)]

Me:

> The customer _is_ controlling an immediate side effect of the server's
> controlling.

Martin Taylor (961205 11:15) --

>Now I'm unclear on how you see the situation... is an action that is a
>(partial) means of bringing a perception to its reference value a side-
>effect?

>As I would use the term, the provision of the item to the customer is
>an influence on the customer's controlled perception...And I wouldn't call
>that a side-effect of the salesperson's control. But you might, and I
>might be persuaded to change my usage, given adequate reason.

I came to my realization that the customer was actually controlling a side-effect of the saleperson's actions when I thought about the interaction in terms of the "rubber band demo". Provision of the item to the customer is a control action just as movement of the rubberband by the subject is a control action. In both cases, the controller (experimenter or customer) is controlling a _side effect_ of actions. A side effect of being handed item X is the customer's perception of that item; a side effect of pulling on the rubber band is the experimenter's perception of the movement of the subject's finger. In both cases, it is a _side effect_ of control actions (perception of item X, perception of finger movement) that is controlled (by the experimenter and the customer, respectively).

These side effects are strongly or weakly linked to the control actions; but they are _not_ the control actions themselves. This can be seen by having the same actions produce different side effects. For example, suppose you are controlling your perception of the position of the subject's finger in the rubber band demo and you want to move the

subject's finger so that the tip of the finger cover's an X on the table. The actions that accomplish this result (from your perspective) are different depending on where you are located relative to the finger and the X. If you are looking from above then the finger must be directly over the X; if your are looking from the left, the finger must be slightly to the left of the X, etc. So when you control for the perception of "finger on X" you are not controlling for particular actions from the subject; you are controlling for particular _side effects_ of those actions (from your perspective).

In the case of the salesperson, handing you item X is a control action (controlling the salesperson's perception of being of service); but the result of this action, from your perspective, is a side effect; and it is this side effect that you are controlling.

Actually, I think we can _only_ control side effects of control actions since we can only see those actions from our perspective. The difference between "controlling actions" vs "controlling side effects" is really a matter of degree. I suppose we would say we are "controlling actions" when actions are very tightly linked to the perceptions we are controlling (as they are in the case of controlling finger position in the rubber band demo) and that we are "controlling side effects" when actions are very weakly linked to the perceptions we are controlling (as is often the case in sales transactions where our main interest is in getting a look at item X, regardless of how the salesperson gets it to us).

Best

Rick

Date: Thu, 5 Dec 1996 13:37:19 EST
From: Bruce Gregory <gregory@CF4.HARVARD.EDU>
Subject: Re: Billy Graham and PCT

[From Bruce Gregory (961205.1335 EST)]

Hans Blom, 961205c

> "God" is a reification/personification of the source of this feeling.
> In the PCT model, both our own actions and "disturbances", often
> created by other persons, act on the world, and both our actions and
> these extraneous disturbances create similar perceptual effects. It
> is thus easy to think that an unseen personal actor creates the
> "disturbances" that we cannot attribute to a visible person.
>
> I hope I didn't offend anyone with this personal view.

Not me. Thanks.

Bruce Gregory

Date: Thu, 5 Dec 1996 13:40:48 EST
From: Bruce Gregory <gregory@CF4.HARVARD.EDU>
Subject: Re: Dark Ages II

[From Bruce Gregory (961205.1340 EST)]

Rick Marken (961205.0830)
>
> >Oded Maler (961205) --

> >From time to time I am astonished by your naive scientificism, Rick.
>
> Gosh. It's just what I learned in chem lab;-)

We naive scientificists have got to stick together. You can
count on me, Rick....

Bruce Gregory

Date: Thu, 5 Dec 1996 14:37:51 -0700
From: Bill Powers <powers_w@FRONTIER.NET>
Subject: Re: Windows 95, Microsoft, and computer progress

[From Bill Powers (961205.1425 MST)]

>From Bruce Gregory (961205.0535 EST)--

>"If you're using the 32-bit version on something like
>Netscape, you'll need a 32-bit Winsock.dll. Window's
>95 32-bit Winsock is called Wsock32.dll. That's the one
>want to use instead of Winsock.dll. so make sure your
>program can find it and it hasn't been renamed
>something else. It should be in your Windows 95\
>System folder.
>
>"If you get the error message "Netscape was unable
>to create a network socket connection (Reason 10047),"
>that means you're trying to use the 16-bit .dll (Winsock.dll).
>Make sure its not in your path. If Windows 95 encounters
>the Trumpet Winsock.dll file, it can crash."
>
>   -Windows 95 Is Driving Me Crazy

That was the very thing I needed -- actually I went looking for a 32-bit
winsock last night, found it, downloaded it this morning when the net was
calm, and got it running. I've looked on Rick's Java program and found it
good. Also, it turns out that the new Trumpet Winsock contains instructions
for connecting to different servers, too.

Sounds like a must-have book; thanks.

Best,

Bill P.

Date: Thu, 5 Dec 1996 14:47:02 -0800
From: Richard Marken <marken@AEROSPACE.AERO.ORG>
Subject: Re: Social Control

Here is a repost of earlier reply to Martin Taylor that seems to have become lost in cyberspace.

[From Rick Marken (961205.1100)]

Me:

> The customer _is_ controlling an immediate side effect of the server's
> controlling.

Martin Taylor (961205 11:15) --

>Now I'm unclear on how you see the situation... is an action that is a
>(partial) means of bringing a perception to its reference value a side-
>effect?

>As I would use the term, the provision of the item to the customer is
>an influence on the customer's controlled perception...And I wouldn't call
>that a side-effect of the salesperson's control. But you might, and I
>might be persuaded to change my usage, given adequate reason.

I came to my realization that the customer was actually controlling a side-effect of the saleperson's actions when I thought about the interaction in terms of the "rubber band demo". Provision of the item to the customer is a control action just as movement of the rubberband by the subject is a control action. In both cases, the controller (experimenter or customer) is controlling a _side effect_ of actions. A side effect of being handed item X is the customer's perception of that item; a side effect of pulling on the rubber band is the experimenter's perception of the movement of the subject's finger. In both cases, it is a _side effect_ of control actions (perception of item X, perception of finger movement) that is controlled (by the experimenter and the customer, respectively).

These side effects are strongly or weakly linked to the control actions; but they are _not_ the control actions themselves. This can be seen by having the same actions produce different side effects. For example, suppose you are controlling your perception of the position of the subject's finger in the rubber band demo and you want to move the subject's finger so that the tip of the finger cover's an X on the table. The actions that accomplish this result (from your perspective) are different depending on where you are located relative to the finger and the X. If you are looking from above then the finger must be directly over the X; if your are looking from the left, the finger must be slightly to the left of the X, etc. So when you control for the perception of "finger on X" you are not controlling for particular actions from the subject; you are controlling for particular _side effects_ of those actions (from your perspective).

In the case of the salesperson, handing you item X is a control action (controlling the salesperson's perception of being of service); but the result of this action, from your perspective, is a side effect; and it is this side effect that you are controlling.

Actually, I think we can _only_ control side effects of control actions since we can only see those actions from our perspective. The difference between "controlling actions" vs "controlling side effects" is really a matter of degree. I suppose we would say we are "controlling actions"

when actions are very tightly linked to the perceptions we are controlling (as they are in the case of controlling finger position in the rubber band demo) and that we are "controlling side effects" when actions are very weakly linked to the perceptions we are controlling (as is often the case in sales transactions where our main interest is in getting a look at item X, regardless of how the salesperson gets it to us).

Best

Rick

Date: Thu, 5 Dec 1996 17:25:50 -0700
From: "T. B. Harms" <tbh@TESSER.COM>
Subject: Ahhh, epistemology at last!

[Tracy Harms 961205.17]

Hans Blom, 961205b corresponds with Rick Marken (961204.0800), (961205.0800)

I am intrigued. I, too, think that "there are no theory-free observations; internal pre-existing knowledge or prejudice always colors the data". This has me inclined to chip in with Hans Blom. However, he also says "I function on the principle that there are no facts or truths, just opinions." That throws me far in an opposite direction.

But much of the discussion here seems to be around the "imagination connection". My impression is that Rick understands this to mean that there is something in the control system like Figure 15.3 of B:CP (with the Immagination Connection diagrammed as a switching system changing the reference signal to a comparator).

I'm not so sure what Hans thinks, but I will say this: If you think, Hans, that the absence of theory-free observation implies the presence of control-system structure of this sort "AT ALL LEVELS", your interpretation is incorrect. If this is what you think, you have misunderstood the meaning of the proposition that all observations are theory impregnated. At least, your treatment is contrary to the writings on this idea which have influenced me.

In regard to this tenet of epistemology, a control system itself counts as "a theory." There is no theory-free observation because there is no observation which does not involve active interpretation with pre-existing bias--such as even the most rudimentary control system embodies.

Tracy Bruce Harms tbh@tesser.com
Boulder, Colorado

"How odd it is that anyone should not see that all observation must be for or against some view if it is to be of any service!"
Charles Darwin

Date: Thu, 5 Dec 1996 20:17:22 -0500
From: GANN Trey <TREYGANN@AOL.COM>

Subject: Re: Billy Graham and PCT

MAYBE IF WE REORGANIZE OUR HIERARCHY THEN GOD AND HIS WILL CAN BE THE HIGHEST LEVEL OF REFERENCE AND THAT IS WHEN WE GREATLY REDUCE OUR ANXIETY AS GREGORY PUT IT. BUT THEN MAYBE WHEN GOD AND HIS WILL ARE NOT AT THE TOP LEVEL OF HIERARCHICAL CONTROL THINGS GET MESSED UP. AND MAYBE OUR SOCIETY OF CONTROL SYSTEMS ARE NOT INCORPORATING GOD OR HIS WILL INTO THEIR HIERARCHY OF CONTROLLED PERCEPTIONS AND THEREFORE WE HAVE TO DEAL WITH THEIR ANTI-SOCIAL BEHAVIOR THAT WE SEE.

I AM A COUNSELOR. IT IS AMAZING WHAT OUR YOUNG ONES ARE DEALING WITH. A NINTH GRADER TOLD ME THAT A SIXTH GRADER THREATENED HIM WITH GUNS. WHAT HAPPENED TO THE PLAYGROUND FIGHTS?

I AM GREATLY CONCERNED WITH THE WAY THINGS ARE GOING. DON'T WANT TO ARGUE ABOUT RELIGION. BUT THAT IT IS INTERESTING TO LOOK AT HOW A GREATER CONTROL SYSTEM THAN US CAN INLUENCE/GUIDE US IF WE LET IT. GOD SEEMS TO YIELD SELF-CONTROL TO US AT THIS POINT IN TIME. WHAT ARE WE GOING TO DO WITH OUR SELF-CONTROL. WHAT EXPERIENCES WILL INDIVIDUAL CONTROL SYSTEMS CONTROL FOR? WILL THEY CONTROL FOR PROSOCIAL OR ANTI-SOCIAL EXPERIENCES? IT APPEARS TO BE UP TO EACH ONE OF THEM.

THANKS GREGORY FOR RAISING THESE QUESTIONS AND MY REACTIONS. TAKE CARE EACH OF YOU AND HAVE A GOOD WEEKEND.

TREY GANN; COUNSELOR IN TYLER, TEXAS

Date: Fri, 6 Dec 1996 12:11:23 +0000
From: Rupert J Young <eep1ry@EE.SURREY.AC.UK>
Subject: Re: Reflections on Java

[From Rupert Young (961206.1200 UT)]

(Rick Marken (961205.1520))

> I have no idea what the
> problem was but I changed the HTML and everything seems to be OK now (is
> that right Rupert?).

Yep, working fine now.

--
Regards,
Rupert

End of CSGNET Digest - 5 Dec 1996 to 6 Dec 1996
****

Date: Sat, 7 Dec 1996 08:00:30 +1800
Subject: CSGNET Digest - 6 Dec 1996 to 7 Dec 1996

There are 17 messages totalling 922 lines in this issue.

Topics of the day:

1. Observation, Theory, God (3)
2. Social Control (2)
3. Epistemology -- again (4)
4. Jehovah's Hans
5. Infinite Potential
6. perception, observation, experience, etc. (2)
7. Side effects (3)
8. Volitional Action update

Date: Fri, 6 Dec 1996 08:28:57 -0800
From: Richard Marken <marken@AEROSPACE.AERO.ORG>
Subject: Observation, Theory, God

[From Rick Marken (961206.0830)]

Tracy Harms (961205.17) --

>I, too, think that "there are no theory-free observations; internal pre-
>existing knowledge or prejudice always colors the data".

I'm not much of a philsopher of science but it seems to me that my observations are simply my observations. My observations -- what I see, hear, taste, feel, smell, imagine, fear, etc -- are my perceptual experiences, which simply are there. I am often simulataneously making mental _interpretations_ of these observations, either formally (as when I interpret observations of control phenomena in terms of a control model) or informally (as when I interpret my inability to open the door with my old house key - - wrong key, broken lock, alternative universe, etc). But the observations _themselves_ (of the key, the effort, the anger and confusion) are simply there, uninterpreted.

>This has me inclined to chip in with Hans Blom.

You'll snap out of it;-)

>However, he also says "I function on the principle that there are no facts
>or truths, just opinions." That throws me far in an opposite direction.

Now you're talkin'.

>There is no theory-free observation because there is no observation which
>does not involve active interpretation with pre-existing bias--such as even
>the most rudimentary control system embodies.

I think the idea that "there is no theory-free observation" is itself a theory based on several sets of observations, the main ones being 1) that observations occur in the brain and 2) that there is a sensory interface between the brain and what is observed. I agree with this basic picture; observations (perceptions) are _representations_ of external reality; they are not the external reality itself. But this does not mean that there is anything _theoretical_ about the observations themselves.

For example, right now I am observing the words on the screen. This (non-verbal) experience is as true as it can be. Even if you could show that the words are "really" just little dots, or collections of photons, or the result of direct electrical stimulation of my brain or whatever; nevertheless, my observation of the words is what it is; I'm seeing them

right now; that's what I observe.

The question of _theory_ comes in when we try to _explain_ what we observe. Theories or models are themselves perceptions that we use to explain collections of other perceptions (observations) and (most important) to explain how our actions will affect what we observe.

Some theories can explain all of what we observe and accurately predict what we will observe when we take certain actions (like when we ignite a booster engine under a Saturn V). These are the theories that we consider correct (until observations are made that require revision). My current theory of what I am seeing on the screen is that these words are collections of pixels, the locations of which are mapped in the computer's memory, etc. I can _test_ this explanation by taking actions that should affect these perceptions in various ways if the theory is correct; I can write programs, for example, that should change the shapes and locations of the letters in the words by changing the contents of memory.

This is my idea of what science is about. It's a systemactic way of taking actions in order to test theories (models) of observations to see if those actions have the expected effect on what is observed.

It seems to me that Hans Blom (961205b) was rejecting the entire scientific enterprise by saying that "it's all opinion". Yes, it is all opinion, but science has made it possible to show that some opinions (like the opinion that behavior is the control of perception) are better than others (like the currently prevailing opinion that perception is the control of behavior).

I found Hans' post particularly annoying because 1) I think scientific test of opinion (theory) is _extremely_ important; otherwise we're back to the use of force to determine who's opinion is best and 2) because Hans continues to carry on about model- based control as though this were some important contribution to our understanding of purposeful behavior despite the fact that there is _no_ evidence that model-based control plays much of a role in behavior _and_ despite the fact there is now evidence (the Vancouver experiment) that model- based control plays _no_ role in purposeful behavior at all.

People (like Hans) who continue to defend beliefs (theories) in the face of evidence that contradicts those beliefs happen to give me the creeps.

Bill Powers (961205.1425 MST)

>I've looked on Rick's Java program and found it good.

And the morning and the evening were the first day.

Halleluja;-)

Best

Rick

Date: Fri, 6 Dec 1996 11:59:52 -0500
From: Martin Taylor <mmt@HERMES.DCIEM.DND.CA>
Subject: Re: Social Control

[Martin Taylor 961206 10:50]
> Rick Marken (961205.1100)]

The location of misunderstanding becomes slowly clearer.

> I came to my realization that the customer was actually controlling a side-
> effect of the saleperson's actions when I thought about the interaction in
> terms of the "rubber band demo".

I'm not sure this is a good metaphor. But I'll stick with it for the rest of this posting, anyway. In the rubber-band demo, the subject does not even need to know that the experimenter exists. The only perception being controlled is that of the deviation of the knot from the marker point. The "side-effect" of tracing an "X," or whatever the experimenter wants, is a means to keeping the knot over the mark. And perhaps should not be called a "side-effect" (as discussed at puzzled length below).

> ... A side effect of being handed item
> X is the customer's perception of that item; a side effect of pulling
> on the rubber band is the experimenter's perception of the movement of the
> subject's finger. In both cases, it is a _side effect_ of control actions
> (perception of item X, perception of finger movement) that is controlled
> (by the experimenter and the customer, respectively).

In the case of the sale, it seems to me that the controlled perception in the salesperson of most interest is that of perceiving a satisfied customer. Handing the item is a means to that end, rather as tracing the X is a means to keeping the rubber-band knot over the mark. Now you say that tracing the X is a side-effect, and in this usage, handing the item is indeed a side-effect.

So the question at issue is whether the words "side effect" are appropriate to apply to the situation. I'm not sure, and as I said before, I'm willing to be persuaded. But I'm not yet persuaded.

Let's consider. If the subject traced something other than "X", or the salesperson handed over a different item, the primary controlled perception would not come closer to its reference value. So, the "X" and the handing over of the correct item are essential components of the environmental feedback path on this particular occasion. To me, that rules them out as side-effects.

On the other hand, it is clear that "X-ness" and this particular item are not essential components of the feedback path for the situation in general. They are essential on this occasion only. What is essential depends on the controlled perceptions of the other party (experimenter or customer) on the particular occasion. The other party controls for a perception of "subject tracing X" or of "salesperson handing me that specific item," and to the other party the subject's (salesperson's) controlled perception is of no interest, except that its control is an essential component of the environmental feedback path for the other party.

If the subject were not controlling for keeping the knot over the mark, or the salesperson were not controlling for perceiving a satisfied customer, then neither the experimenter nor the customer would have a useable environmental feedback path. In that sense, then, the subject's/salesperson's actions in control of their perceptions are not side effects, even though, in themselves their specific natures

are irrelevant. The experimenter/customer relies on their _being controlled_.

Conflict arises in PCT when the actions of one ECU affect some aspect of the environment that is perceived by another ECU. Note that when we are talking about ECUs, what is perceived is a _one-dimensional_ (scalar) variable, and it is _necessarily_ a controlled variable unless the loop gain is zero. In a conflict, this means each is trying to move some aspect of the environment, but to a different place. However, if the CEVs for these two ECUs are merely correlated, but not identical, it is often possible for each to bring its perceptual variable to the reference value eventually, despite the mutual disturbance. The conflict may subside as each ECU uses other aspects of the environment to bring its perception to the reference value (without going up a level, note). We can then talk about the two ECUs acting as disturbances on one another through the coupling of the non-orthogonal CEVs. Is this what is happening in the rubber band or the sale? I don't think so. I think we have neither conflict nor mutual disturbance.

Now, what are side-effects? I tend to look at the world environment as a space of high dimensionality. The CEV defines some line or one-dimensional curve in this space, specified by the Perceptual Input Function of an ECU. The output of an ECU is distributed among various acts that affect the environment, but, because there is only a single scalar output value, the effect of the output on the world is also along a single line or one-dimensional curve. The environmental feedback function is the projection of the output curve onto the CEV curve, and side-effects are the portion of the output curve that remains after the projection onto the CEV curve is removed. They do not affect the CEV in any way.

Conflict and mutual disturbance relate to the projections of one output curve onto the CEV curve of another ECU. And this includes both direct effects (the projection of one CEV curve onto the other, which represents conflict) and side-effects (which, added to the direct effects, yield the total disturbance effect).

Looking at the situation from this viewpoint, I can't see the "X" control or the "handing the item" control as side effects. They define the way that the output curve projects onto the CEV for the same ECU at the time of interest. That on another occasion the traced shape might be "O" or the item different merely represents a change in the projection. But these differences don't represent conflict. Of course, the actions of the experimenter do disturb the controlled perception of the subject, which is why the subject acts in the first place, and when the experimenter is controlling for the subject's tracing an "X", the effect of the experimenter's actions on the position of the knot _are_ side-effects that disturb. But the subjects actions in bringing the knot over the mark are _not_ side-effects either from the subject's or from the experimenter's point of view. They _are_ the CEV for the experimenter, and they are control actions for the subject.

In this sense, then, so long as the subject sustains a reference for perceiving the knot to be over the mark, the experimenter _controls_ the subject's actions, and those actions that are controlled are not side-effects of anything.

>
> These side effects are strongly or weakly linked to the control actions;
> but they are _not_ the control actions themselves.

Yes. I think that's a more straightforward way of saying what I tried to

put in semi-technical language above. The "control actions themselves" are the projection of the output onto the CEV. The "side-effects" are the rest of the influence of the control output on the environment.

>
> Actually, I think we can _only_ control side effects of control actions
> since we can only see those actions from our perspective.

This wording is obscure. Our own actions, we cannot see (except from some other ECU). The actions of the other, we can see, whether or not those actions are controlling actions or side-effects from the viewpoint of the other. Hence we can _try_ to control them either way, side-effect or not. We will not succeed in controlling them (expect through overwhelming force) if they are controlling actions, unless--and this is important-- out actions in controlling them are part of the environmental feedback path that they are using. And that might be through either our controlling actions or through side-effects.

That "unless" matters, because if our actions are independent of the actions of the other, then it is almost certain (in the probability theory sense) that our actions on average influence the other's CEV so as to increase the error. But if our actions are part of the other's environmental feedback path, this is by no means assured. It depends on gain and phase relationships and can be analyzed (in principle) for any particular coupling.

> I suppose we would say we are "controlling actions"
> when actions are very tightly linked to the perceptions we are controlling
> (as they are in the case of controlling finger position in the rubber
> band demo) and that we are "controlling side effects" when actions are
> very weakly linked to the perceptions we are controlling (as is often the
> case in sales transactions where our main interest is in getting a look at
> item X, regardless of how the salesperson gets it to us).
>
As you can see, I'm still not sure whether the term "side-effects" is properly employed in either case. Maybe a new term is required for the situation were the actions of each party for part of the environmental feedback path for the perceptions of the other, and where the two CEVs are orthogonal. In one sense "side-effect" is perfectly proper, and in another sense it isn't.

Anyway, thanks for the explanation. And I hope that this also helps to explicate my continued puzzlement. And the possible obtuseness and obscurity of the writing above. I'm puzzling through what may be perfectly clear to you, and puzzled writing isn't necessarily clear writing.

Martin

Date: Fri, 6 Dec 1996 12:11:35 -0500
From: Bill Leach <b.leach@WORLDNET.ATT.NET>
Subject: Epistemology -- again

<Bill Leach 961206,1130

>Rick Marken (961205.0800)
>Hans Blom (961205b)
>[From Oded Maler 961205]

I don't know why these epistemological issues keep resurfacing about once or twice a year...

Han's (and of course Oded) are likely correct in the same sense that Kant's critique of pure reason is likely correct. However, even Kant perceived that such a philosophy is useless when actually doing anything.

As has been stated here many times before:

Yes there are axiomatic assumptions (often implicit) in any discourse, research, or other activity legitimately qualifying for the use of the term "scientific".

Probably most gemain to the discussions(?) between Rick and Hans are (in my opinion of course):

1. There is a "real", "Concrete", world "out there".

2. That world imposes restrictions upon the state and or changes in state of items of sensory perception independent of the observer.

3. These restrictions are consistent and reliable (even when perceptual observation appears to deny that they are).

4. It is possible to learn useful information about these "Natural Laws" by employing what is called the "scientific method".

All of the above are axiomatic (religious beliefs if you prefer). Basically if someone does not believe the fundamental assumptions that collectively are used for the scientific method then such a person can not constructively carry on a discourse concerning any "scientific matters". Even the science of Epistemology itself relies upon some of the assumptions fundamental to "natural science".

I am reminded of some of the "religious" debates that have occurred here in the past. Such debates are a total waste of time and energy. Discussions are possible and can be quite enlightening if either party is willing to "suspend their belief" or at least suspend the attempt at defense of their belief and proceed to explore the possible implications of the two (or more) belief systems and their relationships.

I suppose that what I should have said to make things brief is that either people agree upon the "rules of discourse" or "rules of scientific investigation" BEFORE they engage in debate or they will never be talking about the same subject.

-bill leach

Date: Fri, 6 Dec 1996 12:26:53 -0500
From: Bill Leach <b.leach@WORLDNET.ATT.NET>
Subject: Re: Jehovah's Hans

<Bill Leach 961206,1210
>[From Oded Maler 961205]

Rick Marken (961205.0800):
That opinion certainly explains why you spurn the scientific approach to the study of living systems. If your statement above is true, then how do you explain the remarkable success of the scientific method?

Oded Maler:
Which sucess, can you remind us?

Oh! Come on Oded! Are you another one of "those" that confuses both the "harm" and "good" accomplished by employing technology with the nature of technology itself?

Oded Maler:
>From time to time I am astonished by your naive scientificism, Rick. Take the most convincing experimental data that confirms your thesis, or that you use in order to track the so-called controlled variable, and I can easily show you on how many assumptions and prejudice it is built.

Since it is easy, why don't you list at least a few of those assumptions that you believe that Rick is making that are not consistent with or are not a necessary part of scientific research.

Though you may not care to do this, to convince me (and probably quite a few other people here on CSGNET) you will need to do a reasonable job of showing that the assumption you are citing IS necessary for Rick's research and data analysis to be logically valid AND that such an assumption is either inconsistent or unnecessary for "proper" application of the scientific method.

-bill leach

Date: Fri, 6 Dec 1996 11:42:03 -0700
From: "T. B. Harms" <tbh@TESSER.COM>
Subject: Re: Epistemology -- again

>From Tracy Harms 1996;12,06.11:30

Bill Leach 961206,1130

>I suppose that what I should have said to make things brief is that
>either people agree upon the "rules of discourse" or "rules of
>scientific investigation" BEFORE they engage in debate or they will
>never be talking about the same subject.

But this must certainly be false, or there would be no way to account for the presence of scientists today, whereas not long ago there weren't any to speak of, nor could one understand how an inarticulate baby could become a great researcher. People can come to talk about the same subject without prior agreement because people can change their mind in the course of discussion. That is one of the great values of inquiring conversation.

Indeed the necessity of understanding learning (for the individual) and the improvement of knowledge (for the culture) are a large part of why

epistemology is of ongoing interest and importance.

Tracy Bruce Harms tbh@tesser.com
Boulder, Colorado

"How odd it is that anyone should not see that all observation must be for or against some view if it is to be of any service!"

Charles Darwin

Date: Fri, 6 Dec 1996 10:35:39 -0500
From: Bruce Gregory <bgregory@CFA.HARVARD.EDU>
Subject: Infinite Potential

[From Bruce Gregory (961206.1035 EST)]

I am reading David Peat's _Infinite Potential: The life and times of David Bohm_. Bohm was a maverick physicist that may remind you of someone we know ;-) Peat writes very well and knows his physics. I recommend that you take a look at the book.

I have not always been a fan of Bohm's, but like Rick, I am getting more mellow as I grow older...

Bruce Gregory

Date: Fri, 6 Dec 1996 14:19:34 -0500
From: Bruce Gregory <bgregory@CFA.HARVARD.EDU>
Subject: Re: Observation, Theory, God

[From Bruce Gregory (961206.1420 EST)]

Rick Marken (961206.0830)
>
> Tracy Harms (961205.17) --
>
> >I, too, think that "there are no theory-free observations; internal pre-
> >existing knowledge or prejudice always colors the data".
>
> I'm not much of a philsopher of science but it seems to me that my
> observations are simply my observations. My observations -- what I see,
> hear, taste, feel, smell, imagine, fear, etc -- are my perceptual
> experiences, which simply are there. I am often simulataneously making
> mental _interpretations_ of these observations, either formally (as when I
> interpret observations of control phenomena in terms of a control model) or
> informally (as when I interpret my inability to open the door with my old
> house key - - wrong key, broken lock, alternative universe, etc). But the
> observations _themselves_ (of the key, the effort, the anger and
> confusion) are simply there, uninterpreted.

"What there is does not in general depend on one's use of language, but what one says there is does."

W.V.O. Quine

Bruce Gregory

Date: Fri, 6 Dec 1996 11:49:09 -0700
From: "T. B. Harms" <tbh@TESSER.COM>
Subject: Re: Epistemology -- again

>From Tracy Harms 1996;12,06.11:45

Bill Leach 961206,1130,

I think my prior answer was too hasy and glib. I now see that I can interpret you paragraph to mean that where important dispute exists in regard to basic presuppositions, debate regarding particulars cannot be productive. The discussion must first turn to those basics, and if agreement cannot be reached on that level, debate on particulars will tend to be nothing but misunderstanding.

So, I take it back, your statement was not false; I just did not read it carefully enough.

Regardless, I still think that my point about the importance of epistemology is worthwhile.

Tracy Harms

Date: Fri, 6 Dec 1996 15:56:48 -0500
From: Bill Leach <b.leach@WORLDNET.ATT.NET>
Subject: Re: Social Control

<Bill Leach 961206;1458

>Martin Taylor 961206 10:50]

Hi Martin, long time...

I suspect that in the discussion between you and Rick the two of you untimately are "saying the same thing".

However, (and there is ALWAYS a "however") I believe Rick's use of terminology is more precise from a perspective of expressing behaviour in PCT terms.

As I see it, you are stating that the customer is "controlling" the salesperson (cooperatively). Where as Rick is asserting that the customer IS ONLY controlling the customer's perception of possessing an object. Another control system, the salesman, is in the environmental feedback path for that perception. Since the salesman (both presumed and explicitely stated in the various postings) has a reference for a perception that "the customer be satisfied" and (forgive me) a "model" of the situation that includes a belief that if the salesman gives the customer the object that the salesman perceives that the customer wants then the customer will indeed be satisfied and thus satisfying the

salesman's reference for a controlled perception.

As I understand it, Rick is asserting that this situation would best be described as the customer controlling an "incidental side effect" and you are asserting that the customer is controlling some aspect of the salesman.

The two of you might almost be "splitting hairs" but I will take "Rick's side" in this for the following reason:

A disturbance to the salesman's perceptions that results in a change in the observed behaviour of the salesman with respect to the customer's controlled perception (of obtaining the item) may well result in a control action on the part of the customer but that action may well also have no effect upon the salesman.

Naturally, one is typically reminded of the observed behaviour of two equally powerful control systems in conflict due to in-phase component of two different reference values (ie: it is physically impossible for both references to be satisfied at the same time). This might suggest that the obove "problem" between the customer and the salesman is the same sort of control conflict situation.

I would suggest however, that is not the case. The salesman could well continue controlling other perceptions (and probably will) without any conflict (particularly if the salesman does not perceive a worsening of the salesman's reference for "satisfied" customer).

This is a difficult example (as so many of the "simple" example usually prove to be). If the customer begins shouting at the salesman and/or threatening then indeed it is quite possible that the customer has switched from controlling for perception of possession of the object by relying upon the incidental relationship between the customer's references and the salesman's references to actually attempting to control the salesman.

These "real world" "temporal" examples are always very difficult to discuss. For example when we talk about a person "controlling a perception" the effectiveness of control is irrelevent to the idea that the perception IS a controlled perception (even though the adequacy of control IS a factor for one applying the TEST).

While we could assert that anytime somene uses language in an attempt to control a perception they are then also controlling or attempting to control another person, I believe that such global application of the term makes such application useless.

I would suggest that until one person actually has a reference for controlling another person the use of the term with respect to their interaction is misleading unless used as Rick has by pointing out the environmental relationship between their two different controlled CEVs.

-bill leach

Date: Fri, 6 Dec 1996 14:10:26 -0700
From: "T. B. Harms" <tbh@TESSER.COM>
Subject: perception, observation, experience, etc.

>From Tracy Harms (961206.14)

I'd be interested in hearing any discussion on the differences between perception, observation, awareness, and experience, as those terms and ideas have come to be used by the HPCT community.

Tracy Bruce Harms tbh@tesser.com
Boulder, Colorado

"Life is the art of drawing sufficient
conclusions from insufficient premises."
Samuel Butler

Date: Fri, 6 Dec 1996 16:16:21 -0500
From: Bill Leach <b.leach@WORLDNET.ATT.NET>
Subject: Re: Epistemology -- again

<Bill Leach 961206;1440
>Tracy Harms 1996;12,06.11:30

>But his must certainly be false, ...

I have no problem with what you are saying. What I was trying to say is that when the field of discussion is supposed to be agreed upon (ie: PCT) then the rules of what constitutes valid data, what the axioms of logical discourse, etc. must be agreed upon or else the discussion, debate, arguement, or whatever is basically noise.

I agree that even in such instances, it is possible (probable?) for any of the parties to "learn" but that learning will be incidental to the allegeded topic.

We have had astrologist, numerologists, spiritualists, etc. on this net many times. Until such time as people "operating" from such backgrounds accept the "rules" of discourse used (or at least supposed to be used here) there is virtually no learning of any value to any of the participants (obviously a perception properly classed as an opinion of judgement on my part but I think that you will find a high level of agreement among PCTers on that one).

In particular, when "religious" discussions move from trying to "prove" that God exists or trying to "prove" that God does not exist (or trying to "make" believers/non-believers out of each other), such discussion can be quite enlightening. Personally a PCT perspective applied to New Testiment verbage is often fascinating and in my own opinion of course, seems to sometimes make very difficult to understand statements "make sense".

-bill leach

Date: Fri, 6 Dec 1996 13:26:52 -0800

From: Richard Marken <marken@AEROSPACE.AERO.ORG>
Subject: Side effects

[From Rick Marken (961206.1330)]

Martin Taylor (961206 10:50) --

>So the question at issue is whether the words "side effect" are appropriate
>to apply to the situation [of customer controlling the "side effects" of
>the salesperson's actions].

I agree.

>Now, what are side-effects? ...The "control actions themselves"
>are the projection of the output onto the CEV. The "side-effects" are
>the rest of the influence of the control output on the environment.

I agree. I would put it this way: In a control system

(1) q = g(o)

where q is the controlled variable, o is the control system output that has an effect on the controlled variable and g() is the nature of the effect of output on the controlled variable. When I say "action" I am referring to the output variable, o, in a control system. A side effect is the effect of o on any variable, v, _other than the controlled variable_, q. If

(2) v = f(o) and v<>q

then variations in v are a _side effect_ of variations in the actions of a control system.

When we try to control the action, o, of a control system, we can only control our perception, p, of that action, o. In this case we are controlling a perception, p, where

(3) p = f(o)

By my definition of side effect in equation (2) it is clear that whenever we control the actions of a control system we are really controlling a _side effect_ of those actions.

If p (the controlled side effect) is _directly_ related to action -- if, for example, p = k*o -- then I think it's fair to say that the system's actions are being controlled. This may be the case when the customer controls a perception of the salesperson's "actions". If, however, p is very indirectly related to o (in the way the position of the mouse -- in x-y coordinates of the table top -- is only loosely related to the screen position of the cursor) then I think its better to say that side effects of the system's actions are controlled. But in both cases it is really side effects (in the sense of (2) above) that are being controlled.

Best

Rick

PS. The IJHMS paper is caught in more beaurocracy; I hope to get it to you by _next_ friday.

Date: Fri, 6 Dec 1996 14:26:59 -0700
From: Bill Powers <powers_w@FRONTIER.NET>
Subject: Re: Side effects

[From Bill Powers (961206.1340 MST)]

Martin Taylor [961206 10:50] et. al.--

>As you can see, I'm still not sure whether the term "side-effects" is
>properly employed in either case. Maybe a new term is required for the
>situation were the actions of each party for part of the environmental
>feedback path for the perceptions of the other, and where the two CEVs
>are orthogonal. In one sense "side-effect" is perfectly proper, and
>in another sense it isn't.

I think this discussion would be made a lot less complicated if you-all just defined what you mean by a side-effect. To me the definition is strictly from the viewpoint of one particular control system, which isn't concerned with HOW its actions affect its perceptions. A side-effect, as I have been thinking of the term, is simply any effect of the action that's not part of the control loop of the system in question.

Consider the loop that controls muscle tension. For this system, spatial accelerations of a limb about a joint are a side-effect, since they aren't included in the controlled variable, the sensed tension. However, that very side-effect can be part of the main effect for a higher-order system, the system that controls joint angle. It _is_ part of that higher-order control loop. So whether a given consequence of an output is a side-effect or a main effect depends on which control system, at which level, you're talking about.

Consider the clerk handing the package to the customer. As far as the position control system is concerned, the fact that the package is being handed to a customer is irrelevant; that system is simply controlling the perceived position of the package. Whether this pleases the customer or not is of no concern to this system. The effect on the customer is a side-effect.

However, for the higher-order system in the clerk concerned with pleasing the customer, that whole package-handling system is merely a means to an end, and the perceived satisfaction of the customer is precisely the controlled variable -- so the act of moving the package to a particular place is not a side-effect with respect to that level of control. A true side-effect at that level might be the effect on the perceptions of the boss who is thinking, "How nicely that clerk relates to that customer." The boss-observer' s perception might come to have some effect, ultimately, on something else that matters to the clerk, such as a pay raise, but it's not part of the customer-satisfaction loop. It's a side-effect relative to _that_ control system.

Of course there's also the case where a control system normally controls via a specific environmental feedback function, but some other system, being affected by the control actions, reacts in such a way as to create another link between the output and the controlled variable, in parallel with the first one. What had been a side-effect now becomes part of the main effect, and the control system might have to reorganize to maintain control.

Another subject. When we speak about "controlling another person," we're just referring generally, and vaguely, to some control process. If this

vagueness leads into confusion, we have to stop and ask _what variables_ are being controlled. You can't control a person; that doesn't mean anything. You can control, or try to control, _some variable aspect_ of a person, such as the direction of effort or movement, or the state of being alive or dead, or the amount of money that person has or spends, and so on. Simply talking about "controlling people" is like talking about "controlling a car," without specifying whether you mean the car's price, its cleanliness, its direction of movement, its speed, its distance from a destination, the number of wrinkles on its fenders, or its mechanical condition.

Most arguments that wander off into philosophical complexities do so, I am convinced, mostly because the original thesis has been stated too loosely, permitting all kinds of contradictory interpretations.

Best,

Bill P.

Date: Fri, 6 Dec 1996 16:30:15 -0600
From: Wayne A Hershberger <tj0wah1@CORN.CSO.NIU.EDU>
Subject: Volitional Action update

[from Wayne Hershberger]

To:
Avery Andrews
Bruce Gregory
Chris Cherpas
Clark McPhail
Ellery Lanier
Frank Schweingruber
Fred Nickols
Gary Cziko
Hugh G. Petrie
Kent McClelland
M. M. Taylor
Ray Jackson
Remi Cote (two copies)
Richard Thurman
Rupert J Young
Scott Brandon
Tracy Harms

I have ordered a copy of Volitional Action for each of you, but I do not expect the copies to arrive here in DeKalb until after the first of the year. They are coming from Europe by sea, and snail mail. Please be patient.

Regards, Wayne

Wayne A. Hershberger Email: wah@niu.edu
Department of Psychology Fax: (815) 753-8088
Northern Illinois University Phone: (815) 753-7097 DeKalb, IL 60115

Date: Fri, 6 Dec 1996 18:26:13 -0500
From: Bruce Gregory <bgregory@CFA.HARVARD.EDU>
Subject: Re: Side effects

[From Bruce Gregory (961206.1830 EST)]

Bill Powers (961206.1340 MST)

> Most arguments that wander off into philosophical complexities do so, I am
> convinced, mostly because the original thesis has been stated too loosely,
> permitting all kinds of contradictory interpretations.

Each of which may be valid under a different set of assumptions, n'est-ce pas?

Bruce Gregory

Date: Fri, 6 Dec 1996 20:33:14 -0500
From: PSY_DELPRATO@EMUVAX.EMICH.EDU
Subject: Re: Observation, Theory, God

[FROM: Dennis Delprato (961206)]

>Bruce Gregory (961206.1420 EST)

>> Tracy Harms (961205.17) --
>>
>> I'm not much of a philsopher of science but it seems to me that my
>> observations are simply my observations. My observations -- what I see,
>> hear, taste, feel, smell, imagine, fear, etc -- are my perceptual
>> experiences, which simply are there. I am often simulataneously making
>> mental _interpretations_ of these observations, either formally (as when I
>> interpret observations of control phenomena in terms of a control model) or
>> informally (as when I interpret my inability to open the door with my old
>> house key - - wrong key, broken lock, alternative universe, etc). But the
>> observations _themselves_ (of the key, the effort, the anger and
>> confusion) are simply there, uninterpreted.

>"What there is does not in general depend on one's use of language,
>but what one says there is does."
>
> W.V.O. Quine

"The inner world of experience is a kind of telling, not a kind of place."

Roy Schafer

Date: Sat, 7 Dec 1996 06:21:45 -0700
From: Bill Powers <powers_w@FRONTIER.NET>
Subject: Re: perception, observation, experience, etc.

[From Bill Powers (961207.0545 MST)]

Tracy Harms (961206.14) --

>I'd be interested in hearing any discussion on the differences between
>perception, observation, awareness, and experience, as those terms and
>ideas have come to be used by the HPCT community.

Here's how I use these terms, as part of a general mental model although not any formal model.

Perception: the existence of a neural signals in a perceptual pathway (at the output of a perceptual input function). Specifically is NOT limited to conscious perception.

Awareness: a function of the mind that receives information selectively from any part of the perceptual systems in the hierarchy. Physical nature unknown. Also known as the Observer, with a capital O.

Experience: the combination of perceptual signals and awareness, defining that part of the workings of the hierarchy of which we are conscious at a given time. It is not possible to be mistaken about an experience.

Observation (with a small o): noticing, recording, or describing the content of experience. "I observe X" means "the experience that I refer to as X is present." One can make a mistake in assigning a label X to an experience -- not pick the best word. Words and other symbols have associations with other experiences as well, and so introduce ambiguity into descriptions -- unless the terms are formally defined. However, the symbol is never the experience being observed. The symbol is a different experience, so naming the experience introduces a relationship between experiences (experience A is the name of experience B), which is an arbitrary (but of course useful) pairing.

Interpretation (belongs in this list): Any statement about or deduction from observations. Example: an _observation_ of a blurry figure that passes into a wall is _interpreted_ as a ghost (and all that ghosts imply). Interpretations are subject to criteria of truth, which are other statements or interpretations. Second example: an _observation_ of a thermometer reading of 30 c is _interpreted_ to mean that the water in which the end of the thermometer is immersed is at a temperature of 30 c. This interpretation can be in error, as can be shown by comparing the first observation with an observation 10 sec later of a reading of 40 c.

That's how I usually use these terms.

Best,

Bill P.

End of CSGNET Digest - 6 Dec 1996 to 7 Dec 1996
****

Date: Sun, 8 Dec 1996 08:02:00 +1800
Subject: CSGNET Digest - 7 Dec 1996 to 8 Dec 1996

There are 2 messages totalling 48 lines in this issue.

Topics of the day:

1. Model-based behavior (2)

Date: Sat, 7 Dec 1996 17:25:46 -0700
From: Bill Powers <powers_w@FRONTIER.NET>
Subject: Model-based behavior

[From Bill Powers (961207.1730 MST)]

Rick Marken (961206.0830) --

Rick, when you say that the Vancouver experiment shows "that model-based behavior plays _no_ role in purposeful behavior at all" you go far beyond the evidence. All that the spiral experiment showed was that the model-based control model would do no better at explaining the behavior than other and far simpler models could do, because the amount of successful "prediction" going on was pretty trivial. I think the experiment was useful in showing the limitations of a model-based approach , which is sometimes presented as the answer to everything. If human beings do, however implausibly, use model-based control in the spiral experiment, they do not use very good models. That's about all we can say.

Best,

Bill P.

Date: Sat, 7 Dec 1996 18:46:52 +0000
From: Richard marken <marken@AERO.ORG>
Subject: Re: Model-based behavior

[From Rick Marken (961207.1740)]

Bill Powers (961207.1730 MST) --

> Rick, when you say that the Vancouver experiment shows "that model-
>based behavior plays _no_ role in purposeful behavior at all" you go
>far beyond the evidence.

I guess I got a bit carried away.

But I figure it doesn't matter much since it's all just a matter of opinion anyway;-).

Best

Rick

End of CSGNET Digest - 7 Dec 1996 to 8 Dec 1996
****

Date: Tue, 10 Dec 1996 08:00:19 -0600
Subject: CSGNET Digest - 8 Dec 1996 to 10 Dec 1996

There are 18 messages totalling 1065 lines in this issue.

Topics of the day:

Date: Mon, 9 Dec 1996 17:54:44 +0100
From: "J.A. Blom" <J.A.Blom@ELE.TUE.NL>
Subject: Re: Ahhh, epistemology at last!

[Hans Blom, 961209]

(Tracy Harms 961205.17)

>Hans Blom, 961205b corresponds with Rick Marken (961204.0800),
>(961205.0800)

>I am intrigued. I, too, think that "there are no theory-free
>observations; internal pre-existing knowledge or prejudice always
>colors the data". This has me inclined to chip in with Hans Blom.
>However, he also says "I function on the principle that there are no
>facts or truths, just opinions." That throws me far in an opposite
>direction.

Word usage again? Let me explain my position. I place "facts" and "truths" (if these exist) in the "real world" domain, to which I have no direct access and about which I thus prefer not to talk. My only access to the "world out there" (sorry, I must use words) is through observations, which color what I perceive of those "truths". Thus, I can know no more than approximations (and possibly distortions) of "truths". These are the opinions I referred to.

When someone talks about his/her discovery of a truth/fact, I hear the proposal of a research program, not the end of one. And I wonder how accurate the statement is and where it will meet its boundaries of applicability.

Even physics is, I believe, not about the world but about our knowledge of it. There is a map and there is a territory. We humans are great map-makers (modellers), and we tend to make maps of even territories that we cannot know. But, even though we know only the map, I believe in the value of reminder ourselves once in a while that this is so. That makes me a supporter of the Copenhagen interpretation in physics, I guess, and a supporter of Wittgenstein's "Let's not talk about things we cannot know," even when others pretend to know "truths" or "facts". I regard what they say as an opinion, whose value (to me) remains up to me to decide (using, and

hopefully updating, my map). Would that make me a postmodernist in science and philosophy? Is there such a thing as postmodernist science?

An example from physics. On several occasions in the past, units had to be decided upon for the different (?) phenomena. For a long time in the previous century, static electricity had a dimension (unit) different from "dynamic" electricity; they were seen as different things entirely. Now, after introduction of the SI system, we see them as the same thing.

Another example. Just suppose that we had somehow chosen the speed of light as a dimensionless constant with value 1 -- well, why not? any consistent basis will do. If we had, Einstein would have discovered that E = m, which would state unequivocally that mass IS energy, much more directly than we say essentially the same thing (E = mc^2) now. Thus, even something as simple as a choice of coordinate system has an impact on how we see the world.

>I'm not so sure what Hans thinks, but I will say this: If you
>think, Hans, that the absence of theory-free observation implies the
>presence of control-system structure of this sort "AT ALL LEVELS",
>your interpretation is incorrect.

I do not posit a strict implication. I both believe in the impossibility of theory-free observation AND in its pervasive character throughout the hierarchy (of control/knowledge). These pretty much hang together for me. But since I do not have a well-defined notion of which levels there are or even whether it is of advantage to think in terms of hierarchical levels at all (as contrasted with, say, parallel operating "agents"), I would not be able to enumerate levels and check whether I've encountered all of them. So, although the two notions relate, I do not see a strict logical implication here.

>If this is what you think, you have misunderstood the meaning of the
>proposition that all observations are theory impregnated. At least,
>your treatment is contrary to the writings on this idea which have
>influenced me.

I admire the modesty of the second sentence ;-). Please tell me what your position is on the relationship between the "coloring" of our perceptions and the accuracy of our knowledge and/or control.

Great quote, this:

>"How odd it is that anyone should not see that all observation must
>be for or against some view if it is to be of any service!"
Charles Darwin

This is, I think, exactly in the same vein (although somewhat more black-and-white...) as me saying that all our observations are in the service of tuning our "world model" (map, view, perspective, subjective "truth"). My admiration for Darwin suddenly doubled!

Greetings,

Hans

Date: Mon, 9 Dec 1996 18:15:02 +0100
From: "J.A. Blom" <J.A.Blom@ELE.TUE.NL>
Subject: Re: Observation, Theory, God

[Hans Blom, 961209b]

(Rick Marken (961206.0830))

>I'm not much of a philsopher of science but it seems to me that my
>observations are simply my observations. My observations -- what I
>see, hear, taste, feel, smell, imagine, fear, etc -- are my
>perceptual experiences, which simply are there. I am often
>simulataneously making mental _interpretations_ of these
>observations, either formally (as when I interpret observations of
>control phenomena in terms of a control model) or informally (as
>when I interpret my inability to open the door with my old house key
>-- wrong key, broken lock, alternative universe, etc). But the
>observations _themselves_ (of the key, the effort, the anger and
>confusion) are simply there, uninterpreted.

Hey, Rick, could you believe that pretty much the same thing happens in/for/to me? And that they have a similar impact on me?

>It seems to me that Hans Blom (961205b) was rejecting the entire
>scientific enterprise by saying that "it's all opinion". Yes, it is
>all opinion, but science has made it possible to show that some
>opinions (like the opinion that behavior is the control of
>perception) are better than others (like the currently prevailing
>opinion that perception is the control of behavior).

I do not reject the scientific enterprise; I'm part of it, in my own small way. What I reject is the use of science as an authoritarian power trip, where a scientist comes across as shouting "my opinion is better than yours!". That demeans science. It is also a bad (control) strategy, in that it seldomly reaches its goal -- if the goal is to hand someone a better tool (theory) so that he/she can be a better controller. PCT tells you why, although maybe not what a better strategy would be. Or does it?

>People (like Hans) who continue to defend beliefs (theories) in the
>face of evidence that contradicts those beliefs happen to give me
>the creeps.

Why is that, Rick? Why the large emotional impact? Can't you simply and diffidently accept that not everyone is as smart as you -- and doesn't want to be as smart?

Greetings,

Hans

Date: Mon, 9 Dec 1996 14:00:51 -0500
From: Martin Taylor <mmt@HERMES.DCIEM.DND.CA>
Subject: Re: Side effects

[Martin Taylor 961209 13:30]
>Bill Powers (961206.1340 MST), Rick Marken (961206.1330) and
>Bill Leach 961206;1458

Three different takes on what "side-effect" means. Four, if you include mine (961206 10:50). As self-appointed referee, I like Bill Powers' version best. I will attempt to paraphrase:

A control system has an output to the world and an input from the world. The "world" includes all lower-level control systems to which the one in question sends its output or from which it may get part of its input. Actions in the world, either within the hierarchy or outside the skin, are not "side-effects" if they form part of the control loop. If the influence of the action does not affect the controlled perception (within the time-scale of the control loop), the action is a side-effect. The term "side-effect" must be used in conjunction with a specification of a controlled perception, because the determination of whether an effect is a "side-effect" or a "main-line effect" depends on whether the effect affects the particular perception.

Is that right?

BP>However, for the higher-order system in the clerk concerned with pleasing
>the customer, that whole package-handling system is merely a means to an
>end, and the perceived satisfaction of the customer is precisely the
>controlled variable -- so the act of moving the package to a particular
>place is not a side-effect with respect to that level of control. A true
>side-effect at that level might be the effect on the perceptions of the boss
>who is thinking, "How nicely that clerk relates to that customer."

How clearly you put it! I hope that's where my muddled musings were going.

>The boss-observer' s perception might come to have some effect, ultimately, on
>something else that matters to the clerk, such as a pay raise, but it's not
>part of the customer-satisfaction loop. It's a side-effect relative to
>_that_ control system.

Right. But we can also look from the viewpoint of the salesperson's control of the perception of being well paid, for which the customer-satisfaction perceptual control may be a means. Just like your muscle-tension/accereration example:
>whether a given consequence of an output is a side-effect or a main
>effect depends on which control system, at which level, you're talking about.

>Another subject. When we speak about "controlling another person," we're
>just referring generally, and vaguely, to some control process. If this
>vagueness leads into confusion, we have to stop and ask _what variables_ are
>being controlled. You can't control a person; that doesn't mean anything.
>You can control, or try to control, _some variable aspect_ of a person, such
>as the direction of effort or movement, or the state of being alive or dead,
>or the amount of money that person has or spends, and so on.

Well said. And in general, so long as we are talking about scalar control systems one-at-a-time, we can't talk about controlling _any_ multidimensional variable.

I don't know whether this is another subject or the same one, but I suspect it links into the confusion about what is a side-effect. If one wants to keep a car in the centre of the driving lane, one _must_ control the angle

of the steering wheel. But one cannot _independently_ control the angle of the wheel and the position of the car in its lane. The position of the wheel is not a side-effect of the control of the perception of the car's position.

In the same way, the salesperson's handing the customer the desired item is not a side-effect of the salesperson's control of the perception of the customer's satisfaction. The actual angle of the wheel (item presented) will vary quasi-randomly over time or occasion, but that variation has nothing to do with whether the action of moving the wheel or handing over the item is a side-effect. So also with the "X" drawn in the rubber-band demo. On another occasion it might be "Mickey Mouse" instead of "X", but that doesn't make the actions that draw it side-effects of controlling the knot position.

And since for the customer, perceiving the salesperson to be handing over the desired item is the target of control, it is not a side-effect for the customer either.

Bill L. and Rick--does this all ring true?

Martin

Date: Mon, 9 Dec 1996 12:56:34 -0700
From: "T. B. Harms" <tbh@TESSER.COM>
Subject: Re: Ahhh, epistemology at last!

>From Tracy Harms 961209.13

Hans Blom (961209),

When conversations like this arise, I find myself wondering about cost/benefit ratios. I think this is worth doing, but the early impression is often that enormous effort could be invested by all parties to little avail. Not to be pessimistic, however, I turn now to some of your clarifications.

Regarding 'fact' and 'truth', and whether to ''place [these] in the "real world" domain'', there are some semantic differences. I can accept your useage more readily for fact than for truth, as truth is primarily a property of *propositions*, and thus while the subjects of the propositions may be "in the real world", the propositions themselves (and their qualities, including truth) must not be so alienated as you would alienate truth.

But even if I simply take the words as you wish to use them, if you say facts (etc.) exist in the real world, and there are no facts, the implication is that there is no real world. In that case, I'm not sure that we can fruitfully discuss ANYTHING. The consequences of this world-view are to my mind entirely incoherent. So conceptualized, argumentation either defies minimal conformance to logic, or logical implications have no persuasive strength. All bets are off; irrationality reigns. And as Rick pointed out, then decision can only be finalized by force.

>When someone talks about his/her discovery of a truth/fact, I hear
>the proposal of a research program, not the end of one. And I wonder

>how accurate the statement is and where it will meet its boundaries
>of applicability.

That paragraph, however, I find entirely acceptable. I would add that a discovery may easily entail the end of one research program at the same time that it is the inception of another.

On a side matter, there is an approach to physics where c is established as a basic unit, and thus it simplifies the formulae just as you indicate. This is in fact used by physicists.

>Please tell me what
>your position is on the relationship between the "coloring" of our
>perceptions and the accuracy of our knowledge and/or control.

OK, I will, but I think I'll post this message as is before I attempt to compose something of that difficulty.

Tracy Bruce Harms tbh@tesser.com
Boulder, Colorado

"Man has created new worlds--of language, of music, of poetry, of science; and the most important of these is the world of moral demands, for equality, for freedom, and for helping the weak."
Karl Popper

Date: Mon, 9 Dec 1996 13:05:56 -0800
From: Richard Marken <marken@AEROSPACE.AERO.ORG>
Subject: System concepts, effects and side effects

[From Rick Marken (961209.1300)]

Hans Blom (961209b) --

>I do not reject the scientific enterprise

Then why, after so many years on CSGNet, do you continue to talk about model-based control as though it were an important extension to (or replacement for) HPCT? You have never presented anything other than (questionable) anecdotal evidence (such the the fact that you can continue the actions, when the light suddenly goes off, that result in picking up your slippers) for the superiority of model- based control over PCT. But these anecdotal examples are handled just as well by an HPCT model that is controlling more than one perception at a time (one that controls not just the visual perception of the slippers but also the kinesthetic perception of the body, the acoustical perceptions of reflected sound, etc).

The model-based control program that you sent showed that what is being modelled in model-based control is the physical effect of the controller on the controlled variable. All this buys is the ability to "virtually control" in the blind for some period after loss of perceptual input. But this ability is paid for in terms of lowered ability to control (resist disturbances) in real time; the longer the model can control after loss of perceptual input, the less able it is to compensate for real time disturbances when perceptual input is intact. But people are able to resist real-time disturbances to controlled variables, even when they are able to "control"

these variables "in the blind" for substantial periods.

Me:

>People (like Hans) who continue to defend beliefs (theories) in the
>face of evidence that contradicts those beliefs happen to give me
>the creeps.

Hans:

>Why is that, Rick? Why the large emotional impact?

Because I am controlling for gaining knowledge based on observation and modeling and you apparently are not. If you really believe that model-based control is the best model of human controlling then, if you were controlling the same system concept as me, you would be willing to subject your belief (personally or via some proxy) to experimental test. I prefer to deal with people who are willing to subject their beliefs to legitimate and rigorous tests -- tests where the results may require revision of one's belief. I am willing to put my belief in PCT to that test; if you are _not_ willing to put your belief in model-based control to that test then we are simply dealing with the world in terms of two very different system concepts. And people who are defending very different system concepts from my own are scary to me (they give me the creeps) - - because I don't know _what_ they are up to or what they might do to get there.

>Can't you simply and diffidently accept that not everyone is as smart as
>you - - and doesn't want to be as smart?

This is _not_ a matter of intelligence. I don't think I'm all that smart. I am not annoyed with you because you are not smart enough; I'm quite sure that your capabilities at the logic and program level (the skills that are considered by many to the the mark of "intelligence") far exceed my own. You just seem to be defending a system concept that is quite different from one that I am defending.

Martin Taylor (961209 13:30) --

>the salesperson's handing the customer the desired item is not a side-effect
>of the salesperson's control of the perception of the customer's
>satisfaction.

>Bill L. and Rick--does this all ring true?

Yes. Moveover, I realized that, in this case, the customer's perception of the salesperson's action (actually, of a result of the salesperson's actions -- getting the item) is _in the feedback path_ between the salesperson's actions and his controlled perception. So the customer's perception of the salespeson's action is not a side effect _in this case_. It is a _relevent_ effect of the salesperson's actions.

Best

Rick

Date: Mon, 9 Dec 1996 16:08:34 EST
From: Bruce Gregory <gregory@CF4.HARVARD.EDU>

Subject: Re: Ahhh, epistemology at last!

[From Bruce Gregory (961209.1610 EST)]

Tracy Harms 961209.13

> >Hans Blom (961209),

> "Man has created new worlds--of language, of music, of poetry,
> of science; and the most important of these is the world of moral
> demands, for equality, for freedom, and for helping the weak."
> Karl Popper

> >When someone talks about his/her discovery of a truth/fact, I hear
> >the proposal of a research program, not the end of one. And I wonder
> >how accurate the statement is and where it will meet its boundaries
> >of applicability.

Bruce Gregory

Date: Mon, 9 Dec 1996 16:59:52 EST
From: Bruce Gregory <gregory@CF4.HARVARD.EDU>
Subject: Re: Effects and side effects

[From Bruce Gregory (961209.1700 EST)]

I trust that I am not the only person who perceives the exchange on effects and side effects to be both instructional and convergent. If this is true, is there anything we can learn from the process that would encourage more exchanges on CSGNet to be equally productive?

Bruce Gregory

Date: Mon, 9 Dec 1996 15:57:24 -0700
From: Bill Powers <powers_w@FRONTIER.NET>
Subject: Re: Side effects

[From Bill Powers (961209.1545 MST)]

Martin Taylor 961209 13:30]

What a lovely day! We agree absolutely everywhere on the subject of side-effects. I accept all your elaborations, too.

Best,

Bill P.

Date: Mon, 9 Dec 1996 15:15:59 -0800

From: Richard Marken <marken@AEROSPACE.AERO.ORG>
Subject: Re: Effects and side effects

[From Rick Marken (961209.1510)]

Bruce Gregory (961209.1700 EST)--

>I trust that I am not the only person who perceives the exchange on effects
>and side effects to be both instructional and convergent.

Yes. You are not the only person. I thought these exchanges were instructional and convergent, too.

>is there anything we can learn from the process that would encourage more
>exchanges on CSGNet to be equally productive?

I think this exchange "worked" because the controlled variable -- the definition of a "side effect"-- could be brought to a state that was "right" for all of us, without causing any other controlled variables to be pushed into a state that felt "wrong" to any of us. The only way, in other words, to have productive exchanges is to make sure, in advance, that there is a state of the controlled variable that will satisfy all parties. Unfortunately, more often than not, such a state of the controlled variable exists only if some or all the parties to the exchange are willing to change their references for the state of the controlled variable -- ie. if they are willing to change what they want.

It's wonderful when a group of living control systems can come to agreement about the apporopriate state of some perception. Unfortunately, such agreement cannot be enouraged; it can only be hoped for because it depends on the selection of reference specifications by systems who set their references completely autonomously; ie. all ya can fo is hope that what everyone wants turns out to be what's OK for all.

Best

Rick

Date: Mon, 9 Dec 1996 16:41:46 -0700
From: "T. B. Harms" <tbh@TESSER.COM>
Subject: knowing the real world

From: Tracy Harms 1996;12,09.16:

Hans Blom, 961209 -- let me try again...

>[...] Let me explain my position. I place "facts" and
>"truths" (if these exist) in the "real world" domain, to which I have
>no direct access and about which I thus prefer not to talk. My only
>access to the "world out there" (sorry, I must use words) is through
>observations, which color what I perceive of those "truths". Thus, I
>can know no more than approximations (and possibly distortions) of
>"truths". These are the opinions I referred to.

In my last response I played a harsh interpretation of your words. I now think I should give you more benefit of the doubt by paying attention to

your attempt to explain, above. What you say here does speak to involvement with a wider world, the whole of which is real and not to be conflated with any given thinker. That is the ante which makes further discussion worth attempting. By guessing that there is a real world you certify your intention to not let things devolve into irrationality (the result of which I warned about last time).

I too make a distinction between the sphere of an organism and the entirety of existence, most of which is beyond the interactions of that organism. The knowledge which an organism has is indeed 'opinion' in your sense of that word: it is a fallible approximation by guesswork. But insofar as these opinions (items of knowledge) do not fail, it is that world itself which we know, contrary to your indication that all we can know is observations. In this message I will argue against what I see as the central metaphor of your vision: the isolation of the organism from the world.

Organisms are isolated in a very important manner: the isolation is in regard to learning. Recognition of that isolation has been an important advancement in our understanding. Unfortunately this very insight has led to a mistaken idea whereby organisms are thought of as epistemologically isolated from their environments. But only in learning are we isolated so; in the processes of knowing we and the world we know are unified.

Kant was first to made clear the insight that our knowledge of the world must originate within our own systems; our ability to know cannot be received 'from the outside.' This was a partial solution to a problem which Hume had identified. Hume demolished the Lockean presumption that we learn by an ingestion of stimuli which contain (and thus provide) knowledge about the thing perceived. Having done that, however, Hume had no positive proposal to offer in its stead. Hume's problem was the problem as to how knowledge about the world beyond an organism becomes present in an organism.

Hume had rained on the parade, and Kant had made a distinction between things-as-known and things-in-themselves; as a result worries intensified along solipsistic lines. But I offer that an error was made in such worrying: the problem of the *origin* of knowledge was being confused with the *operation* of existing knowledge. This is an error because the isolation which must be taken into account in the former case has no relevance in the latter. Once you have knowledge, said knowledge is the "bridge between the worlds" as it were.

In HPCT terms, the difficulty we received from Hume and Kant is how perceptual control systems can arise. That this is the nature of their problem was not recognized by them; indeed, it was not until recently that Donald T. Campbell identified that the solution to their problem was fundamentally present in the work of Wallace and Darwin. In the meantime, however, attempts at solving this problem were undertaken by examining the operations of knowledge-intensive systems, especially human consciousness. And indeed Hume himself had probed along these lines as he confronted the impossibility of receiving knowledge from 'the world' by exhausting the options by which such knowledge might possibly be bestowed from the outside in the course of perceptual processing.

What has been overlooked is that this absence of knowledge-flow does not imply the isolation of the organism from knowledge of its environment. For while the organism does not receive *knowledge*, it does receive *perception,* and that perception exhibits the presence of knowledge. For

example, the fact is that external heat cannot transmit into me the ability to know such heat, but if I *have* gained the ability to know external heat, it is in fact external heat that I know. It really is that simple.

Perception is obtainable because the organism's systems are intimately, coherently involved with the wider world. There simply is no breach here, there is no film or membrane which separates a creature from the rest of creation. How can we understand this involvement? A hint is to be found in the words of Bill Powers: "feedback, when correctly analyzed, is the central and determining factor in *all* observed behavior." (B:CP p44) Perception is a feedback phenomenon, and in feedback every component of the circuit is involved. A portion of the circuit we naturally think of as outside the organism, and it is. BUT IT IS NOT OUTSIDE THE CIRCUIT. The solipsistic indulgences which have been inspired by philosophizing of the past are mistakes: The basic mistake was considering the identity (and hence the edges) of the control system *hardware*, when the questions of the status of our perception can only be understood in consideration of the control system *operation*, which includes the entirety of the loop.

The central subjects for epistemology are not knowers, but knowings. What must be examined are not the organic units which are studied by, say, zoologists, but rather the perceptual control units which are studied by -- well, I suppose somebody must be paying attention to 'em, somewhere... ;-)

The fundamental unit of PCT is a feedback cycle. Proper attention to this cycle in its completeness shows that environment and organism are indistinguishably co-present in perception. The misconception that there is a gap between knower and known is correctable by this recognition.

That's all for now, folks. Time to roll some credits: My description of the historical problem situation and Campbell's rough solution is an echo of a portion of Gary Cziko's _Without Miracles_. The major refinement to that solution is Peter Munz' _Philosophical Darwinism_, and my application of PCT to Munz' epistemology is an original effort which appears here in print for the first time.

Tracy Bruce Harms tbh@tesser.com
Boulder, Colorado

> "The problem of reference--of how concepts or words hook into the world--disappears because it has become clear that the objects and events in the world only appear in so far as they are constituted by the theories and organisms."
>
> Peter Munz
> Philosophical Darwinism, p206

Date: Mon, 9 Dec 1996 16:57:24 -0800
From: David Wolsk <dow@PINC.COM>
Subject: Skinner quote

At 17:00 09/12/96 David Wolsk fowarded:

from another net today came this indication that Skinner was a closet PCTer (close anyway)

As Skinner put it in 1956 in an

article for the American Psychologist titled A Case History for the Scientific Method:

"The organism whose behavior is most extensively modified and most completely controlled in research....is the experimenter hiself (sic)...The subjects we study reinforce us much more effectively than we reinforce them." (p.232)

Date: Mon, 9 Dec 1996 18:44:07 -0700
From: Bill Powers <powers_w@FRONTIER.NET>
Subject: 1997 CSG Conference

[from Mary Powers 961126]

1997 CSG CONFERENCE
(just some preliminaries)

The Control Systems Group Conference for 1997 will be in Durango, Colorado, from Wednesday evening, July 30 to Sunday morning, August 3.

Sorry about that, for those looking forward to meeting in North Carolina. Others, I know, like D'go a lot. It is, however, a long and expensive haul for Easterners. This has not gone unnoticed by some smaller, cheaper airlines that fly into Colorado Springs and Albuquerque, with commuter connections to D'go just starting up or in the works - so those who can't extract travel funds from somewhere may do better this year. Also direct flights now from LA.

For those of you who have been teaching PCT and have students who are interested, we can help students by waiving all or part of their conference fees (which will be somewhere between $250 and $300). Depending on the demand, we may be able to help some with travel also. This goes for financially challenged non-students as well.

I'd really like to see some of the people who are active on the net but have never come to a meeting. Face-to-face contact for a few days puts a very different light on net personalities. And we have missed those people who used to come but have passed up the more recent meetings.

By January I should have much more detail. This is just to get things started. Early start for a reason: it's FLC's busiest summer weekend, and dorm space at the college is going to be limited because of remodelling.

Mary P.

Date: Mon, 9 Dec 1996 21:32:27 -0500
From: Bill Leach <b.leach@WORLDNET.ATT.NET>
Subject: Re: Skinner quote

<Bill Leach 961209;2130
>David Wolsk 09/12/96 17:00

>from another net today came this indication that Skinner was a closet
>PCTer (close anyway)

I do hope you say that in jest!

-bill leach

Date: Mon, 9 Dec 1996 14:31:31 -0500
From: "Hugh G. Petrie" <hgpetrie@ACSU.BUFFALO.EDU>
Subject: Re: Volitional Action update

Wayne
>
>I have ordered a copy of Volitional Action for each of you, but I do not
>expect the copies to arrive here in DeKalb until after the first of the
>year. They are coming from Europe by sea, and snail mail. Please be
>patient.
>
Thanks,

Just send when available and remind me then of what I owe.

==========++++++++++==========**********==========++++++++++==========

Hugh G. Petrie 716-645-2491
367 Baldy Hall FAX: 716-645-2479
University at Buffalo
Buffalo, NY 14260
USA HGPETRIE@ACSU.BUFFALO.EDU

Date: Tue, 10 Dec 1996 10:47:42 +0100
From: Stefan Balke <sbalke@HRZ.UNI-BIELEFELD.DE>
Subject: Memory switch

>From Stefan Balke (961210.1030 CET)

Jeff,

I tried to understand your mastermind model, but I have only vague memories
at this game. I'll ask some friends here for the rules. Because of this
limited background I didn't understand much of your model, however I grasped
the idea, that there is a ECU analysing the error state of another ECU and
if an error is detected than the system switches to the imagination mode
until the reorganization process leads to a sufficient decrease of the error
and to a switch back to the control mode. Is this an appropriate description
of your idea and what did the others say to it? For me it seems to make sense.

Best, Stefan

Date: Tue, 10 Dec 1996 11:36:42 +0100
From: Oded Maler <Oded.Maler@IMAG.FR>
Subject: Jehovah's witness retracts

[From Oded Maler 961210]

> <Bill Leach 961206,1210
> >[From Oded Maler 961205]
>
>
> Rick Marken (961205.0800):
> That opinion certainly explains why you spurn the scientific approach to
> the study of living systems. If your statement above is true, then how
> do you explain the remarkable success of the scientific method?
>
> Oded Maler:
> Which sucess, can you remind us?
>
> Oh! Come on Oded! Are you another one of "those" that confuses both the
> "harm" and "good" accomplished by employing technology with the nature
> of technology itself?

I should have kept my mouth shut..
I was half-joking (the other half it wrt to the eternal questions
which were not solved and will never be solved by the "scientific methods").

> Oded Maler:
> >From time to time I am astonished by your naive scientificism, Rick.
> Take the most convincing experimental data that confirms your thesis,
> or that you use in order to track the so-called controlled variable,
> and I can easily show you on how many assumptions and prejudice it is
> built.
>
> Since it is easy, why don't you list at least a few of those assumptions
> that you believe that Rick is making that are not consistent with or are
> not a necessary part of scientific research.
>
> Though you may not care to do this, to convince me (and probably quite a
> few other people here on CSGNET) you will need to do a reasonable job of
> showing that the assumption you are citing IS necessary for Rick's
> research and data analysis to be logically valid AND that such an
> assumption is either inconsistent or unnecessary for "proper"
> application of the scientific method.
>
> -bill leach
>

Take for example the existence of something like "the position of a cursor
on the screen" - does it have an objective meaning or is it relative to
the perceptual world of certain experimenter?

In another reply, Rick said something that the scientific method suggests
an alternative to "might is right" in deciding what is the better thing
to do. This does not seem to be the case in the world we live in, neither.

Funny, there is here a meta-level paradox. According to this view, the scientific method is a kind of model-based behavior that humanity (should) employ, but since model-based behavior does not exist in the individual, how are we to expect it to emerge for the whole humanity?

--Oded

p.s.

An anecdote: last night I played a society game called "Heimlisch & co." by Ravensburger (probably marketted in the US under another name). All the players can control all the characters on the board but each knows only which character is his but does not know to whom belong the other characters (some of them may belong to none of the players). So the main goal of the play is to advance your character without disclosing who it really is, and make it harder to those of the others. So it is a complicated version of the Test..

Date: Tue, 10 Dec 1996 13:07:32 +0100
From: "J.A. Blom" <J.A.Blom@ELE.TUE.NL>
Subject: Re: Vancouver's experiment

[Hans Blom, 961210]

(Bill Powers (961204.0800 MST))

>It seems to me that your interpretation [prediction by a model]
>implies a very large expenditure in neurons and computing power to
>achieve a very small advantage

We humans do seem to have a very large number of neurons compared to an E. coli, a spider or a rat. What is our "very small advantage" compared to these species? Whatever it is, it seems to reside mainly in what neurologists call the association cortex. What would all those associations be for?

>, while giving up the ability to compensate immediately for
>unexpected (unpredicted) disturbances like Jeff's stuck mouse.

If the mouse gets stuck, the loop gain decreases to zero; no controller, model-based or not, will be able to cope with this situation immediately. What is necessary now is to recognize _that_ the loop gain is zero, to discover the cause, and to repair the situation in order to be in control again. This requires, I guess, the formulation of a number of hypotheses of what the cause might be (stuck mouse, stuck software, and any other likely possibility), to check them, maybe in order of plausibility, and so to discover the one single cause. Very difficult for a simple control system, although we humans are likely to repair the problem without much thought -- except maybe the first time, when we have no idea _that_ a mouse can get stuck.

>We can't let a particular engineering approach, developed under
>conditions that are, perhaps, appropriate for the construction of
>artificial systems where we have extensive knowlede of the
>environment and where the objectives are external to the system,
>dictate a model of the human or animal system.

That argument can be used against any theory-based approach, including (H)PCT. Why mention it? On the other hand, we can only construct new things (such as models of organisms) out of building blocks that we are already familiar with. Historically, paradigms of organisms have been clocks, steam engines, telegraph systems, computers, and servo (or model-based ;-) control systems. All these paradigms provide a perspective; none provides the "truth". Let's be history-aware (what will they say about our beliefs, five centuries from now?) and modest...

>I think we should use the concept of model-based control sparingly,
>where it is clearly required to explain phenomena, and keep the
>simpler model where it seems adequate (and where dealing with
>unexpected disturbances is taken care of without an encyclopedic
>model of the entire physical universe).

When would that "sparingly" be? When would it be "clearly required"? What is the context in which it would fullfil a meaningful role? Why is there so much emphasis on (Trivial Pursuit-like) encyclopedic "knowledge" of the entire physical/social/psychological universe in our society?

Thus far, you have been pretty unclear about when it would serve the larger scheme of things (successful control) to collect and store knowledge about the world, and what exactly the impact of such knowledge might be on the quality of our behavior. That issue needs to be resolved, I think. And note that all knowledge, even if accurate at the time it was stored, runs the risk of being outdated at a later time, when it might be used. PCT focusses on "living in the here and now", and that is fine. But we live in the past in so far as we have collected and now use large amounts of "knowledge" derived from our personal experiences, all of it inaccurate and much of it outdated. And we live in the future in so far as we have collected a number of life-time goals that now determine much of what we do. In order to understand people (and ourselves), we need better models of how our personal past, present and future hang together.

>A case in point is your recent post on transfer functions. You
>propose putting a transfer function in series with the reference
>input. ... How would a living system perceive that the direct
>reference input is giving a wrong result, and adjust the
>compensation until the result is better?

As the song says "I want it, and I want it now!" In many cases, the notion "better" seems to imply "fast" or "as fast as possible". In a control system that would be a perception that rapidly and accurately tracks reference level changes (I feel hungry and therefore I want to eat _now_, not tomorrow). How to adjust the pre-compensation TF1? By the usual mechanisms of random trial-and-error and hill-climbing, I guess. For the sake of efficiency preferably the latter.

>You asked where the world-model is in the system I sketched in for
>mimicking the behavior in Vancouer's "spiral" experiment. There
>isn't one, unless you want to stretch the definitions and say that
>it's implicit in the various leaky integrators and in the
>relationships among the levels of systems.

That is _not_ necessarily stretching the definitions, I think. In

engineered designs, it depends on whether the result of learning is laid down in hardware or software. In the nervous system these two seem difficult to distinguish.

>That, of course, makes any control system into a sort of model of
>its environment, but I don't think that's what you mean by a
>world-model.

That would be a hard-wired world-model. But large changes in wiring seem to take long (evolutionary) times and don't apply to the individual (except in freak accidents such as a non-lethal bullet in the brain). So you are right: when I think of a world-model, I usually think of rapid, incremental real-time learning and (software-like) adjustments of model/control parameters.

>My main purpose in showing that model was to show how a system could
>be built without a world-model that would still be able to continue
>control of the derivatives for some time after loss of the position
>perceptual signal.

Great. But through which mechanism was "control of the derivatives" decided upon? Why derivatives and not some other function? Which derivatives? And multiplied by what factors? My point is: you, the designer, were able to discover how to design such a system. Could some mechanism within the system itself do the same? Which properties would such a mechanism need to have?

>>We humans like predictability a lot, and we pretty much organize
>>our world to be predictable. No shape-shifting houses, cars, roads,
>>etc. Even education/enculturation has as its main goal to get
>>people to behave predictably.

>Speak for yourself, Hans. I think that a person who really loves
>predictability will also love your world-model approach. But I think
>that such a person will also tend to minimize the variability of the
>world, because in fact we all manage to behave successfully by
>_varying_ our actions as appropriate to the immediate environment,
>and this doesn't fit a conception in which everything is understood
>and planned out in advance.

How come we vary our actions _appropriately_? How come we often know immediately what to do, and do not need to fall back on random trial and error time and again? It is because the world has been observed to have certain regularities and because we believe that those regularities will remain reliably the same, without us needing to reestablish their constancy time and again.

>Planning is a highly overrated activity; if you put too much faith
>on planning, you are likely to say that whatever happens is what you
>intended, whether it actually was or not.

Planning takes place in all our activities all of the time. If I need to grab something, I know -- unlike a newborn baby -- that I need to use my arm muscles rather than my facial muscles, say. You make planning a very high-level concept, which I think it isn't. I think of it as knowing _which_ tools (lower hierarchical levels) to use as the means to reach a certain goal. In HPCT, this side of planning has become invisible once the hierarchy is fixed and the parameters of the perceptual in- and output functions have stabilized.

I'm not sure any of this helps...

Greetings,

Hans

Date: Tue, 10 Dec 1996 13:44:47 +0100
From: "J.A. Blom" <J.A.Blom@ELE.TUE.NL>
Subject: Re: simultaneity

[Hans Blom, 961210b]

(Bill Powers (961204.1010MST))

>>>Flipping a light switch is not supposed to be followed instantly
>>>by a loud bang. In fact, it is supposed NOT to be followed by
>>>anything but a light turning on or off.

>>I would call that a mental model: in my experience, flipping a
>>light switch does not cause anything but turn a light on or off
>>(this may not be true for some electrical engineering students).

>You can see that as a mental model, or simply as a perceptual
>signal. The perception would be that of a particular sequence:
>switch, light. The reference condition is to perceive that sequence.

I think that is stretching the meaning of perception. We're talking here about a process that unfolds over time. The action "switch" (confirmed by a perception "switch") is linked with an expectation "light" and another expectation "nothing else". If both expectations are subsequently perceived to be correct, the underlying cause-effect model is correct. If not, then not.

>But this isn't a world-model in the same position as in your model,
>running from output to input via an imagination connection.

Yes it is, accurately. "Switch" is an action, an "light" is the predicted consequence -- anticipation -- of that action. That anticipation is internally generated. If action "switch" does not result in "light" and nothing else, our anticipation is incorrect; we experience surprise and generally will want to find out why our prediction was incorrect; that leads to updating the model or possibly even an entirely new model.

>It involves real-time perceptions from the outside world, not
>imagined perceptions from the inside model.

If the model were perfect (which it is not and cannot be), the imagined perceptions would be accurately in step with the real-time "perceptions" of the world (whether perceived or not). In that case, it would not matter whether we control on the basis of real-time or imagined perceptions. Since the model is not perfect and the world changes, the model must be recalibrated regularly. This is particularly acute when predictions turn out to be incorrect.

>Of course in your model you still have to explain how it is that

>the person perceives sequence as a variable that can be correct or
>incorrect.

An incorrect prediction automatically leads to readjustment of the model. The standard adaptation mechanism takes care of that. Small prediction errors are normal, and they hardly deteriorate the quality of control. Detection of large prediction errors indicates that the model isn't even approximately correct; in such cases, learning -- building a new model -- may be forced to restart from scratch. This latter strategy is unpopular in humans. And entirely impractical: it would discard the entire existing knowledge base. Since it is often impossible to decide immediately which knowledge is still correct and which not, this is a critical situation which is difficult to handle by simple adaptive techniques.

Greetings,

Hans

End of CSGNET Digest - 8 Dec 1996 to 10 Dec 1996
****

Date: Wed, 11 Dec 1996 08:00:04 -0600
Subject: CSGNET Digest - 10 Dec 1996 to 11 Dec 1996

There are 17 messages totalling 1676 lines in this issue.

Topics of the day:

Date: Tue, 10 Dec 1996 15:32:56 +0100
From: "J.A. Blom" <J.A.Blom@ELE.TUE.NL>
Subject: Re: Ahhh, epistemology at last!

[Hans Blom, 961210d]

(Tracy Harms 961209.13)

>When conversations like this arise, I find myself wondering about
>cost/benefit ratios.

Me too. The greatest benefit to me in this (sometimes hostile ;-)

environment is that I am forced to (re)consider what my own point of view really is and how robust it is in the face of "disturbances" of others. My greatest disappointment is that I am often unable to clearly put my thoughts into words that can be understood by others who are not familiar with the (solid, I think) origin of my beliefs and convictions. Words are only words, and far too many would usually be required to make myself understood (whatever that is). A book, maybe one of these days...

>Regarding 'fact' and 'truth', and whether to ''place [these] in the
>"real world" domain'', there are some semantic differences.

Yes, aren't there always?

>But even if I simply take the words as you wish to use them, if you
>say facts (etc.) exist in the real world, and there are no facts,
>the implication is that there is no real world.

That is not _my_ implication; I don't consider myself a Berkeleyan. Much, but definitely not everything, of Berkeley's world view rests on semantic confusion. What, for instance, do we call a "sound" that no-one hears? In a sense, Berkeley pre-thought what physics now calls the measurement problem where the ultimate question is what/whether we can know about the world if we don't perceive/measure. My point of view -- and that implemented by model-based control -- is that the laws of the world continue to reign even if nobody watches.

>In that case, I'm not sure that we can fruitfully discuss ANYTHING.

That may have other reasons ;-).

>The consequences of this world-view are to my mind entirely
>incoherent. So conceptualized, argumentation either defies minimal
>conformance to logic, or logical implications have no persuasive
>strength. All bets are off; irrationality reigns.

Which logic? The logic that I consider most useful and most realistic is probabilistic logic, where conclusions are at best _almost_ true. Black-and-white yes/no types of logics are useful as well (and far simpler), but their domain of applicability is much smaller. Even where I use them (in my expert systems) for reasons of simplicity and speed of inference, I use a logic that is three-valued, with values true, false and unknown, the latter being indispensible (because of the ubiquity of missing data) but also the most difficult to reason with. It turns out that the notions "true" and "false" are valid only within a carefully specified context. When one enlarges the context, what was previously true or false often becomes unknown. When one enlarges the context to include everything that is or could be, probably no "true" or "false" would survive. A simple example: the definition "a tiger is a large, tawny, black-striped cat-like mammal" may lead to a unique identification in a small zoo but not in a large one. In a large zoo we would need a more extended definition. And all bets for a good definition are off if there are earth-similar life forms on many other planets in the galaxy. Translate "definition" into "perceptual input function" and you see the relation to PCT.

>And as Rick pointed out, then decision can only be finalized by
>force.

What do you mean by that? Who forces whom to decide what?

Probabilistic logic says -- and I think -- that every decision is somewhat like a gamble: we always reach our conclusions (or "compute" our actions) in the face of insufficient or not fully reliable data. Wasn't that one of your quotes?

>>When someone talks about his/her discovery of a truth/fact, I hear
>>the proposal of a research program, not the end of one. And I
>>wonder how accurate the statement is and where it will meet its
>>boundaries of applicability.

>That paragraph, however, I find entirely acceptable. I would add
>that a discovery may easily entail the end of one research program
>at the same time that it is the inception of another.

Certainly.

>On a side matter, there is an approach to physics where c is
>established as a basic unit, and thus it simplifies the formulae
>just as you indicate. This is in fact used by physicists.

Tell me more about that or give me a reference. I thought I made something up, just as an example. I'd like to know more about the "alternative world (view)" that this particular unusual coordinate system might lead to.

>Please tell me what your position is on the relationship between the
>"coloring" of our perceptions and the accuracy of our knowledge
>and/or control.

>OK, I will, but I think I'll post this message as is before I
>attempt to compose something of that difficulty.

Ack.

Hans

Date: Tue, 10 Dec 1996 09:46:19 EST
From: Bruce Gregory <gregory@CF4.HARVARD.EDU>
Subject: Re: Effects and side effects

[From Bruce Gregory (961210.0945 EST)]

Rick Marken (961209.1510)

> It's wonderful when a group of living control systems can come to agreement
> about the appropriate state of some perception. Unfortunately, such
> agreement cannot be encouraged; it can only be hoped for because it depends
> on the selection of reference specifications by systems who set their
> references completely autonomously; ie. all ya can do is hope that what
> everyone wants turns out to be what's OK for all.

I'm more optimistic than you are about this. I think one key lies in Bill's persuasive argument that we all do well to avoid generalizations such as one individual controlling another and focus on exactly what variable is being controlled. The more

specific we are, the more like we are to make progress and avoid unproductive side-trips. (I know this is a lot easier said than done. I'm just taking an opportunity to seize the high ground ;-)

Bruce Gregory

Date: Tue, 10 Dec 1996 10:16:20 -0700
From: Bill Powers <powers_w@FRONTIER.NET>
Subject: Re: CRITICAL-CAFE: Popper & facts

[From Bill Powers (961210.0630 MST)]

Tracy Harms (961209) --

I'm taking this segment public because it relates to your public post.

>I've been pecking at some replies to your questions, but one I find
>especially hard to reply to:
>
>>Is this a proposed model, or is this a statement about how Reality is?
>
>I'm getting something similar from Hans, it seems.
>
>To help you understand how this leaves me perplexed, perhaps I should ask
>you "Is HPCT a proposed model, or is this a statement about how Reality
>is?" I don't see how it could be anything but *both*. If HPCT does not
>apply to the real world, it isn't worth my time. If I want fantasy I'll
>re-read Beowulf or Lord of the Rings.

So your problem now becomes "Is PCT a statement about reality?" You have to find an answer in order to know whether you're wasting your time on a fantasy. While I would really like to tell you "yes, it's about reality," unfortunately all I can say is that I _hope_ it's a statement about the real world, and that there seems to be _evidence_ that it's about something real, and that I'm prepared to _act_ as if it's correct in at least some of its details. I might even say there are brief periods when I _believe_ that PCT is about reality, but those periods are necessarily brief because as soon as I realize that the truth of PCT rests on my belief I realize that I could equally well believe something else (because there are others who actually do believe something else). I'm thus working under a handicap: I might believe that PCT is about reality, but I seem unable to forget that it was I who decided to believe it.

I guess what I'm saying is that I don't think there there is any way to answer the basic question of epistemology except to declare faith in some answer, and then forget that you were responsible for the declaration.

Tracy Harms 1996;12,09.16: --

In replying to Hans, you said

>What you say here does speak to
>involvement with a wider world, the whole of which is real and not to be
>conflated with any given thinker. That is the ante which makes further
>discussion worth attempting. By guessing that there is a real world you

>certify your intention to not let things devolve into irrationality (the
>result of which I warned about last time).

The "ante" of which you speak is a committment to a certain belief, by which, as I said above, I mean a belief which you have decided to accept as certain, implying that you have also decided to forget that it was your own act that made any doubts disappear. That, however, is the ultimate irrational act as masters of theological rationality know full well. The moment you assert that there is _evidence_ for anything you say, certainty goes out the window along with true faith. What convinces you will not necessarily convince someone else because different people put different interpretations on the same evidence, and evidence is never complete. Faith is achieved in a single step, or by accident (or through grace). It's an act of will, not of reason.

My watered-down epistemology is not very satisfactory if what you are after is justified certain knowledge.

You say

>Organisms are isolated in a very important manner: the isolation is in
>regard to learning. Recognition of that isolation has been an important
>advancement in our understanding. Unfortunately this very insight has led
>o a mistaken idea whereby organisms are thought of as epistemologically
>isolated from their environments. But only in learning are we isolated so;
>in the processes of knowing we and the world we know are unified.

The term "isolation" is too strong for me; it implies a complete cutoff. I think there's a difference between suspecting very strongly -- even having faith --- that an external reality _exists_ and having faith that what we know about it is true. Perhaps this is what you mean by "learning" -- the _content_ of what we know. People "knew" for 150 years that combustion is caused by the release of phlogiston; now nobody "knows" that any more. For me, this kind of thing greatly weakens the idea that what we know has something directly to do with the properties of the real world, even though I have no doubts any more about the basic existence of such a world.

You say " Once you have knowledge, said knowledge is the 'bridge between the worlds' as it were." There is a sense in which I can agree with you, although you may not want to accept this sense. Under the assumption or faith that a real external world exists, it follows (from our received knowledge) that perceptions must be functions of happenings in that world. The problem is that given the set of all sensors at the interface with that world, there is a multiple infinity of functions that can use the sensory signals as arguments to generate a perceptual signal. The nature of these input functions determines _how_ the world of perception depends on the external world. Except for inherited similarities of neural function, there is no _a priori_ reason why any two organisms should perceive the same world, even if there is only a single reality (a matter of definition, of course).

A long long time ago, Don Campbell consulted with me on the subject of "lens grinding." He knew I was an amateur astronomer and wanted me to explain how lenses are given the right shape. I told him how by grinding one piece of glass back and forth against another in a randomized way, the overhanging piece could be given a concave shape while the piece underneath -- the "tool" -- took on the complementary convex form, both forms being nearly spherical since that is the only shape that allows equal contact to exist everywhere despite translations. I think this was about the time that

Campbell came up with his "lens-grinding" analogy for learning or adaptation. It was his idea, I believe, that by rubbing against the environment, the organism shaped its perceptions in the only way that would fit everywhere with the effects of the environment on it. This, of course, would eliminate most of the physically possible perceptual functions, leaving only those that were both mutually consistent and consistent with the properties of the world at all points of contact..

There are two problems I can see with this idea. The first has to do with degrees of freedom. In the lens-grinding example, we are speaking of only three degrees of freedom for both the tool and the lens or mirror (which workpiece is considered the tool depends on whether you're making a refractor or a reflector!). But in the interface between the perceptual systems and the outside world, the "tool", the environment, most probably has enormously more degrees of freedom than the set of all input receptors has. The "fit" that is achieved is only in terms of the limited set of points of contact, leaving everything between those points ambiguous. This means that there is an unlimited number of forms that the external reality might take on, all of which would receive the same perceptual representation (as there is an infinity of combinations of wavelengths that lead to the same perception of color).

The other problem is that Campbell overlooked (I think) the effect of the lens on the tool. In lens-grinding, there is not a single form of the tool to which the lens is made to conform. Both lens and tool begin as flat pieces of glass. Both lens and tool alter their shapes under the interaction, with the final result being a product of the lens-grinding process _on both sides of the relationship_. The organism, in rubbing against the environment, alters the form of the environment at the same time it is altering its own perceptual organization. The result is that both organism and environment change to fit each other in terms of the existing points of contact. What we end up with is not truth but mutual consistency. (Or course, maybe I'm just now catching on to what Campbell really meant. It's still hard to realize that I can't e-mail him to find out his answer).

My curse, which was laid on me when I began looking into levels of perception, is that I can no longer indulge in arguments like those above without inadvertently wondering "what am I doing now, and how does this fit with the model?" All that I have said above is the result of carrying on certain mental processes, many of them symbolic. And where, according to my model, is all this going on? Primarily, at what I call the ninth level of perception and control. In other words, in my head, on THIS side of the sensory interface, not THAT side, the other side we would all like to talk about.

Paradoxically, being reminded that it is an "I" who is trying to construct a consistent picture of my relationship to the world makes my sense of the existence of that world much sharper. It's like saying "Yes, the world is a black box (hence the CSG logo), but _there's got to be something inside it to make it behave that way_." Being aware that all I know of that world comes to me as perceptions, including my actions on it, makes it ever so much clearer that I do NOT know why it is that creating certain actions has certain reliable effects on my perceptions. I cease to take these links from my outputs to my inputs for granted, and begin to wonder what sorts of mechanisms might be hidden out there that would explain these effects.

It seems to me that this is the entire enterprise of the physical sciences -- trying to come up with mutually consistent proposals about what might lie between our experimental manipulations and the subsequent effects on our

sensors. It is perfectly obvious in these realms of science that we have no _direct_ way to determine what is Out There. All we can do is build and test models of what MIGHT be Out There: if we could "just look", we would. What has made these sciences so successful is the refusal to accept any model that fails to predict correctly within our current limits of measurement. This means that it becomes very, very hard to think of an alternative model that would predict as well. In fact, the physical sciences are always crowded up against the frontiers where nobody can think of a better model.

The other side of this is that the models which survive are so good that one is hard-pressed to believe that they do NOT in some way, to some degree, reflect actual processes in the world we can't directly observe. But we are always separated from reality by that final fraction of a millimeter that we can't cross. The one step that is denied to us is that tiny step that would take us, naked, into reality itself where we could see, feel, touch, and hear the Dingen an Sichem that we have only been guessing about. We simply can't conceive of that world except in terms of seeing, feeling, touching, and hearing, and the moment we realize the terms in which we are imagining doing this, we realize that we can never take that step.

It is very understandable that this tantalizing situation proves too much for some people to stand, so they say "To hell with it. What I perceive is the world itself, and that settles that." This act of faith erases their frustration at a single stroke, and from then on they have no more uncertainty about the nature of experience than a dog has.

My epistemology, therefore, has nothing to do with justifiable certain knowledge. I suppose you'd use a term like "pragmatist" for me, although I don't know all those multitudes of terms for philosophical positions. I think in terms of propositions and evidence for and against them, and the devisement of experimental tests that might help us pick one proposition as preferable to another. For me, the world remains a black box, and my perceptions remain in an unknown relationship to that world.

Best,

Bill P.

Date: Tue, 10 Dec 1996 18:24:44 +0100
From: "J.A. Blom" <J.A.Blom@ELE.TUE.NL>
Subject: Re: knowing the real world

[Hans Blom, 961210f]

(Tracy Harms 1996;12,09.16)

>In my last response I played a harsh interpretation of your words.
>I now think I should give you more benefit of the doubt by paying
>attention to your attempt to explain, above.

This time I somehow feel much better understood...

>By guessing that there is a real world you certify your intention
>to not let things devolve into irrationality (the result of which I
>warned about last time).

I'm not quite sure what you mean by (ir)rationality. One thing that

I'm acutely aware of (coming from the practical/engineering side) is how important the timeliness of decisions is. An intensive care patient will be dead within three minutes after a major calamity, for instance. A "rational" alarm system must thus come with an alarm signal/message within one minute, preferably less, in order to be useful at all. More generally, can we also expect "rational" decisions when the decision-maker (e.g. a control system) is in a hurry? I remember reading this crazy philosopher who says that it is sometimes the most rational to be irrational, e.g. flip a coin and leave everything to chance. In fact, predicate logic is only semi-decidable, which means in practice that it takes an unpredictable time (up to infinity in a complex system) for inferencing to derive a conclusion from the premises. That, surely, is no acceptable logic for a "rational" real-time agent such as a control system!

Even rationality is relative to a set of axioms, and it has become clear by now that ours are not the same. In a real-time agent such as a control system or an organism, the timeliness of decision-making is an essential aspect, and I don't even know whether that can be captured in axioms. So we may have to depart from standard logics and standard concepts of rationality here...

>The knowledge which an organism has is indeed 'opinion' in your
>sense of that word: it is a fallible approximation by guesswork.

Yes, that's what I attempted to say.

>But insofar as these opinions (items of knowledge) do not fail, it
>is that world itself which we know, contrary to your indication that
>all we can know is observations.

Your "do not fail" tells me that you believe that an opinion either fails or it does not. Is that correct? I hope not. I think that there are _degrees_ of failing; no "opinion" is 100% "true", although one might come close to this mark -- which, of course, we strive for, both in science and in daily life.

>In this message I will argue against what I see as the central
>metaphor of your vision: the isolation of the organism from the
>world.

I do not see that as the central metaphor of my vision; far from it. You express my vision well when you say:

>Perception is a feedback phenomenon, and in feedback every component
>of the circuit is involved. A portion of the circuit we naturally
>think of as outside the organism, and it is. BUT IT IS NOT OUTSIDE
>THE CIRCUIT.

I have often vocalized this point of view, in various terms, although maybe not as well as you do here. It is the control _loop_ which determines the "behavior" of the controller/organism. It is the _loop_ which determines whether a system is even a controller at all (in contrast to, e.g., an oscillator). Feedback loops have as their purpose to make "weak" (compliant, variable, unreliable) linkages "rigid" (reliable), as in an operational amplifier or audio system, where the feedback results in fixed, reliable characteristics of the system as a whole despite the tolerances of many of the components. In a sense, we make the world part of ourselves, due to the feedback

loop. We are us and we have our behavioral characteristics only because there is that complement, the world. And in all aspects of our behavior we "mirror" the world. We cannot know the world, but we _can_ know (partly, approximately) how we interact with the world. And thus "see" the world, as through a mirror darkly, in what we want, believe, and do.

Greetings,

Hans

Date: Tue, 10 Dec 1996 11:34:06 -0700
From: "T. B. Harms" <tbh@TESSER.COM>
Subject: control? WHAT is under control?

>From Tracy Harms (1996;12,10.11:30)

I concur with Bruce Gregory's optimism (961210.0945 EST) that agreement can be actively promoted by establishing a prior agreement on goals which themselves facilitate rational discourse. He gives an example:

>I think one key lies
>in Bill's persuasive argument that we all do well to avoid
>generalizations such as one individual controlling another and
>focus on exactly what variable is being controlled. The more
>specific we are, the more like we are to make progress and avoid
>unproductive side-trips. (I know this is a lot easier said than
>done. I'm just taking an opportunity to seize the high ground ;-)
>
>
>Bruce Gregory

I myself am itching to apply this to my strongest area of interest, applied political philosophy. There are endless invocations of "control" in political discussions, and almost without exception they are hopelessly vague. From academics on one Internet list I follow I recently saw assertions that economics is only sound (and/or scientific) if it leads to or allows control of the economy. In other discussions I've seen accusations that various organizations or elites control people, or control the media, or control our ideas, etc. Thanks to PCT it is suddenly very clear that these sorts of musings are unworkable. I'm not one to expect that adopting technical semantics for terms will solve genuine problems, but in this case I think it does. By consistently thinking of control in the technical sense we can correct such statements straightaway, and the process of looking for unidimensional aspects which might actually be attempted to be controlled (for example, the reported rate of inflationary price increases) any topic naturally becomes clarified.

Tracy Bruce Harms tbh@tesser.com
Boulder, Colorado

"I didn't think it was possible -- humanity is actually getting dumber!"
The Brain

(Brain is a cartoon character, a genetically-enhanced laboratory mouse
who is bent on taking over the world. Your basic CONTROL freak...)

Date: Tue, 10 Dec 1996 09:39:49 -0800
From: Richard Marken <marken@AEROSPACE.AERO.ORG>
Subject: Model-based control, science, agreements

[From Rick Marken (961210.0930)]

Oded Maler (961210) --

>Take for example the existence of something like "the position of a cursor
>on the screen" - does it have an objective meaning or is it relative to
>the perceptual world of certain experimenter?

It's _all_ perception. So?

I observe that (perceived) cursor movements are less when a (perceived) controller is (perceived) to be manipulating a (perceived) mouse that (is perceived to) influence the cursor. I can explain all these perceptions with a (imagined and perceived) model that assumes that the (perceived) person is controlling a perceptual representation of the distance between a (real) cursor and (real) target.

>In another reply, Rick said something that the scientific method suggests
>an alternative to "might is right" in deciding what is the better thing
>to do. This does not seem to be the case in the world we live in, neither.

It doesn't _suggest_ it; it _provides_ thje opportunity for it. Science makes it _possible_ for people to resolve differences in beliefs (if those beliefs are properly formulated as working models) by submitting those differences to empirical test. People have to _want_ to use this approach. But if they don't want to use this approach, science won't _make_ them use this approach.

>Funny, there is here a meta-level paradox. According to this view, the
>scientific method is a kind of model-based behavior that humanity
>(should) employ, but since model-based behavior does not exist in the
>individual, how are we to expect it to emerge for the whole humanity?

Science does, indeed, have superficial similarities to the kind of _model-based_ control that Hans describes. Like Hans' model, the scientist acts on the environment and on a model of that environment, monitors the results of those actions on both environment and model and (if there is a discrepency) adjusts the model so that the model's response matches the environment's response to actions.

One problem with Hans' _model-based control_ theory (from my perspective) is that it says that _all_ control (from control of the position of your slippers to control of your position on model-based control) occurs in this model-based fashion. I claim that there is no evidence that _any_ behavior is model-based -- even the behavior we call "doing science".

In PCT, the appearance of "model based" behavior (like "doing science") results from the operation of perceptual control systems at many levels of a hierarchy of control systems, none of which operate on the basis of model-based control. A scientist is controlling a system concept perception (science) by controlling for perceptions of principles (like "careful

observation", "skepticism", etc); these principles are controlled by controlling programs of activities (the research process) which are controlled by controlling for relationships (mathematical, qualitative, etc), sequences, etc.

Some of the perceptions controlled in the process of doing science are controlled in imagination; when we "think of a model, how it works and what it implies about what we will observe" we are producing perceptions for ourselves via imagination. Imagination is _not_ model-based control; it is good old PCT with the control loop closed inside the system (the output of the control loop is replayed directly back into the perceptual input function - - so the system perceives exactly what it wants - - rather than through the environment (where all those pesky disturbances require that you _vary_ control outputs in order to get the higher level perceptions you want).

Me:

>...all ya can do is hope that what everyone wants turns out to be what's OK
>for all.

Bruce Gregory (961210.0945 EST) --

>I'm more optimistic than you are about this. I think one key lies in Bill's
>persuasive argument that we all do well to avoid generalizations such as one
>individual controlling another and focus on exactly what variable is being
>controlled.

I agree that this is a very good approach. My pessimism comes from experience. B:CP was published 23 years ago; I don't think there is a clearer description of a model of behavior (with few generalizations). Nevertheless, after all this time there are still only a handful of behavioral and life scientists who are practicing perceptual control theorists. Clarity is great but it doesn't _necessarily_ lead to agreement -- especially when you are presenting ideas that people clearly don't _want_ (for whatever reason) to agree on. I wish it were different, but it's not.

Best

Rick

Date: Tue, 10 Dec 1996 13:54:29 -0700
From: "T. B. Harms" <tbh@TESSER.COM>
Subject: knowledge and rationalism

>From Tracy Harms 1996;12,10.13:30

Bill Powers (961210.0630 MST)

>My watered-down epistemology is not very satisfactory if what you are after
>is justified certain knowledge.

That's not what I'm after, and that's not a problem. What I see as a problem is that your corrections are something you think of as a watering-down. You have misidentified a strengthening as a weakening.

>[...]

>My epistemology, therefore, has nothing to do with justifiable certain
>knowledge. I suppose you'd use a term like "pragmatist" for me, although I
>don't know all those multitudes of terms for philosophical positions. I
>think in terms of propositions and evidence for and against them, and the
>devisement of experimental tests that might help us pick one proposition as
>preferable to another. For me, the world remains a black box, and my
>perceptions remain in an unknown relationship to that world.

You are so close, Bill, but one glaring flaw remains in your epistemology, and that flaw is that it has far too much to do with justifiable certain knowledge! That you intend to purge this blot is most encouraging; I'll be happy to help if I can.

In what way have you accepted the epistemology of justified certain knowledge? You have done so by granting the very concept of knowledge to it. This concession to that view of knowledge is heavy in your statements, for example: "But we are always separated from reality by that final fraction of a millimeter that we can't cross." Here you reiterate what I deny, the barrier metaphor which misapplies isolation by misconceiving knowledge. By allowing 'knowledge' to mean justified true belief you catapult toward skepticism, for as you assert *knowledge which fits this vision of knowledge is unobtainable*. Therefore, if this is knowledge there is no knowledge; we can know nothing. Such a conclusion is both unacceptable and unnecessary. All we need do is insist that this familiar word, knowledge, indicates something real and important, but that since whatever it is it clearly cannot be justified certainty, it must be something else.

You should no more accept that knowledge is justifiable certainty than you would accept that behavior is the consequence of stimuli. Never abandon key terms to proponents of inferior theories.

This concession you've carelessly made to justificationists is coupled with an even larger problem, a concession to a justificationist vision as to the nature of rationality. This is revealed in your statements:

>I guess what I'm saying is that I don't think there there is any way to
>answer the basic question of epistemology except to declare faith in some
>answer, and then forget that you were responsible for the declaration.
>[...]
>The "ante" of which you speak is a committment to a certain belief, by
>which, as I said above, I mean a belief which you have decided to accept as
>certain, implying that you have also decided to forget that it was your own
>act that made any doubts disappear.

You are promoting Hume's advice as to how to approach rationality in the wake of glimpsing that induction cannot provide infallible certainty. But that is not a solution to the crisis of rational integrity, it is an evasion. Fortunately somebody else has done the hard work on this problem, and a genuine solution is available ready to hand. Bill Bartley solved the crisis of rational integrity back in the early '60s, and his work and its implications are, to my mind, vital components to a thoroughly Darwinian world-view. (_The Retreat To Commitment_, William Warren Bartley III, 1984 (2nd ed.), Open Court Press, LaSalle)

In keeping with evolutionary rationalism I do *not* speak of a committment to a certain belief. I do not decide to accept anything as certain, so there is nothing for me to forget; no skeletons in my closet, no dirty little secrets of irrational leaps of faith, no actions by which I make any

doubts disappear. My doubts are not to be banished except as a spontaneous consequence of considering well-argued theories. Those which do not perish so shall remain part of me, never to be refused, not denied nor renounced--they are appreciated.

The basic explanation as to why evolutionary rationalism may (and must) accept uncertainty in regard to logical fundamentals, whereas justificationist (conventional) rationalism is forced to deny uncertainty in regard to them, is because justificationists rely on those items as foundations which must bear the logical weight of other knowledge, with transmission of truth included in this dependency. Evolutionary rationalism, in contrast, does not rely on some pool of certain truths, but instead relies on a diversity of uncertain untruths. The advancement through double-negation which was proposed by Darwin in regard to speciation is equally applicable to all developments of fit. Things get good by discarding the bad, not by passing some sort of torch from a Pure, Absolute Goodness into the more ambiguous world.

With evolutionary rationalism no disaster occurs if there are doubts and uncertainties involving critical components, because soundness of knowledge and participation in rational methods have both been logically segregated from any matters of belief or confidence. I'm not going to spell out the details on this. Those who are curious should read Bartley's book.

Thanks for your many other comments, they are thought-provoking. I share your sadness at Donald Campbell's death. Although he never so much as knew I am alive, his work lives on in me, as this very posting exemplifies.

Tracy Bruce Harms tbh@tesser.com
Boulder, Colorado

"The life of humanity is so long, that of the individual so brief, that we often see only the ebb of the advancing wave and are thus discouraged. It is history that teaches us to hope." -- Robert E. Lee

---

Date: Tue, 10 Dec 1996 17:39:05 -0500
From: Jeff Vancouver <jeffv@PSYCH.NYU.EDU>
Subject: Re: Memory switch

>From Jeff Vancouver (961210.17:40 EST)

> >From Stefan Balke (961210.1030 CET)
>
> Jeff,
>
> I tried to understand your mastermind model, but I have only vague memories
> at this game. I'll ask some friends here for the rules. Because of this
> limited background I didn't understand much of your model, however I grasped
> the idea, that there is a ECU analysing the error state of another ECU and
> if an error is detected than the system switches to the imagination mode
> until the reorganization process leads to a sufficient decrease of the error
> and to a switch back to the control mode. Is this an appropriate description
> of your idea and what did the others say to it? For me it seems to make sense.
>

That is basically it. The only slight alteration is that the system can

use input from the environment to test the result of reorganization, not simply memory as the imagination mode implies. Nonetheless, the output switch is not set to the environment until a reorganized output passes the test.

No others have commented on it. I think they are having the same problem as you. They are not familiar with the game.

Jeff

Date: Tue, 10 Dec 1996 17:46:47 EST
From: Bruce Gregory <gregory@CF4.HARVARD.EDU>
Subject: Re: knowledge and rationalism

[From Bruce Gregory (961210.1745)]

Tracy Harms 1996;12,10.13:30

Bill Powers (961210.0630 MST)

I think my views are consistent with both of yours, but let me phrase mine slightly differently and see your reactions.

I don't think that we infer the existence of a real external world. I think we are born into a real world that we struggle to control. (If this world were not external and shared, it would be a _lot_ easier to control.) As part of this process we invent models that allow us to predict the outcome of our interventions. PCT is an example of one such model. PCT is not a part of our process of controlling, but explains _how_ we are able to control and allows us to plan more successful interventions.

I agree with Tracy Harms,

> In what way have you accepted the epistemology of justified certain
> knowledge? You have done so by granting the very concept of knowledge to
> it. This concession to that view of knowledge is heavy in your statements,
> for example: "But we are always separated from reality by that final
> fraction of a millimeter that we can't cross." Here you reiterate what I
> deny, the barrier metaphor which misapplies isolation by misconceiving
> knowledge. By allowing 'knowledge' to mean justified true belief you
> catapult toward skepticism, for as you assert *knowledge which fits this
> vision of knowledge is unobtainable*. Therefore, if this is knowledge
> there is no knowledge; we can know nothing. Such a conclusion is both
> unacceptable and unnecessary. All we need do is insist that this familiar
> word, knowledge, indicates something real and important, but that since
> whatever it is it clearly cannot be justified certainty, it must be
> something else.

The notion that "we are separated from reality by that final fraction of a millimeter that we can't cross" is not given to us by way of observation, it is a property of our models. Nothing prevents us from trying to build ever more powerful models that allow us to make more and more sense of the world. In this sense, there is no ultimate separation between us and reality.

> You are promoting Hume's advice as to how to approach rationality in the
> wake of glimpsing that induction cannot provide infallible certainty. But
> that is not a solution to the crisis of rational integrity, it is an
> evasion. Fortunately somebody else has done the hard work on this problem,
> and a genuine solution is available ready to hand. Bill Bartley solved the
> crisis of rational integrity back in the early '60s, and his work and its
> implications are, to my mind, vital components to a thoroughly Darwinian
> world-view. (_The Retreat To Commitment_, William Warren Bartley III, 1984
> (2nd ed.), Open Court Press, LaSalle)

I knew Bill Bartley back in the 1970s. I agree that his book is
well worth reading.

> In keeping with evolutionary rationalism I do *not* speak of a committment
> to a certain belief. I do not decide to accept anything as certain, so
> there is nothing for me to forget; no skeletons in my closet, no dirty
> little secrets of irrational leaps of faith, no actions by which I make any
> doubts disappear. My doubts are not to be banished except as a spontaneous
> consequence of considering well-argued theories. Those which do not perish
> so shall remain part of me, never to be refused, not denied nor
> renounced--they are appreciated.

Indeed.

Bruce Gregory

Date: Tue, 10 Dec 1996 17:29:13 +0100
From: "J.A. Blom" <J.A.Blom@ELE.TUE.NL>
Subject: Re: System concepts, effects and side effects

[Hans Blom, 961210e]

(Rick Marken (961209.1300))

>>I do not reject the scientific enterprise

>Then why, after so many years on CSGNet, do you continue to talk
>about model-based control as though it were an important extension
>to (or replacement for) HPCT? You have never presented anything
>other than (questionable) anecdotal evidence ...

Time and again I decide not to talk to you again, yet I break my
decision when I find your contributions worthwhile. Time and again it
is not what you say but the way you say it that disturbs me. By now,
it is clear to me that it is difficult for you to adopt a different
perspective and a "suspension of disbelief", even briefly, which
might let my perspective come through. I have decided -- once again
-- to minimize my exchanges with you. Slowly by slowly I have come to
the realization that they do not seem to serve any worthwhile goal
for me.

>The model-based control program that you sent showed that what is
>being modelled in model-based control is the physical effect of the
>controller on the controlled variable. All this buys is the ability
>to "virtually control" in the blind for some period after loss of

>perceptual input. But this ability is paid for in terms of lowered
>ability to control (resist disturbances) in real time; the longer
>the model can control after loss of perceptual input, the less able
>it is to compensate for real time disturbances when perceptual input
>is intact.

I guess it is these uninformed misunderstandings that I'm tired of.

>Because I am controlling for gaining knowledge based on observation
>and modeling and you apparently are not.

And these.

>I am willing to put my belief in PCT to that [experimental] test

And this. How do you put a meta-theory to an experimental test? I am certain that there is no such test, and thus nothing that can shake your belief in PCT. Or can you think of one?

>You just seem to be defending a system concept that is quite
>different from one that I am defending.

Well, Rick, I guess that most people are. Do they all give you the creeps?

Bye,

Hans

Date: Tue, 10 Dec 1996 11:11:00 -0800
From: "Marken, Richard S." <Marken@COURIER3.AERO.ORG>
Subject: Model-based control, science, agreements

Another errant post; I apologize if this repeats.

[From Rick Marken (961210.0930)]

Oded Maler (961210) --

>Take for example the existence of something like "the position of a cursor
>on the screen" - does it have an objective meaning or is it relative to
>the perceptual world of certain experimenter?

It's _all_ perception. So?

I observe that (perceived) cursor movements are less when a (perceived) controller is (perceived) to be manipulating a (perceived) mouse that (is perceived to) influence the cursor. I can explain all these perceptions with a (imagined and perceived) model that assumes that the (perceived) person is controlling a perceptual representation of the distance between a (real) cursor and (real) target.

>In another reply, Rick said something that the scientific method suggests
>an alternative to "might is right" in deciding what is the better thing
>to do. This does not seem to be the case in the world we live in, neither.

It doesn't _suggest_ it; it _provides_ thje opportunity for it. Science makes

it _possible_ for people to resolve differences in beliefs (if those beliefs are properly formulated as working models) by submitting those differences to empirical test. People have to _want_ to use this approach. But if they don't want to use this approach, science won't _make_ them use this approach.

>Funny, there is here a meta-level paradox. According to this view, the
>scientific method is a kind of model-based behavior that humanity
>(should) employ, but since model-based behavior does not exist in the
>individual, how are we to expect it to emerge for the whole humanity?

Science does, indeed, have superficial similarities to the kind of _model-based_ control that Hans describes. Like Hans' model, the scientist acts on the environment and on a model of that environment, monitors the results of those actions on both environment and model and (if there is a discrepency) adjusts the model so that the model's response matches the environment's response to actions.

One problem with Hans' _model-based control_ theory (from my perspective) is that it says that _all_ control (from control of the position of your slippers to control of your position on model-based control) occurs in this model-based fashion. I claim that there is no evidence that _any_ behavior is model-based -- even the behavior we call "doing science".

In PCT, the appearance of "model based" behavior (like "doing science") results from the operation of perceptual control systems at many levels of a hierarchy of control systems, none of which operate on the basis of model-based control. A scientist is controlling a system concept perception (science) by controlling for perceptions of principles (like "careful observation", "skepticism", etc); these principles are controlled by controlling programs of activities (the research process) which are controlled by controlling for relationships (mathematical, qualitative, etc), sequences, etc.

Some of the perceptions controlled in the process of doing science are controlled in imagination; when we "think of a model, how it works and what it implies about what we will observe" we are producing perceptions for ourselves via imagination. Imagination is _not_ model-based control; it is good old PCT with the control loop closed inside the system (the output of the control loop is replayed directly back into the perceptual input function - - so the system perceives exactly what it wants - - rather than through the environment (where all those pesky disturbances require that you _vary_ control outputs in order to get the higher level perceptions you want).

Me:

>...all ya can do is hope that what everyone wants turns out to be what's OK
>for all.

Bruce Gregory (961210.0945 EST) --

>I'm more optimistic than you are about this. I think one key lies in Bill's
>persuasive argument that we all do well to avoid generalizations such as one
>individual controlling another and focus on exactly what variable is being
>controlled.

I agree that this is a very good approach. My pessimism comes from experience. B:CP was published 23 years ago; I don't think there is a clearer description of a model of behavior (with few generalizations). Nevertheless, after all this time there are still only a handful of

behavioral and life scientists who are practicing perceptual control theorists. Clarity is great but it doesn't _necessarily_ lead to agreement -- especially when you are presenting ideas that people clearly don't _want_ (for whatever reason) to agree on. I wish it were different, but it's not.

Best

Rick

Date: Tue, 10 Dec 1996 16:18:49 -0700
From: Bill Powers <powers_w@FRONTIER.NET>
Subject: Re: Vancouver's experiment

[From Bill Powers (961210.1415 MST)]

Hans Blom, 961210 --

>(Bill Powers (961204.0800 MST))
>
>>It seems to me that your interpretation [prediction by a model]
>>implies a very large expenditure in neurons and computing power to
>>achieve a very small advantage
>
>We humans do seem to have a very large number of neurons compared to
>an E. coli, a spider or a rat. What is our "very small advantage"
>compared to these species? Whatever it is, it seems to reside mainly
>in what neurologists call the association cortex. What would all
>those associations be for?

You misunderstand; I was referring to the very slight advantage, in the case of the Vancouver Experiment, of the postulated model-based control model over a PCT type of control system.

>>, while giving up the ability to compensate immediately for
>>unexpected (unpredicted) disturbances like Jeff's stuck mouse.
>
>If the mouse gets stuck, the loop gain decreases to zero; no
>controller, model-based or not, will be able to cope with this
>situation immediately.

Not true. The response of a simple control system will be to increase the output immediately, which in the case of the stuck mouse ball will usually free it up and permit control to continue. The great advantage of the PCT model is that when an unexpected disturbance does occur, the system will automatically increase or decrease its output to oppose the effects of the disturbance, without requiring any prediction that the disturbance is going to occur.

> What is necessary now is to recognize _that_
>the loop gain is zero, to discover the cause, and to repair the
>situation in order to be in control again. This requires, I guess,
>the formulation of a number of hypotheses of what the cause might be
>(stuck mouse, stuck software, and any other likely possibility), to
>check them, maybe in order of plausibility, and so to discover the
>one single cause. Very difficult for a simple control system

Not at all; it's difficult for the model-based system, because the

model-based system must be modified to include predictions of the disturbance, which in most cases would involve impossibly complex computations encompassing large parts of the local physical world. In the above paragraph you cite many of the reasons for which I find model-based control implausible. You describe what must be done to make such systems work, without apparently seeing how impractical these requirements are for any _general_ model of behavior. You're talking about complex troubleshooting and cognitive processes that are typical of the way a whole intelligent human being might approach a problem with a piece of machinery -- not about the sort of process that would account for walking on a slippery surface or manipulating a mouse on a table with slippery spots.

When I say that I accept the idea of model-based control for some kinds of higher-level control processes, I am going out of my way to make allowances for such possibilities, even though neither you nor I can see how the many difficulties involved in making such systems work would be solved.

>although we humans are likely to repair the problem without much
>thought -- except maybe the first time, when we have no idea _that_ a
>mouse can get stuck.

What's most troublesome for the model-based approach is predicting _when_ it will get stuck. The model has to generate the effect of a stuck mouse just at the moment when the ball in the real mouse runs over a slippery part of the surface. And then, of course, the ball start rolling again and the model has to stop applying the model of the slippage -- again, just at the right time.

>>We can't let a particular engineering approach, developed under
>>conditions that are, perhaps, appropriate for the construction of
>>artificial systems where we have extensive knowlede of the
>>environment and where the objectives are external to the system,
>>dictate a model of the human or animal system.
>
>That argument can be used against any theory-based approach,
>including (H)PCT. Why mention it?

Because the question is whether the model is used because it seems necessary to explain specific observed behaviors, or whether it is used because someone thinks it is elegant and, without proof, assumes that it must therefore be correct.

>>I think we should use the concept of model-based control sparingly,
>>where it is clearly required to explain phenomena, and keep the
>>simpler model where it seems adequate (and where dealing with
>>unexpected disturbances is taken care of without an encyclopedic
>>model of the entire physical universe).
>
>When would that "sparingly" be? When would it be "clearly required"? What
is the >context in which it would fullfil a meaningful role?

A more complex model would clearly be required when a simpler one leaves significant parts of the behavior in question unexplained, or predicts them incorrectly. If a model-based control system could explain behavior that a simpler model couldn't explain, it would not only "fulfill a meaningful role," but would be the only available explanation. We would have no choice but to use it.

>Why is there so much emphasis on (Trivial Pursuit-like) encyclopedic
>"knowledge" of the entire physical/social/psychological universe in

>our society?

Because disturbances arise from the operation of other parts of this universe, and to be predicted they must be continuously derived from events outside the scope of the immediate control problem. In most cases this is impossible, because the data from which such predictions would have to be made is not available. When you walk around the corner of a building, there may or may not be a person walking toward you in your path. You can't predict this event; you simply have to be organized to avoid collisions no matter what you find when you turn the corner. And that requires control based on present-time perception, not on models that require all disturbances to be predicted.

>Thus far, you have been pretty unclear about when it would serve the
>larger scheme of things (successful control) to collect and store
>knowledge about the world, and what exactly the impact of such
>knowledge might be on the quality of our behavior. That issue needs
>to be resolved, I think.

I don't really care about that problem because I don't know enough about higher-level processes in human beings to speculate about it. I know this is a big philosophical deal to you, but it isn't to me.

>>A case in point is your recent post on transfer functions. You
>>propose putting a transfer function in series with the reference
>>input. ... How would a living system perceive that the direct
>>reference input is giving a wrong result, and adjust the
>>compensation until the result is better?
>
>As the song says "I want it, and I want it now!" In many cases, the
>notion "better" seems to imply "fast" or "as fast as possible". In a
>control system that would be a perception that rapidly and accurately
>tracks reference level changes (I feel hungry and therefore I want to
>eat _now_, not tomorrow). How to adjust the pre-compensation TF1? By
>the usual mechanisms of random trial-and-error and hill-climbing, I
>guess. For the sake of efficiency preferably the latter.

That answer is sufficiently vague that it could be applied to any model at all. Conspicuously lacking is any attempt to say what sort of mechanism would DO this hill-climbing. In the Extended Kalman Filter model, at least you supply a specific mechanism to do one interesting part of the job. That mechanism will not handle the discovery and adjustment of the required form of TF1.

>>You asked where the world-model is in the system I sketched in for
>>mimicking the behavior in Vancouer's "spiral" experiment. There
>>isn't one, unless you want to stretch the definitions and say that
>>it's implicit in the various leaky integrators and in the
>>relationships among the levels of systems.
>
>That is _not_ necessarily stretching the definitions, I think. In
>engineered designs, it depends on whether the result of learning is
>laid down in hardware or software. In the nervous system these two
>seem difficult to distinguish.

If you want to say that there's a world-model implicit in the various components of a PCT system, be my guest. But the point is that such a world-model would be distributed over the basic components of the system, and would nowhere exist in a form that predicts the response of the

environment to the actions applied to it. So it would be organized quite differently from the model you have proposed.

>>That, of course, makes any control system into a sort of model of
>>its environment, but I don't think that's what you mean by a
>>world-model.
>
>That would be a hard-wired world-model.

I don't see why you keep asserting that. A PCT-type model can just as easily be reorganized as a model of the form you have proposed. I've shown one or two ways in which this reorganization can be accomplished, although I have no _general_ model that covers all possible kinds of reorganization (and neither do you).

>But large changes in wiring
>seem to take long (evolutionary) times and don't apply to the
>individual (except in freak accidents such as a non-lethal bullet in
>the brain). So you are right: when I think of a world-model, I
>usually think of rapid, incremental real-time learning and
>(software-like) adjustments of model/control parameters.

That's also how I think of the PCT model, although I don't have any neat scheme like your Extended Kalman Filter to implement real-time learning. The PCT model, of course, requires much less of this rapid reorganization. I have no doubt that if I knew enough and were willing to postulate complex enough systems, I could adapt the Kalman Filter approach to reorganization of the PCT-type model, with equally good results -- and without having to assume that an inverse exists for the world-model, or that if it exists, it can be calculated in real time by any realistic neural system.

>>My main purpose in showing that model was to show how a system could
>>be built without a world-model that would still be able to continue
>>control of the derivatives for some time after loss of the position
>>perceptual signal.
>
>Great. But through which mechanism was "control of the derivatives"
>decided upon? Why derivatives and not some other function? Which
>derivatives? And multiplied by what factors?

Why Hans, you know the answer to those questions; you use it all the time. The functions are those that enable the organism to survive! It's EVOLUTION that decides these things! When I ask you where your model gets its world-model, and how it happens that both this model and its inverse are calculated and by what, and where just the right reference signals come from, isn't this the answer that you give me?

>My point is: you, the
>designer, were able to discover how to design such a system. Could
>some mechanism within the system itself do the same? Which properties
>would such a mechanism need to have?

Good questions. Aren't they the ones that we're all trying to answer?

>How come we vary our actions _appropriately_? How come we often know
>immediately what to do, and do not need to fall back on random trial
>and error time and again? It is because the world has been observed
>to have certain regularities and because we believe that those
>regularities will remain reliably the same, without us needing to

>reestablish their constancy time and again.

A PCT-type control system is so organized that it automatically produces the amount and direction of action needed to maintain the controlled variable near its reference level. It doesn't have to stop and think about it, or make predictions, or contain a model for all possible disturbances. It will work about equally well over a range of environmental circumstances without needing any adjustments to its parameters. It doesn't need any _beliefs_ about the constancy of the world; indeed, its design is such as to rely minimally on such (imaginary) constancy. The whole power of the PCT kind of model is that it compares the effects of behavior directly with reference specifications, so it makes little difference if the external world changes by amounts that would invalidate any internal world model and require major adjustments of parameters before that kind of system could work again. The PCT model, precisely because it can function over a wide range of circumstances, requires a minimum of rapid reorganization; what reorganization is needed can happen on a much slower time scale.

>>Planning is a highly overrated activity; if you put too much faith
>>on planning, you are likely to say that whatever happens is what you
>>intended, whether it actually was or not.
>
>Planning takes place in all our activities all of the time. If I need
>to grab something, I know -- unlike a newborn baby -- that I need to
>use my arm muscles rather than my facial muscles, say.

You're not saying that when the baby starts toppling out of its chair, you stop to formulate a plan of action, calculate all the disturbances that will intefere with your action, and only then reach out to grab the baby -- are you? It seems to me that you're only describing what your conception of behavioral organization would call for, and in that very process laying out just what is wrong with it.

Best,

Bill P.

Date: Tue, 10 Dec 1996 16:18:58 -0700
From: Bill Powers <powers_w@FRONTIER.NET>
Subject: Re: simultaneity

>From Bill Powers (961210.1600 MST)]

Hans Blom, 961210b --
>
>(Bill Powers (961204.1010MST))
>
>>>>Flipping a light switch is not supposed to be followed instantly
>>>>by a loud bang. In fact, it is supposed NOT to be followed by
>>>>anything but a light turning on or off.
>
>>>I would call that a mental model: in my experience, flipping a
>>>light switch does not cause anything but turn a light on or off
>>>(this may not be true for some electrical engineering students).
>
>>You can see that as a mental model, or simply as a perceptual
>>signal. The perception would be that of a particular sequence:

>>switch, light. The reference condition is to perceive that sequence.
>
>I think that is stretching the meaning of perception. We're talking
>here about a process that unfolds over time.

It is precisely the kind of thing I mean by perception. You say that "in your experience" flipping a light switch causes a light to turn on and off. Is that not a perception? If it's not, how do you know about it? Are you saying that if two events occur a second or two apart, A and B, you can't perceive which came before the other? How do you "experience" things, if not in the form of perceptions?

> "Switch" is an action, an "light" is the
>predicted consequence -- anticipation -- of that action. That
>anticipation is internally generated. If action "switch" does not
>result in "light" and nothing else, our anticipation is incorrect; we
>experience surprise and generally will want to find out why our
>prediction was incorrect; that leads to updating the model or
>possibly even an entirely new model.

But how do you know that the action you call "switch" is taking place? Don't you have to perceive it? The experience is of "switch" followed by "light", whether you are the one operating the switch or not. Behavior is a process of controlling perceptions: "switch on" is a perception produced by varying muscle forces and tactile sensations in certain places in the visual world. It's a perceptual consequence of motor actions.

>>It involves real-time perceptions from the outside world, not
>>imagined perceptions from the inside model.
>
>If the model were perfect (which it is not and cannot be), the
>imagined perceptions would be accurately in step with the real-time
>"perceptions" of the world (whether perceived or not). In that case,
>it would not matter whether we control on the basis of real-time or
>imagined perceptions. Since the model is not perfect and the world
>changes, the model must be recalibrated regularly. This is
>particularly acute when predictions turn out to be incorrect.

That's your problem, caused by the model you have adopted. Your model creates conditions that have to be met; if they aren't met, your model has difficulties. The PCT model is not without difficulties of its own, but they are not these difficulties.
>
>>Of course in your model you still have to explain how it is that
>>the person perceives sequence as a variable that can be correct or
>>incorrect.
>
>An incorrect prediction automatically leads to readjustment of the
>model. The standard adaptation mechanism takes care of that.

I have seen nothing to indicate that an Extended Kalman Filter could adjust a world model that predicts one sequence of occurrances rather than another. In fact, your model completely begs the question of how "sequence" itself could be perceived. Your claims just above are totally unsupported -- you're only describing what would NEED to be done, not showing that your model could do it.

>Small
>prediction errors are normal, and they hardly deteriorate the quality

>of control.

Unless they involve one or more successive time integrations, as most real behaviors do. Then small prediction errors can cause extremely large deviations from reality. The world, as you have probably heard, is basically chaotic,

> Detection of large prediction errors indicates that the
>model isn't even approximately correct; in such cases, learning --
>building a new model -- may be forced to restart from scratch.

That's what would be required. Unfortunately, the model you have presented contains no method for starting from scratch. You must begin with a model of at least the right general form before the Kalman Filter approach can work at all.

>This latter strategy is unpopular in humans. And entirely impractical: it
>would discard the entire existing knowledge base.

That's required only in a model of the kind you advocate, where the entire system is one immense interlocking set of computations. In a hierarchical system built from parallel modules of simple basic organization, no such radical surgery is required.

>Since it is often
>impossible to decide immediately which knowledge is still correct and
>which not, this is a critical situation which is difficult to handle
>by simple adaptive techniques.

Yes, that's true. I advocate a model in which this problem is minimized.

Best,

Bill P.

Date: Wed, 11 Dec 1996 00:07:25 +0000
From: Richard marken <marken@AERO.ORG>
Subject: Re: System concepts

[From Rick Marken (961210.2300)]

Hans Blom (961210e) --

> By now, it is clear to me that it is difficult for you to adopt a
> different perspective and a "suspension of disbelief", even briefly,
> which might let my perspective come through.

Let me give you a clue, Hans. I'm not interested in trying to adopt your "perspective" because I am a PCTer. I have been convinced (by testing, observation, modeling, etc) that behavior is the control of perception. I think this idea is the most profound, deep, extraordinary, elegant, interesting and satisfying insight about the nature of life that has occured in a millenium. It is a view that is completely at odds with all previous attempts to explain the behavior of living systems -- S-R theories, functionalist theories, reinforcement theories, cognitive theoies (including calculated output theories -- like your model-based controller-- that were popular in the 1980s), psychoanalytic theories,

etc. Because PCT exposes all current approaches to understanding behavior as being based on a misconception about the nature of behavior itself, very few behavioral scientists have had the courage to leave the conventional fold and become PCTers. Becoming a PCTer requires a serious change in system concepts (and, very often, careers); VERY few people have been willing to do either.

The Internet provided an exciting new way for the very few (and very dispersed) PCTers in the world to get together on a regular basis and discuss PCT. People who are not full tilt PCTers are certainly welcome to join in. And we do welcome scrutiny from advocates of other perspectives. But it's going to take a lot more than "argument" to convince a PCTer like me to readopt a perspective on behavior (such as you computed-output "model-based" approach) that I rejected long ago.

If you like model-based control then go with it. But you're just wasting your breath (and our bandwidth) if you think you're going to convince me that there is something to a perspective on behavior that, from my point of view, misses the point completely (notice how often all you model-based controllers talk about the perceptions that organisms control -- _never_ is my count).

I'm as likely to be convinced that there is something to be learned from model-based control as I am to be convinced that there is something to be learned from reinforcement theory, cognitive theory or whatever. I've been through all that. Years ago I've even believed in some of the things you believe in now. But I found that I had to reject these beliefs completely once I had studied and (eventually) understood PCT.

The one thing that you, as an advocate of model-based control, could contribute to CSGNet is data that is consistent with a model-based control model and inconsistent with a PCT model. Otherwise, I think you might as well give up trying to convert me to model-based control; easier to convert the Jews to Christianity. The only thing that could change me now is some clear evidence that organisms "compute" even one second worth of, say, the trajectory of a movement.

Me:

>I am willing to put my belief in PCT to that [experimental] test

Ye:

>How do you put a meta-theory to an experimental test?

It's really very easy; so easy that even I can do it. Take a look at some of the experiments in _Mind Readings_. Take a look at some of the experiments done by Tom Bourbon and described in various venues.

>I am certain that there is no such test, and thus nothing that can
>shake your belief in PCT. Or can you think of one?

I've thought of several. I'm always trying to think of more. Why don't you give it a try?

Best

Rick

Date: Wed, 11 Dec 1996 05:32:58 -0700
From: Bill Powers <powers_w@FRONTIER.NET>
Subject: Re: knowledge and rationalism

[From Bill Powers (961210.2010 MST)]

Tracy Harms 1996;12,10.13:30 --
>You are so close, Bill, but one glaring flaw remains in your epistemology,
>and that flaw is that it has far too much to do with justifiable certain
>knowledge! That you intend to purge this blot is most encouraging; I'll be
>happy to help if I can.
>
>In what way have you accepted the epistemology of justified certain
>knowledge? You have done so by granting the very concept of knowledge to
>it. This concession to that view of knowledge is heavy in your statements,
>for example: "But we are always separated from reality by that final
>fraction of a millimeter that we can't cross." Here you reiterate what I
>deny, the barrier metaphor which misapplies isolation by misconceiving
>knowledge.

>You should no more accept that knowledge is justifiable certainty than you
>would accept that behavior is the consequence of stimuli.

I don't accept that knowledge is justifiably certain. I have intended to be arguing against the notion that this is possible, and therefore that if "knowledge" means anything, it does not mean that. Quite probably my words have given a different impression.

> Never abandon key terms to proponents of inferior theories.

That is a hell of a good idea. It's hard to put into practice, however, when someone else is not willing to be considered the proponent of the inferior theory, and follows the same principle. See the arguments on CSGnet about what the term control "really means."

>This concession you've carelessly made to justificationists is coupled with
>an even larger problem, a concession to a justificationist vision as to the
>nature of rationality. This is revealed in your statements:
>
>>I guess what I'm saying is that I don't think there there is any way to
>>answer the basic question of epistemology except to declare faith in some
>>answer, and then forget that you were responsible for the declaration.
>>[...]
>>The "ante" of which you speak is a committment to a certain belief, by
>>which, as I said above, I mean a belief which you have decided to accept as
>>certain, implying that you have also decided to forget that it was your own
>>act that made any doubts disappear.

I guess I made it seem as though I were _advocating_ this strategy. I meant to be pointing out that the only way to claim true knowledge of the relation of experience to reality is to indulge in this form of self-deception.

It would probably be helpful to more people than me if you were to sum up Bill Bartley's solution of the problem of rational integrity (also, perhaps, defining what that problem is for those of us far from enriched libraries).

>In keeping with evolutionary rationalism I do *not* speak of a committment

>to a certain belief. I do not decide to accept anything as certain, so
>there is nothing for me to forget; no skeletons in my closet, no dirty
>little secrets of irrational leaps of faith, no actions by which I make any
>doubts disappear. My doubts are not to be banished except as a spontaneous
>consequence of considering well-argued theories.

I guess I don't believe in such spontaneous consequences. I can't think of any theory, however well-argued, in which I believe to the extent of banishing all doubt. That includes particularly my own -- I know too much about PCT to believe in it without having concerns about its shortcomings. I think that full belief comes only when you make the conscious decision to stop looking for problems. I don't think I could do that. The magic words concerning knowledge, in my opinion, are "It seems to me ..."

> Things get
>good by discarding the bad, not by passing some sort of torch from a Pure,
>Absolute Goodness into the more ambiguous world.

That sounds more like it to me.

Best,

Bill P.

Date: Wed, 11 Dec 1996 05:33:14 -0700
From: Bill Powers <powers_w@FRONTIER.NET>
Subject: Re: knowledge and rationalism

[From Bikll Powers (961210.2100 MST)]

Bruce Gregory (961210.1745)

>The notion that "we are separated from reality by that final
>fraction of a millimeter that we can't cross" is not given to
>us by way of observation, it is a property of our models.

Yes, exactly. At present, the best models I know of say that there is no way the brain can directly contact the reality that causes primary sensory signals.

>Nothing prevents us from trying to build ever more powerful
>models that allow us to make more and more sense of the world.
>In this sense, there is no ultimate separation between us and
>reality.

But such hypothetical models would have to reveal to us how the brain could know about an external world without having to depend on primary sensory signals. Right now I don't know what kind of qualitative improvement could accomplish that.

My epistemology is purely practical engineering and physics -- model-dependent, of course. When I build a control system, I can see that it knows nothing of the world outside it except what is represented by the input signals coming out of its sensors. Since it can't sense its own sensors, it can't know _even approximately_ what the actual inputs were. I can't get past that problem. There are ways, in more complex systems, to represent relationships between actions and consequences, but how we explain these relationships is always dependent on postulating models and testing

them. And no matter what model you propose, I can always come up with a different one that will do EXACTLY THE SAME THING but by a different means. That's what _I_ mean by the "barrier." It seems inherent in the model.

Best,

Bill P.

Date: Wed, 11 Dec 1996 05:33:27 -0700
From: Bill Powers <powers_w@FRONTIER.NET>
Subject: Re: model-based tradeoff

Hans Blom (961210e) -

Rick Marken said:

>the longer
>the model can control after loss of perceptual input, the less able
>it is to compensate for real time disturbances when perceptual input
>is intact.

To which you replied:

>I guess it is these uninformed misunderstandings that I'm tired of.

Actually. that is also my "uninformed misunderstanding." I had understood you to say that in setting up a Kalman filter system one of the factors that the analyst has to adjust is the rate of convergence of the model, which is done by cut and try rather than any strictly analytical method. If the model converges very rapidly, it will be able to keep up with rapid changes in the properties of the external world, but in doing so it will lose the ability to predict far ahead and thus the ability to carry on during protracted losses of input. Convergence may also be adversely affected. On the other hand, if you adjust the model to converge slowly but surely, it will be able to continue operating in nearly the same way for a considerable time after loss of input, but will be unable to keep up with rapid changes in the external world.

I don't think this problem is peculiar to the Kalman filter approach; it's inherent in the nature of prediction. The accuracy of a prediction depends on the length of the sample you work with. If you want to predict successfully for a long time ahead, you have to use a long sample, and you have to count on the characteristics of the world remaining constant during that sampling time as well as during the prediction time. This means that you have to average over variations that take place within the sampling time, and lose the ability to correct for them in detail. I think this tradeoff is a natural aspect of any prediction scheme. Either you predict the details very accurately for a short time into the future, or you predict the long-term trends accurately at the expense of short-term accuracy. I don't believe you can do both at the same time. You're simply making different assumptions about the time-scale on which the world is regular. What says CSGnet?

Of course this applies to modeling disturbances. In "challeng5" you adjusted the
Kalman filter computations to correct essentially all the prediction error for the disturbance in a single iteration of the program, in effect

extrapolating one iteration ahead. As a result, when input was cut off the errors of prediction started growing very rapidly on each successive iteration, and there was essentially no long-term prediction capability.

While I can't untangle the complexities of your computations in detail, I'm quite sure that the result of this rapid response is to convert your "model-based" system into a simple closed-loop feedback control system witb a slight amount of first-derivative feedback (the one-iteration extrapolation). This comes about because the disturbance part of the model is simply transferred without change (except for sign) into the inverse calculation that is placed in series with the inverse of the world model, to generate the output u. The effect of the disturbance reaches the output part of the model just as though there were a direct negative feedback connection with respect to transient changes in the real perceptual input (the world-model's output doesn't enter into that loop). However, since the other properties of the world-model itself are not used, this is of no help in combating long-term loss of feedback. When input is lost, the real disturbances go right on affecting the "plant" outside the control system, but they disappear within a few iterations from the model, so they are no longer compensated.

This removes one of the main distinctions between the world-model based control system and the PCT type of control system, the ability to continue producing approximately appropriate actions after loss of input. If you adjust the Kalman filter calculations so the adaptive system competes with the real-time control system in its ability to resist rapid disturbances, to that degree you have to give up the ability
to operate for any significant time without input.

Suppose that the disturbance had been a slow sine-wave, a regular waveform with a period of several hundred iterations of the program.. To take advantage of this regularity, you could now use a sine-wave generator in the world-model, adjusted appropriately in frequency, phase, and amplitude, to model the disturbance. To achieve accuracy it would be necessary to sample over at least one full cycle, so as to establish the required frequency and phase.

As long as the perceptual input were intact, the behavior of the model-based control system and that of the PCT system would be essentially the same, although in principle the model-based system could achieve an exact cancellation of the error while the PCT system would always have to contain at least some small amount of error. However, if the perceptual input were lost, a _simple one-level_ PCT model would immediately fail, while the disturbance being simulated in the model-based system would continue for some time, perhaps hundreds or thousands of iterations, to match and nearly cancel the real sine-wave disturbance. The model-based system is the clear winner in this case.

But consider what will happen with the perceptual input intact if the real sine-wave disturbance suddenly ceases to change and becomes constant. The PCT model, which does not predict, simply adjusts its output as usual to cancel the effect of the now-constant disturbance. It continues to contain the same small error. The world-model based system, however, continues to produce the same sine-wave modeled disturbance, so it now is generating output to cancel a non-existent external disturbance. The result will be an immedate departure of the real controlled variable from the specified reference level, just as if a new disturbance equal and opposite to the old one had been introduced. It will take _at least_ one full cycle at the old frequency for the world-model to establish the new frequency (zero). So now

the model-based system is the clear loser. Its advantage becomes an equal disadvantage when the character of the disturbance changes.

Do you consider this analysis to be the result of "uninformed misunderstandings"?

Best,

Bill P.

End of CSGNET Digest - 10 Dec 1996 to 11 Dec 1996
****

Date: Thu, 12 Dec 1996 08:00:17 -0600
Subject: CSGNET Digest - 11 Dec 1996 to 12 Dec 1996

There are 18 messages totalling 1084 lines in this issue.

Topics of the day:

1. knowledge and rationalism
2. System concepts (2)
3. Memory switch (2)
4. Model-based control, science, agreements (2)
5. New papers on complex systems, economy and control (4)
6. PCT Acceptance (2)
7. Solving "loss of input" problem (3)
8. Periodicity; event control
9. Modelling, Apes & Self

Date: Wed, 11 Dec 1996 09:49:52 EST
From: Bruce Gregory <gregory@CF4.HARVARD.EDU>
Subject: Re: knowledge and rationalism

[From Bruce Gregory (961211.0950 EST)]

Bill Powers (961210.2100 MST)

> My epistemology is purely practical engineering and physics --
> model-dependent, of course. When I build a control system, I can see that it
> knows nothing of the world outside it except what is represented by the
> input signals coming out of its sensors. Since it can't sense its own
> sensors, it can't know _even approximately_ what the actual inputs were. I
> can't get past that problem. There are ways, in more complex systems, to
> represent relationships between actions and consequences, but how we explain
> these relationships is always dependent on postulating models and testing
> them. And no matter what model you propose, I can always come up with a
> different one that will do EXACTLY THE SAME THING but by a different means.
> That's what _I_ mean by the "barrier." It seems inherent in the model.

I completely agree.

Bruce Gregory

Date: Wed, 11 Dec 1996 10:13:35 EST
From: Bruce Gregory <gregory@CF4.HARVARD.EDU>
Subject: Re: System concepts

[From Bruce Gregory (961211.1015 EST)]

Rick Marken (961210.2300)]
to
Hans Blom (961210e) --
>

> If you like model-based control then go with it. But you're just wasting
> your breath (and our bandwidth) if you think you're going to convince me
> that there is something to a perspective on behavior that, from my point
> of view, misses the point completely (notice how often all you
> model-based controllers talk about the perceptions that organisms
> control -- _never_ is my count).

> The one thing that you, as an advocate of model-based control, could
> contribute to CSGNet is data that is consistent with a model-based
> control model and inconsistent with a PCT model. Otherwise, I think you
> might as well give up trying to convert me to model-based control;
> easier to convert the Jews to Christianity. The only thing that could
> change me now is some clear evidence that organisms "compute" even one
> second worth of, say, the trajectory of a movement.

It's always hard for me to infer what Rick _really_ means,
because he is so reticent about expressing his true feelings
;-) I _think_ Hans that Rick is saying that you are offering him
a solution to a problem he does not yet have. This is always a
very hard sell. To date you have not been too successful in
convincing many of us that we should devote any effort to
understanding your solution to problems we cannot even
identify. Pity, doubtless our loss. But since life, as far as
we know, is finite, we have to set priorities.

Bruce Gregory

Date: Wed, 11 Dec 1996 17:26:01 +0100
From: Stefan Balke <sbalke@HRZ.UNI-BIELEFELD.DE>
Subject: Re: Memory switch

>From Stefan Balke (961211.1715 CET)

>>From Jeff Vancouver (961210.17:40 EST)

>That is basically it. The only slight alteration is that the system can
>use input from the environment to test the result of reorganization, not
>simply memory as the imagination mode implies. Nonetheless, the output
>switch is not set to the environment until a reorganized output passes
>the test.
>
>No others have commented on it. I think they are having the same problem
>as you. They are not familiar with the game.

Jeff,

just one remark to the idea of a memory switch. Isn't it a general problem to propose something like a switch which implies a discrete variable (on - off state) within a model that basically assumes continuously changing variables?

Best, Stefan

Date: Wed, 11 Dec 1996 17:49:54 +0100
From: Stefan Balke <sbalke@HRZ.UNI-BIELEFELD.DE>
Subject: Re: Model-based control, science, agreements

>From Stefan Balke (961211.1730 CET)

Rick Marken (961210.0930) to Bruce Gregory (961210.0945 EST) --

>>I'm more optimistic than you are about this. I think one key lies in Bill's
>>persuasive argument that we all do well to avoid generalizations such as one
>>individual controlling another and focus on exactly what variable is being
>>controlled.
>
>I agree that this is a very good approach. My pessimism comes from
>experience. B:CP was published 23 years ago; I don't think there is a
>clearer description of a model of behavior (with few generalizations).
>Nevertheless, after all this time there are still only a handful of
>behavioral and life scientists who are practicing perceptual control
>theorists. Clarity is great but it doesn't _necessarily_ lead to agreement --
>especially when you are presenting ideas that people clearly don't _want_
>(for whatever reason) to agree on. I wish it were different, but it's not.

The problem is only with people who defend their own scientific view. (And this phenomenon is not too suprising from the view of PCT). I'm now sometimes invited to speak about Ed's Program and I always speak about Control Theory at this meetings. I have the experience that _everybody_ outside my university accepts the PCT ideas. In fact people say that they already think so. I could imagine that this is also a reason for the great success of Ed's program in the school practice. So, maybe there is still some reason for optimism :-)

Best, Stefan

Date: Wed, 11 Dec 1996 11:54:11 EST
From: Bruce Gregory <gregory@CF4.HARVARD.EDU>
Subject: Re: Model-based control, science, agreements

>From [Bruce Gregory (961211.1200 EST)]

Stefan Balke (961211.1730 CET)

> The problem is only with people who defend their own scientific view. (And
> this phenomenon is not too suprising from the view of PCT). I'm now
> sometimes invited to speak about Ed's Program and I always speak about
> Control Theory at this meetings. I have the experience that _everybody_

> outside my university accepts the PCT ideas. In fact people say that they
> already think so. I could imagine that this is also a reason for the great
> success of Ed's program in the school practice. So, maybe there is still
> some reason for optimism :-)

I have a very similar experience. In a sense Rick and Bill have been talking to the worst possible audience as far as openness or interest is concerned. You don't make converts by preaching to another congregation ;-)

Bruce Gregory

Date: Wed, 11 Dec 1996 12:05:28 -0500
From: Jeff Vancouver <jeffv@PSYCH.NYU.EDU>
Subject: Re: Memory switch

[from Jeff Vancouver 961211.1200 EST]

> >From Stefan Balke (961211.1715 CET)
>
> just one remark to the idea of a memory switch. Isn't it a general problem
> to propose something like a switch which implies a discrete variable (on -
> off state) within a model that basically assumes continuously changing
> variables?
>

Yes and no. No, because I believe that Power's description of the switch is discrete. Given that the choice to act is often discrete (we only have so many degrees of freedom here), this seems reasonable. THe degree to which you act could well be continuous. IN the particular example, the degree to act is also discrete, but that is a function of the task and not the model. (I talk about this in the notes button. It is an unfortunately side effect of the task I choose).

Yes, because on might better conceive the switch in a neural net way. I see this as more relevant for the input switch where, as in the Vancouver experiment, input from the environment and from imagination is probably combined to determine perceptions for controlling the mouse/cursor. Of course the interpretation of that data has not been fully fleshed out. It is on my list, but my next comments are likely to be long and I have very pressing matters to attend to. But I digress.

Later

Jeff

Date: Wed, 11 Dec 1996 18:16:34 +0100
From: "J.A. Blom" <J.A.Blom@ELE.TUE.NL>
Subject: Re: System concepts

[Hans Blom, 961211]

(Bruce Gregory (961211.1015 EST))

>It's always hard for me to infer what Rick _really_ means, because
>he is so reticent about expressing his true feelings ;-) I _think_
>Hans that Rick is saying that you are offering him a solution to a
>problem he does not yet have. This is always a very hard sell.

Yes, yes and yes.

>To date you have not been too successful in convincing many of us
>that we should devote any effort to understanding your solution to
>problems we cannot even identify. Pity, doubtless our loss.

No, _my_ loss: too bad I cannot discuss _my_ problems here without
getting helpful responses, except frequently repeated statements that
I'm hallucinating and seeing problems that don't really exist :-).

>But since life, as far as we know, is finite, we have to set
>priorities.

Yes, alas. But that still leaves me with my problems and how to best
solve them. A major one is -- in all my engineered control systems --
of how to go on controlling (if I may use that word ;-) when one or
more of the feedback sensors fails, or is temporarily out of order or
blinded. Humans may not (seem to :-) have that problem but control
systems that are to be useful in complex, critical tasks certainly
do. Maybe that comparison fails entirely...

But thanks for your sympathy.

Greetings,

Hans

Date: Wed, 11 Dec 1996 18:04:43 +0100
From: Francis Heylighen <fheyligh@VNET3.VUB.AC.BE>
Subject: New papers on complex systems, economy and control

Approved: Francis.admin
To: PCP-news@listserv.vub.ac.be
From: fheyligh@vnet3.vub.ac.be (Francis Heylighen)
Subject: New papers on complex systems, economy and control

Two new papers are available on the Principia Cybernetica server
(http://pespmc1.vub.ac.be/):

Francis HEYLIGHEN: The Economy as a Distributed, Learning Control System

http://pespmc1.vub.ac.be/Papers/MarketCo.html
(to be published in the journal "Communication & Cognition-AI")

ABSTRACT. On the basis of the perceptual control theory of Powers, the
market mechanism is analysed as a negative feedback loop which controls the
deviation between demand (goal) and supply (perception) by adjusting the
amount of effort invested in the production process (action), through the
setting of the price. The interconnection of distributed control loops for
the different products and services facilitates the allocation of
production factors over the different products. The resulting global
control system becomes more efficient by learning how to be more sensitive

to deviations from the goal, and less dependent on the availability of resources. In that way, it resembles the nervous system of a supra-individual organism, characterized by socially distributed cognition.

Francis HEYLIGHEN: Classic publications on complex, evolving systems: a citation-based survey

http://pespmc1.vub.ac.be/Papers/PublicationsComplexity.html
(submitted to the journal "Complexity")

ABSTRACT. A list of the most relevant publications on complex, evolving systems is produced by counting the number of times each publication is cited in a collection of texts on the domain. The importance of these books and papers is summarized and put into its historical context by noting the main contribution to the field of their authors, categorized by the research tradition they originated from. These include biology, physics, chemistry, mathematics, cybernetics, systems theory, economy and complex adaptive systems.

Dr. Francis Heylighen, Systems Researcher fheyligh@vnet3.vub.ac.be
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Date: Wed, 11 Dec 1996 10:56:41 -0800
From: Richard Marken <marken@AEROSPACE.AERO.ORG>
Subject: PCT Acceptance

[From Rick Marken (961211.1050)]

Stefan Balke (961211.1730 CET) --

>I have the experience that _everybody_ outside my university accepts
>the PCT ideas.

Bruce Gregory (961211.1200 EST) --

>I have a very similar experience.

I've had the same experience as well. I find that the people who accept PCT most readily are those who are outside of academia, using behavioral theories to solve "real world" problems. I am thinking, in particular, of clinicians and educators.

When I taught at Augsburg College I had a running argument with my Skinnerian colleague (and good friend) about PCT. He was pretty "anti" PCT until he tried a new career as a student counselor. Then he started noticing all these people coming in for help whose main complaint was that their life felt "out of control" (a good description of the results of internal conflict). This led him to realize that people want to be "in control" of things (their perceptions, of course;-)) and that this is what PCT is about. Unfortunately, instead of getting into real PCT he got into Glasser's version. But at least he got the main point -- that people are controllers of, not controlled by, the environment.

I'm glad that applied people tend to accept PCT. My only problem with this is that applied people are often so busy solving the problems at hand that they have no time to carefully test and expand the theory. Applied people are not able to spend much time testing for controlled variables in a reasonably formal way, for example. Most of the applied work I have seen is also based on giving a PCT interpretation to the external appearance of some behavior. There is no tome for a systematic effort to _determine_ (by test) what is being controlled or why it is being controlled.

I think the tools used by the applied people -- the systematic procedures that are used in a PCT-based approach to clinical treatment or education -- have to be developed by the academic (research) types. The applied people just don't have the time, the skills or the inclination to do this. That's why it's too bad (from my point of view) that the academic types have not gotten into PCT.

Best

Rick

Date: Wed, 11 Dec 1996 13:28:21 -0800
From: Richard Marken <marken@AEROSPACE.AERO.ORG>
Subject: Solving "loss of input" problem

[From Rick Marken (961211.1330)]

Hans Blom (961211) --

>But that still leaves me with my problems and how to best solve them. A
>major one is -- in all my engineered control systems --of how to go on
>controlling (if I may use that word ;-) when one or more of the feedback
>sensors fails, or is temporarily out of order or blinded.

If you understood that control systems control their perceptions then you might have looked for a different solution to your problem than the "model based" solution that you seem to think of as the only one.

The "model-based" solution seems to be based on the assumption that the control system's problem (when the sensor fails, is temporarily out of order or is blinded) is generation of the "appropriate" output. The thinking might have gone something like this: "When there is no feedback input to the control system, then there is no basis for computing output. So we have to give the control system a feedback- free basis for generating output when there is no sensory feedback. Hey, I got it! How about an internal model that will continue to generate output in response to the the input signal [the PCT reference signal] even when there is no feedback signal."

The problem with this reasoning is that a control system _never_ computes output, even when it _has_ sensory feedback. When the feedback input to a control system is lost, control is lost -- period.

If you can count on the world remaining the same after the system is blinded, then you can have the system produce outputs "open loop" in order to keep a variable "under control", But these outputs only _seem_ to keep the variable undercontrol; the system is not really controlling with these computed outputs, a fact that would become immediately apparent as soon as a small disturbance was added to the effects of the output.

A PCT savvy control engineer would probably take a completely different approach to solving the problem of dealing with possible loss of sensory input. Since control systems control inputs, then one way to deal with the possible loss of an input is the good, old fashioned engineering way of dealing with possible component failure -- _redundant components_ (in this case, redundant inputs). This redundancy could be implemented in many ways, depending on system design. Suppose, for example, that you are building a temperature control system and you are afraid that the temperature sensor might go out occasionally. One way to handle this is to have several control systems controlling (in parallel) inputs from different temperature sensors, where each sensor is measuring the temperature in the same room. Another apporach is to have several control systems controlling inputs from several different _kinds_ of sensors but where the output of each sensor (the input to the control system) is proportional to temperature. This seems similar to the organismic solution, where we are able to maintain control of the perception of a variable in one modality when we lose the perception of that variable from another modality.

Anyway, there are other (and possibly better) solutions to your problem than "model based" output generation (which is really just one notch above praying -- that there are no disturbances during the period of loss of input).

Best

Rick

Date: Wed, 11 Dec 1996 14:43:01 -0700
From: Bill Powers <powers_w@FRONTIER.NET>
Subject: Re: New papers on complex systems, economy and control

[From Bill Powers (961211.1320 MST)]

fheyligh@vnet3.vub.ac.be (Francis Heylighen)

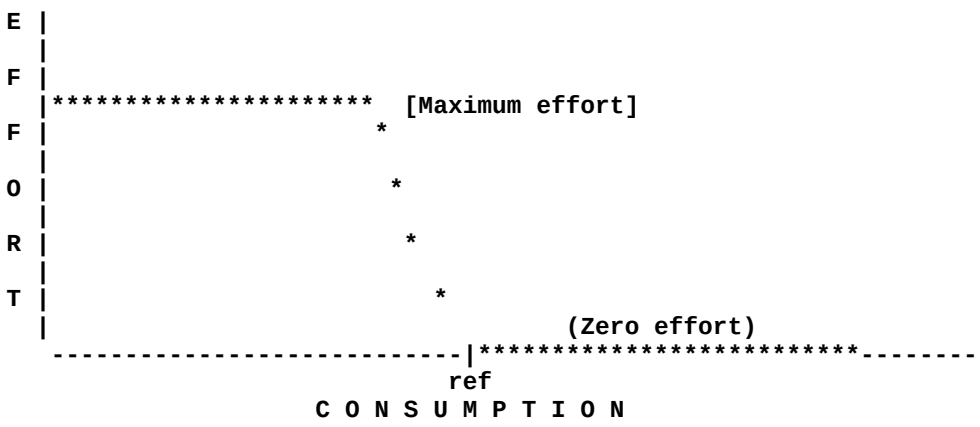
Hello, Francis.

I received your notice about your two new papers, and have read the one on economics. This is precisely the kind of treatment I have had in mind attempting, although yours begins at a level that I hadn't thought of and which is a far better introduction than I could have contrived. I am really delighted with your treatment. It's especially satisfying to me that you have grasped and communicated so well the "assymetry of control" idea, and indeed expanded it into a practical tool for analysis. Your identification of demand with reference signals is exactly what I had in mind: "Full marks," as they say across the Channel from you.

>From the looks of your publications list you're a man with a finger in many pies. I hope, however, that you intend to pursue the economics thread further, because I believe there are many more ways in which to apply the basic principles of control to this field. Just to tempt you to go in this direction a little further, here is a thought on the "demand curve" that you might find suggestive (and feel free to carry it on from here -- there are lots of ideas I will be glad to shift to the shoulders of others).

Suppose we plot the demand curve for an individual and for a given good or service, but using the basic idea of a control model. The relevant idea is

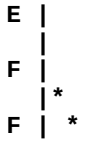
that a person has a reference level for the amount or rate of consumption of a given good, which that person will exert effort to obtain (good old K being the measure of the amount of effort resulting from a given level of error). Let's assume that K is rather high, so that only a small deficit in the good relative to the reference level will create a rather large error. This leads to the following curve, where now "effort" is interpreted as proportional to the error.

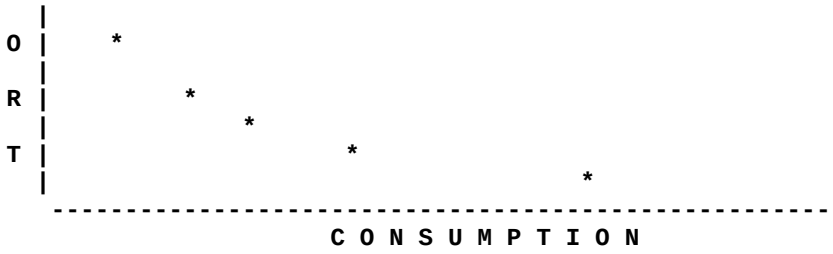


The location along the Consumption axis labeled "ref" represents the amount of the good that the person wants. The curve represents the effort put out by the person for various amounts obtained. At the reference level for the obtained amount, the effort just becomes zero: at or above that level of consumption the person will exert no effort to get more. This is a one-way control system (you could propose, for some goods or services, that above the reference amount obtained the person would begin exerting efforts to get rid of the excess -- negative effort).

As the amount obtained falls below the reference level (moving to the left on the curve), the effort increases quite sharply. As drawn, it takes only a 20% shortfall in the amount of good obtained to generate maximum effort. The slope of the rising-to-the-left part of the curve corresponds to the magnitude of K.

This doesn't look much like the demand curves you see in economics texts, which are more like hyperbolas that descend smoothly over the whole range from a high value at the left to zero far to the right. But I contend that this is only because those demand curves are for populations rather than individuals. A person might work quite hard to purchase one car, or even two cars, but when it comes to obtaining a third car the effort will drop, and few people I know would go to much effort to buy the fourth car. However, if you do a survey over a whole population, you will find that some people won't work hard to get more than one car, while a few others might still be laboring mightly to get their eighth car. When you sum over the whole population, what you get is the _aggregate_ demand in terms of effort, and this composite curve is very likely to look like this:





This curve is the sum of many curves like the first one, for a population in which there is a range of reference levels for the same good, and a range of K values, and even a range of maximum levels of effort. What you get is one of those statistical lies, in which the apparent characteristic of the population is seriously different from the characteristic of any individual in that population. For any one individual, there is a definite amount of a good that is considered "enough," and even if the supply is free, the person will not exert any effort to get more than that amount (your example of air is a good one). However, it seems that for the population, there is no upper limit to the demand; the lower the cost in labor or money, the greater the consumption will be. This can easily be mistaken for a basic characteristic of individuals, leading to the notion that the greater the reward, the harder the person will work -- since there is no apparent limit to the desired amount of any good.

The "supply" curve can be derived in a similar way, by interchanging the axes. What the supplier demands is the efforts of individual workers. The supplier's "effort" is the amount of goods that will be produced to obtain these efforts from the individuals (where of course the "goods" can be measured in terms of an equivalent amount of money, and the supplier, apart from his company, is also a consumer). The price of the goods, perhaps divided by productivity, is the slope of this curve. We can imagine a large number of suppliers, each with a reference level for obtained labor or its money equivalent, and each with a rather steeply-rising error curve as in the first diagram (but rotated 90 degrees on that plot). The_composite_ curve for the supplier will also be a sum over the population of suppliers, a smoother curve than we find for any individual supplier.

Where these two curves intersect we have the actual state of the system, in which the errors in supplier and consumer control systems, weighted by the respective K values, are minimized in the aggregate.

I think there are all kinds of titillating possibilities that can be developed out of this beginning. For example, what happens when there is only a single supplier for a good, a monopoly (with a steep error curve)? More: each part of this loop is now a control system, so the output of each one is the controlled input for the other. What are the potentials for conflict? Are there forbidden states of the system, or boundaries beyond which it breaks down? What do the demand curves say about the distribution of purchasing power? What happens when we consider multitudes of goods/services for which individuals have reference levels and for which they will apportion their total available effort according to their individual preferences?

As you noted, I'm in favor of treating social phenomena as being emergent from interactions among individual autonomous systems. This is an example of how we can get from individual characteristics to characteristics of social systems. It's sort of like starting with the kinetic theory of molecular motion and ending up with thermodynamics. I like this approach because it's

solidly grounded in first principles, while the system analysis develops smoothly out of considering basic individual characteristics in the aggregate -- not as averages, but as a composite phenomenon.

Does this sound interesting?

Best regards,

Bill Powers

Date: Wed, 11 Dec 1996 16:19:18 -0600
From: Bruce Abbott <abbott@CVAX.IPFW.INDIANA.EDU>
Subject: Re: Solving "loss of input" problem

[From Bruce Abbott (961211.1720 EST)]

>Rick Marken (961211.1330) to Hans Blom:

>The problem with this reasoning is that a control system _never_ computes >output, even when it _has_ sensory feedback. When the feedback input to a >control system is lost, control is lost -- period.

>If you can count on the world remaining the same after the system is blinded, >then you can have the system produce outputs "open loop" in order to keep a >variable "under control", But these outputs only _seem_ to keep the variable >undercontrol; the system is not really controlling with these computed >outputs, a fact that would become immediately apparent as soon as a small >disturbance was added to the effects of the output.

>A PCT savvy control engineer would probably take a completely different >approach to solving the problem of dealing with possible loss of sensory >input. Since control systems control inputs, then one way to deal with the >possible loss of an input is the good, old fashioned engineering way of >dealing with possible component failure -- _redundant components_ (in this >case, redundant inputs).

Redundant inputs is an excellent idea if the problem is sensor reliability, but it will do you no good at all if the problem is a genuine loss of signal (e.g., driving into a fog on the Santa Anna at 70 mph, the fact that your right eye has a back-up left eye is of no help).

It seems to me that a PCT savvy control engineer facing the sort of problem Hans is concerned about (with the controller working in an environment in which the signal tends to vary in a way that can usually be predicted fairly well in the short term) would design a system that would attempt to model these systematic variations (as Han's system does) and use this model, not to generate computed _outputs_, but to generate a computed substitute _input_ to what otherwise would be an ordinary PCT-style controller. The system would also have to generate (or be given) a model of the environmental feedback function, so that the expected effect of the controller's own actions on the model CV could be computed. To the extent that the behavior over the short term of the actual CV can be modeled (and in the absence of unexpected serious disturbances to that CV during that time), such a system would be capable of maintaining at least some degree of control during brief loss of input. (I am assuming that some criterion has been put in place that tells the controller when its real-time perceptual input has been lost and regained, so that it knows when to switch to this

"imagination" mode.

Hans, is this conception different from your model-based controller?

Regards,

Bruce

Date: Wed, 11 Dec 1996 18:03:13 -0500
From: Martin Taylor <mmt@HERMES.DCIEM.DND.CA>
Subject: Re: Periodicity; event control

[Martin Taylor 961211 16:50]
Bill Powers (961124.0645 MST)

Some things take a long time to percolate...
>
> Tracy Harms (19961123.22 MST) --
>
> >What does PCT say
> >regarding periodic behavior? Yes, I can see how it can always be mapped as
> >instrumental to a (relatively?) stable reference level, but still I suspect
> >there is something significant about oscillation, something which makes it
> >morphologically distinct from other feedback structures.

Actually, _any_ oscillation indicates that there is some kind of feedback structure involved. The problem is the reverse of what you see. It is how to prevent structures with feedback from oscillating or even going chaotic. The more links there are in a network, the harder it is to ensure that oscillation does not dominate the behaviour of the network. The PCT hierarchy avoids oscillation primarily be ensuring that successive levels within the hierarchy operate sufficiently slower at each level upward, and by ensuring through reorganization that control units on the same level minimize their interaction.

An oscillator is morphologically identical to a control loop. The distinction is in the parameter values, especially gain and loop delay.

If a putative control loop goes into oscillation, what is happening is that its output at any one moment is primarily responsive not to either of its inputs (disturbance or reference) but to its output value at some earlier time(s). What this means is that there has to be some memory of signal values, implicit or explicit, in at least two places around the loop. This "memory" might be the delay in the effect of an output on the sensory input, or it might be the kind of transfer or energy from kinetic to potential and back that happens in a pendulum.

Around any feedback loop, there is some value(s) of delay for which the effect of output tends more to reinforce later output than at other values of delay. For example, if I shout across at a strongly echoing cliff, and one second later the sound comes back to me and hits the cliff I'm leaning against, then I can build a big sound pulse by shouting at one-second intervals, but not at intervals of 1.1 sec (I ignore acoustic phase effects here).

"Bad things" (such as oscillation or clamping) happen when the gain around a loop is greater than +1 at some time delay. In practical systems, these

"bad things" are self-limiting, if only because there isn't enough power in the system for it to blow itself up. In biological systems, usually there are non-linear components in the loop that limit the size of the excursions. And, if the oscillation is reasonably stable (i.e. the dominant loop delay doesn't change much), then the loop that is oscillating can be put to good use as a clock whose output is insensitive to input, and can therefore be used as one input component for perceptions that depend on time.

Delays in such oscillating systems can be of any magnitude from milliseconds to years. It depends where the influences of the output values are strongest, and when those influences come back through the feedback path. We probably have a large number of oscillators in our biochemical and neurological systems, with all sorts of time periods.

Most oscillating systems are readily synchronized. A small impulse fed at the appropriate moment to add to a building value will advance the time when the value reaches its peak, cause in a slight phase advance, which is equivalent to a momentary increase in frequency (the reverse does not usually work). Our internal circadian clock, in most people, seems to run at around a 25 or 26 hour cycle, but the daily sun cycle is enough to synchronize it so that it seems to run at a 24-hour rate.

Detection of periodicity might be related to the existence of internal oscillators, or clocks. The clock output might form an input to a perceptual function together with the possibly periodic signal, or the possibly periodic signal might serve as a synchronizing signal for the clock. The latter possibility is quite attractive, in that there could be a perceptual function having the form of a filter that rejects signals at the free-running frequency of the clock. A non-periodic signal would not shift the clock frequency, but a periodic signal at a frequency slightly higher than the clock would, so long as the discrepancy was not too great. The perceptual input function filter would then have an output proportional to the frequency difference between the free-running oscillator and the periodic input.

> About the only literature I can think of is by people in the Motor Control
> field, and I don't much like their solutions to the problem. They propose,
> correctly I think, that there have to be oscillators and pattern generators
> in what I would call the output functions. But they think this can be done
> open-loop, which I don't believe. I think we also perceive in such terms as
> phase, frequency, and amplitude, and can control with respect to reference
> phases, frequencies, and amplitudes.
>
> If I were to try to get this kind of behavior explicitly into the HPCT
> model, I think I'd start by looking at the "event" level with the idea of
> expanding it to include oscillatory or repetitive pattern control. As it
> stands now, this level is vaguely associated with short, familiar space-time
> packages of transitions, configurations, sensations, and intensities which
> are perceived as single units, like the bounce of a ball.

I've always had a problem understanding event-level control. An event is a one-shot deal. It happens and is gone, and nothing one can do will change that. One's output may change what might happen during a later event, but not during the one that different from its reference perception. It seems to me that event control is necessarily ballistic, not continuous like control of the separation between a cursor and a target. The bullet is fired and misses the target by X. The rifle is readjusted so that the _next_ bullet misses by less. But nothing can be done about _this_ bullet.

It has missed, and that's that.

Event control, it seems to me, is always sequence control or soemthing like that. There is a presumption that another related event is going to happen and that the output consequent on the error of the event perception will affect the next, related event. Whether the related event is another of the same kind, like shooting another bullet from the same rifle, or is something of a different kind, seems immaterial. It looks to me like sequence control, either way.

Which brings us back to periodicity, or perhaps I should say "timed" control. Delays in feedback loops need not cause oscillation, but they can be important in timed event/sequence control. Something in the world is consistent enough that we have reorganized to be able to control a sequence of related events. Our system delays must match the consistent delays in the world or we will oscillate, not control the event sequences. The accelerations of a baseball bat or a tennis racquet must begin long before the ball arrives, but not too long before--that's what a change-up pitch does to the batter. The "sequence" here is reasonably continuous, but there's a discrete event that synchronizes it--the moment of release of the pitch or the opponent's strike of the tennis ball. And if our event control (perception of an effective hit) is to work, it must include perceptions of time intervals--that is to say, delays.

>
> I've always promised myself that I'd try a model of this kind of control,
> but somehow have never got around to it. It's not hard to put together
> phase, frequency, and amplitude detectors, but the details of how error
> signals get turned into corrections of phase, frequency, and amplitude of
> the output pattern generators have to be worked out, and I've just never
> done it. Maybe I keep hoping that someone else will do it.
>
I don't know whether this is a baby step in that direction, or if it goes off somewhere else. All I want to do is to point out two things: (1) that oscillation in complex networks with gain is extraordinarily hard to avoid, and (2) that the control of event perception is (seems to me to be) an aspect of sequence control that requires the kind of delay that is involved in oscillation and internal clocks, which almost certainly exist in us.

Martin

Date: Wed, 11 Dec 1996 18:35:38 -0700
From: Bill Powers <powers_w@FRONTIER.NET>
Subject: Re: Solving "loss of input" problem

[From Bill Powers (961211.1640 MST)]

Bruce Abbott (961211.1720 EST)

>Redundant inputs is an excellent idea if the problem is sensor reliability,
>but it will do you no good at all if the problem is a genuine loss of signal
>(e.g., driving into a fog on the Santa Anna at 70 mph, the fact that your
>right eye has a back-up left eye is of no help).

This is true. You can switch to holding the steering wheel in the same position it was in before you hit the fog; this, at least, will keep the car stiffer against bumps and tilts in the road. But what is basically going to

happen is that if you can't brake to a stop first and get off the road, you're probably going to be dead when you rear-end the guy going 40 in front of you in the fog.

The place to run your world-model is at the cognitive levels where you reason things out from experience and symbolic knowledge about the world. The time to run it is before you enter the fog bank. The model runs something like this:

That is a fog bank ahead. I am aproaching it at 70 miles per hour. There might be an obstacle on that road like a car or a truck or a construction barrier. There is no chance in hell that I can keep from hitting it by the time I see it if I am still going 70 miles per hour when I get there. Conclusion: SLOW DOWN.

You might reason this out in words, or you might simple imagine a terrifying scene based on imagination and experience, Either way, the model is a good way to anticipate trouble to let you take action before it's too late. But this is a _high level_ model, meaning it takes time to run and to make predictions from it, and to implement whatever change of action is called for.

The actual modeling is only part of the process; most of it uses the same perceptions you use in real time. The main difference is that you're imagining being in a different situation from the one you're actually in -- the situation that will hold 10 or 20 seconds from now.

>It seems to me that a PCT savvy control engineer facing the sort of problem
>Hans is concerned about (with the controller working in an environment in
>which the signal tends to vary in a way that can usually be predicted fairly
>well in the short term) would design a system that would attempt to model
>these systematic variations (as Han's system does) and use this model, not
>to generate computed _outputs_, but to generate a computed substitute
>_input_ to what otherwise would be an ordinary PCT-style controller.

This won't work because you would also have to model the disturbances acting on the input. Normally you don't perceive the effects of disturbances because the output's opposing them. Also, there's a serious problem with the system's gain. With the feedback loop open, the output will be very sensitive to small changes in the input, so you'd have to simulate the input with great accurary to get the right output. Since all you want is the right output, it would be much more direct and appropriate to generate the output.

But all that assumes that any disturbances are going to be small or very slowly varying. If that's not the case, then when you lose input you simply lose control. The simulated output will quickly become inappropriate. When a truck splashes mud all over your windshield, holding the wheel the way you were holding it when the world disappeared won't save you for very long; all you can hope is that if you avoid turning the wheel you'll have time to roll down the window and stick your head out, or that you can find a substitute perception like looking out the side window at the edge of the road.

As Rick pointed out, most of our control processes have a large degree of redundancy. This isn't just a matter of having another sensor, like a second eye. A given controlled variable is usually covariant with many different perceptions. In driving a car, for example, the primary control is of a visual relationship, but there are also various inertial sensors that, for a time, can keep the car on a straight or even constantly curving path. When you turn the wheel, your whole body is accelerated sideways, and you can use

the steering wheel to control for zero lateral acceleration, which you feel as a sideward force from the seat. That can greatly reduce the curvature of the car's path, enough, perhaps, to postpone going off the road until you can stop. If you're visually tracking something by turning your head, and the object goes behind an obstacle, you can switch to maintaining the same kinesthetically-sensed rate of head turning, and when the object reappears on the other side you'll at least be looking close to where it is. Basically all you have to do is pick a perception that covaries with the perception you were controlling before, and you can maintain at least some degraded degree of control.

There are all kinds of possible answers as to how we manage to maintain a semblance of control when specific inputs are temporarily lost. Hans' way is just one of them, and considering its computational complexity and the amount of knowledge about the environment and body that are needed to make it work, one of the less plausible ones.

You have to realized that the basic idea for the Kalman filter was published in the 1960s (1963 if I remember right), and that tens of thousands of modeler-hours have been put into applying it to the one particular architecture that Hans has described. When I looked up references for the Kalman filter with Altavista, I got 20,000 hits. And of course none of these papers was concerned with how to apply similar principles to the adaptation of other kinds of control architectures like HPCT.

Here's my point. If the same amount of labor had been put into applying these adaptive methods to a PCT model, it would be much easier to put aside the distraction of "adaptiveness" and simply look at the basic organization being proposed. The "modern control theory" approach is basically an attempt to conform to a top-down, command-driven type of system in which the point is to compute the right output. That basic concept absolutely REQUIRES an internal world-model, and also the ability to compute the INVERSE of the same world-model. Once you start down that road there's no turning. The shape of the final model is foreordained.

A modular hierarchical negative feedback architecture offers all kind of alternatives to the "modern control theory" approach. The solutions that suggest themselves for various problems are very different from those in the MCT approach. The kind of reorganizing process that is needed is different; the amount of information about the external world that is needed is different (less); and even the criteria for reorganization are different. The basic organization, adaptation aside, is much more robust, particularly in a world where disturbances are both ubiquitous and unpredictable.

Give us a world in which you will get 20,000 hits in looking up "control of perception" and the status of the PCT model will be greatly different. Someone on CSGnet commented, years ago, that PCT is never fairly compared with other approaches by their proponents. Most other approaches have been under serious development not only for some years, even decades, but with the help of lots of people with access to lots of resources. When a PCTer is asked what PCT has to say about some popular research question, the chances are that this question has never arisen before and no attempt has ever been made to answer it in PCT terms. The PCTer is being asked, in effect, to report on the results of an extended research project that has never been done.

This leaves the PCTer with an unsatisfactory choice; either wing it and made an intelligent guess on the spot about what a proper PCT model would say (and get it wrong), or say "Gee, I don't know, I've never thought about that" (and sound like a dilettante).

The well-developed approaches have all the advantages of incumbency, which as we know have nothing to do with merit.

Best,

Bill P.

Date: Thu, 12 Dec 1996 14:37:12 +1100
From: Avery Andrews <andaling@PRETTY.ANU.EDU.AU>
Subject: Modelling, Apes & Self

[Avery Andrews 961213]

There's an article in Nov. Discover that argues that the kind of self-concept that apes (including us) have originates from the `clambering' mode of locomotion used by orangutans. Somewhat PCT-ified, the story goes like this. Orang utan locomotation is difficult, because they're heavy, and have to really think about where they put their weight, in particular what will happen to the position of a tree-branch of they let go of the other ones they're holding on to (one common technique is to get to where they're going by committing their weight to a branch that swings down and also out to whereever they want to go).

This requires both sophisticated modelling, and a self-concept, since one of the things that has to be modelled is the contribution of the self to the configuration of branches. The argument assumes that this style of locomotion was used by the common ancesteor of orangatuan, chimps, bonobos gorillas and people, since these are the only creatures that are able to figure out that when they're looking at themselves in a mirror.

Avery.Andrews@anu.edu.au

Date: Wed, 11 Dec 1996 23:08:48 -0700
From: "T. B. Harms" <tbh@TESSER.COM>
Subject: Re: New papers on complex systems, economy and control

<From Tracy Harms 1996;12,11.23

Bill Powers (961211.1320 MST)

>Suppose we plot the demand curve for an individual and for a given good or
>service, but using the basic idea of a control model.

Your proposed demand curve is so far from what is used in economics that I'm doubtful whether it is reasonable to give it the same label.

>[...]
>This doesn't look much like the demand curves you see in economics texts,
>which are more like hyperbolas that descend smoothly over the whole range
>from a high value at the left to zero far to the right. But I contend that
>this is only because those demand curves are for populations rather than

>individuals.

But standard demand curves *are* proposed as applicable to individuals, Bill. They represent the number of units which will be sold to a given market (be it one buyer or many) depending on price, in a given preference-context.

Tracy Bruce Harms tbh@tesser.com
Boulder, Colorado

Date: Thu, 12 Dec 1996 00:32:53 -0700
From: "T. B. Harms" <tbh@TESSER.COM>
Subject: Re: New papers on complex systems, economy and control

<From Tracy Harms 1996;12,11.23.30

Bill Powers (961211.1320 MST)

I posted my prior response too quickly. There are other problems which I can't resist speaking to.

>The "supply" curve can be derived in a similar way,
>by interchanging the axes.

This makes no sense. If what you wish to diagram is preference, by charting the inspired degree of effort a buyer makes under differing inventories, the axis which measures effort must remain the same if we attempt to do something similar for preferences of a seller. Whether this sort of diagramming is actually worth doing or not, that axis must be the same for both.

>What the supplier demands is the efforts of individual workers.

Not only is this not so, that's all beside the point, for what you're doing here is not discussing anything like the supply curve; instead you are talking about the demand curve again, having merely switched your consideration to a different product.

>The supplier's "effort" is the amount of goods that will be produced
>to obtain these efforts from the individuals

But it is the quantity of goods produced which is controlled by the supplier, not the efforts of those from whom the supplier buys.

>(where of course the "goods" can be measured in terms of
>an equivalent amount of money

Of course? And what, pray tell, extablishes an equivalent amount of money for a given good? Methinks you have overlooked what economics must explain. If anything is clear from supply curves and demand curves, it is that the same product would be traded across a range of prices. One cannot presume monetary equivalents for goods without ripping all the explanatory power out of these economists' devices.

Tracy Bruce Harms tbh@tesser.com
Boulder, Colorado

"The life of humanity is so long, that of the individual so brief, that we often see only the ebb of the advancing wave and are thus discouraged. It is history that teaches us to hope." -- Robert E. Lee

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Date: Thu, 12 Dec 1996 12:35:58 +0100
From: Stefan Balke <sbalke@HRZ.UNI-BIELEFELD.DE>
Subject: Re: PCT Acceptance

>From Stefan Balke (961212.1200 CET)

Rick Marken (961211.1050) --

>I've had the same experience as well. ...
>
>I'm glad that applied people tend to accept PCT. My only problem with this is
>that applied people are often so busy solving the problems at hand that they
>have no time to carefully test and expand the theory.
>
>I think the tools used by the applied people -- the systematic procedures
>that are used in a PCT-based approach to clinical treatment or education --
>have to be developed by the academic (research) types. The applied people
>just don't have the time, the skills or the inclination to do this. That's
>why it's too bad (from my point of view) that the academic types have not
>gotten into PCT.

Here we need PCT researchers going into the practice. And as Kurt Lewin stated 'there is nothing as practical as a good theory' I see a synergetic effect. The practice gives the questions and the theory a first idea of the solutions, which will raise further questions and so on. The important thing is that the field researcher has accepted PCT at his own principle level. In this case he will have the desire to develop PCT related questions. In my case I have to look how Ed's program workes. There are questions like: is there a conflict between old and new memories (under the assumption that either the old or new memory can set the lower order reference level) while dealing with disruptive kids, based upon the observation, that some teachers tell about difficulties to decide themselves to apply the new questions although they say that they agree with the basic assumptions of the program.

But, you are right, to do the test for the controlled variable seems to be hard under school conditions. I would like to have a PCT-based model about decision making. I don't think that it's all random reorganization, because the decisions people take at the program level are always within the range of their principles. There must be something like an overall check whether the decision will probably (according to the own memories) lead to undesired consequences. In this sense an error will not be removed by a random process, but by an initial random process and a subsequent filtering process.

Best, Stefan

End of CSGNET Digest - 11 Dec 1996 to 12 Dec 1996
****

Date: Fri, 13 Dec 1996 08:00:21 -0600
Subject: CSGNET Digest - 12 Dec 1996 to 13 Dec 1996

There are 18 messages totalling 1112 lines in this issue.

Topics of the day:

1. Periodicity; event control (2)
2. Modelling, Apes & Self (3)
3. New papers on complex systems, economy and control (6)
4. PCT-based control engineering, Bill's paper
5. economics & HPCT (2)
6. Reactions of others to PCT
7. observation and theory
8. macroeconomic outcomes
9. primates, animal behavior

Date: Thu, 12 Dec 1996 10:12:55 EST
From: Bruce Gregory <gregory@CF4.HARVARD.EDU>
Subject: Re: Periodicity; event control

[
[From Bruce Gregory (961212.1015 EST)]

Martin Taylor 961211 16:50

> All I want to do is to point out two things: (1) that
> oscillation in complex networks with gain is extraordinarily hard to avoid,
> and (2) that the control of event perception is (seems to me to be) an
> aspect of sequence control that requires the kind of delay that is involved
> in oscillation and internal clocks, which almost certainly exist in us.

Thanks for the very informative post.

Bruce Gregory

Date: Thu, 12 Dec 1996 08:56:39 -0700
From: Bill Powers <powers_w@FRONTIER.NET>
Subject: Re: Modelling, Apes & Self

[From Bill Powers (961212.0630 MST)]

Avery Andrews 961213] --
> Orang utan locomotation is difficult,
>because they're heavy, and have to really think about where they
>put their weight, in particular what will happen to the position of
>a tree-branch of they let go of the other ones they're holding on
>to (one common technique is to get to where they're going by committing
>their weight to a branch that swings down and also out to whereever
>they want to go).
>
>This requires both sophisticated modelling, and a self-concept, since
>one of the things that has to be modelled is the contribution of the
>self to the configuration of branches.

Gee, Avery, I would have thought it was the contribution of the BODY to the configuration of the branches that had to be modeled. Since the Self weighs only 0.0000315 nanograms, by latest estimates from mass-energy equivalence, I don't think the Self would bend a branch very much.

People always refer to "sophisticated modeling" when they don't have any idea WHAT model they're talking about. I don't think there's any modeling necessarily going on. Control of perception might be adequate to do the job. Of course I'd alway be willing to look at a model that works.

Best

Oncle Bill.

The argument assumes that this
>style of locomotion was used by the common ancesteor of orangatuan,
>chimps, bonobos gorillas and people, since these are the only creatures
>that are able to figure out that when they're looking at themselves
>in a mirror.
>
> Avery.Andrews@anu.edu.au
>
>

Date: Thu, 12 Dec 1996 08:56:42 -0700
From: Bill Powers <powers_w@FRONTIER.NET>
Subject: Re: New papers on complex systems, economy and control

[From Bill Powers (961212.0645 MST)]

Tracy Harms 1996;12,11.23 --

Woops! I thought I was replying to a post from Francis Heylighen -- did my reply go to CSGnet? If so, I'll have to send a copy to Francis. Let me know quick!

>Your proposed demand curve is so far from what is used in >economics that I'm doubtful whether it is reasonable to give it >the same label.

This is the demand curve for one single individual, not for a population. The whole point is that it DOESN'T look like the curve used in economics texts, yet when you add up the individual curves over a population, you will get something that looks like the standard demand curve.

>>[...]
>>This doesn't look much like the demand curves you see in >>economics texts, which are more like hyperbolas that descend >>smoothly over the whole range from a high value at the left to >>zero far to the right. But I contend that this is only because >>those demand curves are for populations rather than individuals.
>
>But standard demand curves *are* proposed as applicable to >individuals,

Bill.They represent the number of units which will >be sold to a given market (be it one buyer or many) depending on >price, in a given preference-context.

It's ASSUMED that because such demand curves might be seen in a population, they would apply to each individual. I'm saying that the curves for an individual are NOT the same as the population curves. See my paper in Hershberger's _Conation and control_ for a clear demonstration of how a population characteristic can be completely wrong as an indicator of the individual characteristics that make it up.

I should think that economists would be delighted to be given some real rationale for the imaginary demand curves they draw. On a supply-demand plot, the only real point is the one where the curves intersect. All other points are hypothetical, figments of the imagination. Economic theories are almost completely data-free, which is why they work so poorly.

If you want another example of how economists confuse the global with the local, just consider the Keynesian concept of savings (this is all due to my father, who has just published his own book on economics). Keynes speaks of savings in terms of a family. When young, the family borrows money to live on (a strange view of the attitudes of lending institutions, but never mind); in its middle years it has a surplus, paying back the loan and saving money for the future; near the end, it draws its savings out to live on. And so, he says, it is for the nation as a whole. Just add more zeros to the amounts involved, and you have the course of the national economy.

Unfortunately, as my father points out, this requires that all fathers and mothers get married on the same day. In fact, at the same moment that one family is depositing money into savings, another family, farther along in the cycle, is drawing down its savings to support its retirement. The net savings rate over the population, in constant dollars per year, must be close to zero. The nation as a whole is not simply a magnified version of a single family. In fact, the laws of macroeconomics are totally different from those of microeconomics, precisely because of such composite effects. One of the consequences of this fact is that national savings, if they exist at all, far from being the wellspring of investment that fuels economic growth, actually constitutes a drag on the economy. The withdrawal of any substantial amount of money from circulation, on the national scale, has essentially the same effect that Greenspan has when he increases interest rates.

The difference between my father's economic theory and others is that, as a retired chemical engineer, he started with the data in the Statistical Abstracts and other sources, and looked at what actually happens instead of what theory says should happen. Did you know that raising interest rates either has no effect on inflation, or worsens it? That's clearly not what Greenspan believes, but it's what actually happens.

Best,

Bill P.

Date: Thu, 12 Dec 1996 08:56:47 -0700
From: Bill Powers <powers_w@FRONTIER.NET>
Subject: Re: New papers on complex systems, economy and control

[From Bill Powers (961212.0730 MST)]

Tracy Harms 1996;12,11.23.30 --

>I posted my prior response too quickly. There are other problems >which I can't resist speaking to.
>
>>The "supply" curve can be derived in a similar way,
>>by interchanging the axes.
>
>This makes no sense. If what you wish to diagram is preference, by >charting the inspired degree of effort a buyer makes under differing >inventories, the axis which measures effort must remain the same if we >attempt to do something similar for preferences of a seller. Whether >this sort of diagramming is actually worth doing or not, that axis >must be the same for both.

The _buyer's_ output (effort,money) is the _seller's_ input. The _seller's_ output (goods, services, wages, capital income) is the _buyer's_ input.It's a question of which is considered the independent variable. If the labels on the axes stay tied to the same physical quantities, then the roles of the variables are swapped when you talk about the seller. This is just how you get two intersecting curves on the same plot to solve graphically for the actual state of the system.

>>The supplier's "effort" is the amount of goods that will be produced
>>to obtain these efforts from the individuals
>
>But it is the quantity of goods produced which is controlled by the >supplier, not the efforts of those from whom the supplier buys.

The composite consumer's effort is the composite supplier's cost of production. The supplier can't arbitrarily control the level of output; it must be neither less than the buying power available to purchase it, nor greater, because the composite supplier uses the income from the sale of goods and services to pay those who purchase the goods and services -- and also produce them. The consumers have no other source of income. In macroeconomics there is a circular flow; if the flow were completely conserved, the producer's income would be exactly equal to the producer's costs, which consist entirely of wages for production and maintenance workers and capital income for owners and investors. And those costs for the composite producer are identically the buying power of the composite consumer, exced\pt for money leaked out of the economy which accounts for a portion of inflation.

>>(where of course the "goods" can be measured in terms of
>>an equivalent amount of money
>
>Of course? And what, pray tell, extablishes an equivalent amount of >money for a given good?

The price, which is in turn determined by the costs of production, which are the source of the consumer's buying power. This is a circular system of causality, in which you can understand the parts only in terms of the whole.

>Methinks you have overlooked what economics must
>explain. If anything is clear from supply curves and demand curves, >it is that the same product would be traded across a range of prices. >One cannot presume monetary equivalents for goods without ripping all >the explanatory power out of these economists' devices.

What explanatory power? Supply and demand curves are figments of the economist's imagination. In microeconomics, it is true that prices vary and that there are local short-term imbalances of supply and demand as inventories rise and fall. But globally, on the national scale, supply and demand are always equal, except for leakage of buying power out of the economy as a whole. No company can stay in business if its inventory either rises or falls indefinitely. Nationally, inventory is constant and as close to zero as can be managed.

The historical record shows that the fraction of total _composite_ producer income that is spend on capital goods (that is, on the labor required to supply them, raw materials themselves being free) is always close to 20% of total income, varying no more than 1 or 2 percentage points above and below that fraction for the past 100 years, in good times and bad. So much for the economic theory that investment drives growth.

The markup of price over cost is also essentially a constant of the economy; it is exactly what is needed to make up for the 20% of producer income spent on capital goods and maintenance: price has remained steady at 1.25 times the cost, in constant dollars, over the last 100 years. That is also a fact from the historical record (rather than from economic theory).

In truth, economic theory has very little to do with the facts.

Best,

Bill P.

Date: Thu, 12 Dec 1996 08:56:52 -0700
From: Bill Powers <powers_w@FRONTIER.NET>
Subject: Re: Periodicity; event control

[From Bill Powers (961212.0810 MST)]

Martin Taylor 961211 16:50 --

Your post on oscillatory phenomena was excellent, a good guide to further thinking on the subject.

In particular, your comments about the "event" level are useful. It's true, as you say, that an event can't be controlled while it is in progress (although the comprising transitions and configurations etc. can certainly be defended against disturbance during the event, since events don't actually take place at an instant of time). Yet we do control events in another sense: we can control _repeated_ events, by slowly adjusting the transitions etc. from one instance to the next, like gradually altering the way we pronounce a word-event. It would seem that repetition is an essential dimension of the event level, and that only through repetition can we control events.

Repetition, of course, is also an essential feature of oscillation or pattern generation. One cycle of any repeating pattern is what I would call an "event" -- the fact that we think of it as "one cycle" shows that we treat some space-time package of variations as a single entity. We say that "it" repeats, where "it" is what I mean by an event. Even the term "repeat" implies that _the same thing_ is happening again. What would that "same

thing" be, but an event?

This level of control just above the transition level, I think, has something to do with all these subjects: eventness, repetition, oscillation, and pattern perception and generation. I don't know whether the term "event" is sufficient to capture what is perceived and controlled at this level. Let's keep it simmering on the back burner.

As to sequences, we have here a recurring problem, which is that of applying a higher level of perception to a lower-level system that doesn't work in terms of that perception. Yes, when you observe an event, particularly if you also observe its component transitions and configurations, you can see that things occur in sequence. One thing changes before or after another. But you can see the same thing at ANY lower level, which doesn't mean that all lower levels are sequence control systems.

Sequence, as I think of it, is defined strictly in terms of ordering. You can go forward or backward through an ordered list without changing its order. You can proceed through the list at a constant speed or a variable speed, or irregularly, without changing the order. But if you change the ordering or leave out an element, there is definitely an error. It was Gary Cziko who pointed out this difference between an event and a sequence some five years ago, and who suggested that the event level (which used to be called the sequence level) needed to be split up. That's when I realized that Gary isn't as dumb as everyone says he is :-).

Anyway, a nice post Martin.

Best,

Bill P.

Date: Thu, 12 Dec 1996 17:16:56 +0100
From: Oded Maler <Oded.Maler@IMAG.FR>
Subject: Re: Modelling, Apes & Self

[From Oded Maler 961212)]

> [From Bill Powers (961212.0630 MST)]
>
> Gee, Avery, I would have thought it was the contribution of the BODY to the
> configuration of the branches that had to be modeled. Since the Self weighs
> only 0.0000315 nanograms, by latest estimates from mass-energy equivalence,
> I don't think the Self would bend a branch very much.

Speak for Your Self! I'm sure the self of some people weighs much more ;-)

--Oded

Date: Thu, 12 Dec 1996 08:42:43 -0800
From: Richard Marken <marken@AEROSPACE.AERO.ORG>
Subject: PCT-based control engineering, Bill's paper

[From Rick Marken (961212.0850)]

Bill Powers (961211.1640 MST) to Bruce Abbott (961211.1720 EST) --

>all that assumes that any disturbances are going to be small or very
>slowly varying. If that's not the case, then when you lose input you simply
>lose control. The simulated output will quickly become inappropriate.

I think it is important to make it clear that loss of input means loss of control _no matter what_. As you you note, Bruce's proposal for dealing with loss of input (via control of an imagined input with the output continuing to affect the now unperceived controlled variable) is not much different than Hans' model_based, computed output system. In both cases the systems will _appear_ to control as long as the environment remains nearly stable. But this is only the appearance of control; the system is not _really_ controlling what had been (while the input was present) the controlled variable.

My proposed approach to designing a redundant system that could deal with loss of input was aimed at making it possible for the system to continue _controlling_ after loss of input. As you note, the redundancy I was talking about wasn't "...just a matter of having another sensor, like a second eye". I was thinking of providing the system with several different ways of representing the same envinronmental situation. If I were designing a driving system (one that controls its position on the road), for example, and I knew that it might have to drive through dense fog, then I might provide it with visual _and_ radar sensors. The system might be a hierarchical control system where the higher level system switches between the system controlling the visual representation of road position to the one controlling the radar representation (or vise versa) depending on the size of the error signal in the road position control system that is currently in effect.

Anyway, I think a way to summerize your (and my) points about a PCT based apporach to designing a system that can deal with possible loss of input would be as follows: The goal of PCT based design is a system that _maintains control_ rather than one that simply _appears_ to maintain control (as long as the envinronment is stable).

Bill Powers (961212.0645 MST)

>See my paper in Hershberger's _Conation and control_ for a clear
>demonstration of how a population characteristic can be completely wrong as
>an indicator of the individual characteristics that make it up.

Actually, that paper is in the special issue of _American Behavioral Scientist_ that I edited (American Behavioral Scientist, Volume 34, Number 1 Sept/Oct 1990). And an excellent paper it is, indeed.

Best

Rick

Date: Thu, 12 Dec 1996 11:49:36 -0700
From: "T. B. Harms" <tbh@TESSER.COM>
Subject: Re: New papers on complex systems, economy and control

>[From Bill Powers (961212.0645 MST)]
>

>Tracy Harms 1996;12,11.23 --
>
>Woops! I thought I was replying to a post from Francis Heylighen -- did my
>reply go to CSGnet? If so, I'll have to send a copy to Francis. Let me know
>quick!

Your message went to the whole CSGnet.

>This is the demand curve for one single individual, not for a population.

What you have offered us is the demand curve for an individual at the margin. If you consider economic action in a very short time-frame there is a buy/don't buy alternative where buying involves a fixed quantity, in contrast with the general shape of a demand curve which maps obtaining increased quantities as a function of decreased price. You are right that lower cost does not translate into increased purchasing in an immediate situation. You are incorrect in saying that the classical demand-curve shape is inapplicable to a single purchaser. It does apply if the time-frame involved is sufficiently large.

>It's ASSUMED that because such demand curves might be seen in a population,
>they would apply to each individual. I'm saying that the curves for an
>individual are NOT the same as the population curves. See my paper in
>Hershberger's _Conation and control_ for a clear demonstration of how a
>population characteristic can be completely wrong as an indicator of the
>individual characteristics that make it up.

I agree that with complex systems composite changes are usually not good indicators of the dynamics of the components which produce them. However, the black-box treatment of what underlies demand-curve demand applies equally well to populations as it does to individuals for the portions of the curves which are where the practical action is. Decreased cost of a good does correlate with increased consumption, all other things being equal, for buyers--considered individually or en mass--over durations where the buyers favor multiple purchases.

>On a supply-demand
>plot, the only real point is the one where the curves intersect. All other
>points are hypothetical, figments of the imagination.

This is well understood among economists, is it not?

>Economic theories are
>almost completely data-free, which is why they work so poorly.

How would increased data improve economic theories?

>Did you know that raising interest rates
>either has no effect on inflation, or worsens it?

Yes, and my knowledge of that comes from *pure theory*, not from data. If the data are consistent with that idea, that indicates a strength where the theory prevailed against an empirical challenge.

>That's clearly not what
>Greenspan believes, but it's what actually happens.

I'm unconvinced that you have identified Alan Greenspan's motivating theories.

Tracy Bruce Harms tbh@tesser.com
Boulder, Colorado

"The life of humanity is so long, that of the individual so brief, that we often see only the ebb of the advancing wave and are thus discouraged. It is history that teaches us to hope." -- Robert E. Lee

---

Date: Thu, 12 Dec 1996 12:20:36 -0700
From: "T. B. Harms" <tbh@TESSER.COM>
Subject: Re: New papers on complex systems, economy and control

>From Tracy Harms 1996;12,12.12

Bill Powers (961212.0730 MST)

>>>The supplier's "effort" is the amount of goods that will be produced
>>>to obtain these efforts from the individuals
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>>But it is the quantity of goods produced which is controlled by the
>>supplier, not the efforts of those from whom the supplier buys.
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>The composite consumer's effort is the composite supplier's cost of
>production. The supplier can't arbitrarily control the level of output; it
>must be neither less than the buying power available to purchase it, nor
>greater, because the composite supplier uses the income from the sale of
>goods and services to pay those who purchase the goods and services -- and
>also produce them. The consumers have no other source of income. In
>macroeconomics there is a circular flow; if the flow were completely
>conserved, the producer's income would be exactly equal to the producer's
>costs, which consist entirely of wages for production and maintenance
>workers and capital income for owners and investors. And those costs for the
>composite producer are identically the buying power of the composite
>consumer, exced\pt for money leaked out of the economy which accounts for a
>portion of inflation.

You turn to macroeconomic analysis too rapidly. Macroeconomics has been a convenient place for economists to hide a multitude of sins. We must start with a sound and detailed microeconomics, and move to macro only with care to keep our macro vision compatible with the more fine-grained understandings.

The studies of economics which have been most significant have been those which do this. There is a direct relevance to PCT here: Microeconomics involves consideration of purposeful, intentional actors. This focus on units-of-autonomy engaged in iterative state-improvement is where PCT is most naturally applied. The intersection between psychology and economics is in the economic actors as individually generating aims which produce their behavior.

Anyway, suppliers *must* arbitrarily control the level of output, but this fact is clouded by imagining "the composite supplier," the aggregation of which presumes away the solution to problems of economic calculation. The mechanics of economic calculation must be *revealed* in our economic models, not relied on as hidden presumptions.

Any given supplier controls for a certain magnitude of product output over a duration. The trend is for the aggregate of such output to reflect the realities of demand, but only because miscalculation involves loss to the supplier, and every supplier is enacting a higher-level control for economic gain. None of these drivers are visible in your macro-loop descriptions, and their absence makes those models useless.

Tracy Bruce Harms tbh@tesser.com
Boulder, Colorado

"The life of humanity is so long, that of the individual so brief, that we often see only the ebb of the advancing wave and are thus discouraged. It is history that teaches us to hope." -- Robert E. Lee

---

Date: Thu, 12 Dec 1996 13:41:42 -0700
From: Bill Powers <powers_w@FRONTIER.NET>
Subject: Re: New papers on complex systems, economy and control

[From Bill Powers (961212.1245 MST)]

Tracy Harms 1996;12,11.23 --
>>
>>Woops! I thought I was replying to a post from Francis Heylighen -- >>did my reply go to CSGnet? If so, I'll have to send a copy to Francis. >>Let me know quick!
>
>Your message went to the whole CSGnet.

Thanks, I'll resend it to the right place.

>>This is the demand curve for one single individual, not for a population.
>
>What you have offered us is the demand curve for an individual at the >margin. If you consider economic action in a very short time-frame >there is a buy/don't buy alternative where buying involves a fixed >quantity, in contrast with the general shape of a demand curve which >maps obtaining increased quantities as a function of decreased price. >You are right that >lower cost does not translate into increased purchasing in an immediate >situation. You are incorrect in saying that the classical demand-curve >shape is inapplicable to a single purchaser. It does apply if the >time-frame involved is sufficiently large.

What the classical demand curve says that for that any good, if you offer it free the individual will consume an infinite amount of it (or consume it at an infinite rate). Do you think that's really true? The PCT view would be that for each good or service, the person has a specific reference level for the amount wanted, and will not put out any effort to get more than that. Of course over time the amount wanted can change, and it can change with other circumstances, in either direction. In conditions of scarcity, furthermore, we may be seeing conflicts, where to satisfy one want fully would be to prevent satisfaction of others, so there must be tradeoffs.

When you speak of individual demand curves, you're really talking about theoretical psychology, not economics. The psychology of economics is basically reinforcement theory: the greater the promised reward, the incentive, the greater will be the effort to obtain it. "Every man has his

price." I think there may be a self-selection factor here, in that the people most concerned with economics and thus economic theories may fit this description better than most other people do. But I don't think it's really true of any normal person.

>... the black-box treatment of what underlies demand-curve demand >applies equally well to populations as it does to individuals for the >portions of the curves which are where the practical action is. >Decreased cost of a >good does correlate with increased consumption, all other things being >equal, for buyers--considered individually or en mass--over durations >where the buyers favor multiple purchases.

But this is not determined by investigating individual behavior, is it? It's found through statistical marketing studies. That is a population study, which is simply assumed to apply to the individual. What I'm suggesting is that the demand curve which seems to exist in the population really reflects a _distribution_ of individual demand curves which have quite a different shape. As you raise the cost of a good, you first eliminate those people who want only a small amount of it, if any (like jalapeno sauce). Then you eliminate the majority that want amounts distributed around some mean value, and finally at the highest costs you eliminate even the few individuals with an abnormally large desire for the good. This gives you a smooth curve which is easily taken to representing the "average individual's" demand curve. In fact it represents no individual.

>>On a supply-demand
>>plot, the only real point is the one where the curves intersect. All
>>other points are hypothetical, figments of the imagination.
>
>This is well understood among economists, is it not?

Is it? Then why are these curves taught as facts?
>
>>Economic theories are
>>almost completely data-free, which is why they work so poorly.
>
>How would increased data improve economic theories?

It would confront their predictions from theory with what actually happens.

>>Did you know that raising interest rates
>>either has no effect on inflation, or worsens it?
>
>Yes, and my knowledge of that comes from *pure theory*, not from data.

That's interesting -- what theory is that?

>>That's clearly not what
>>Greenspan believes, but it's what actually happens.
>
>I'm unconvinced that you have identified Alan Greenspan's motivating theories.

Well, I've heard it said that Greenspan offers "combating inflation" as his reason for wanting to keep the economy from becoming "overheated", but that his real reason is to keep labor in a poor bargaining position. I don't know. Which is better, to accuse him of ignorance or of evil intentions? I've been giving him the benefit of the doubt.

Best,

Bill P.

Date: Thu, 12 Dec 1996 14:59:59 -0700
From: Bill Powers <powers_w@FRONTIER.NET>
Subject: Re: New papers on complex systems, economy and control

[From Bill Powers (961212.1345 MST)]

Tracy Harms 1996;12,12.12 --

You say that your preference is to derive macroeconomics from microeconomics, and I generally agree with this approach. But having seen what can be done with macroeconomics, I am also convinced that there is merit in looking at the boundary conditions as well. For one thing, the assumptions about individual characteristics are far fewer in macroeconomics, and there is less reliance on multiple untestable assumptions. If I were to try to model the whole economy, I would prefer to start at the level of the individual, then built up to the emergent picture from there. However, since there are so many possible interaction paths and there is so much variation across individuals, I think it is helpful to have an idea of the global constraints on the system as a whole.

>You turn to macroeconomic analysis too rapidly. Macroeconomics has been a
>convenient place for economists to hide a multitude of sins. We must start
>with a sound and detailed microeconomics, and move to macro only with care
>to keep our macro vision compatible with the more fine-grained
>understandings.

I agree with all that, too, but there is an inevitable interplay between "whole-picture" and "fine-grained" understandings. I think my father, who writes about macroeconomics, would agree wholeheartedly with you about the "multitude of sins." He has spent 20 years comparing what macroeconomists have said about the economy with the record of what has actually happened, and has come to the conclusion that they basically don't know what they're talking about. When you begin with the data and work out the relationships that actually seem to hold, you come up with a very different picture, and one that is both simple and defensible.

Ideally, a valid macroeconomic analysis will be consistent with a valid microeconomic analysis, just as thermodynamics is consistent with the kinetic theory of matter. Each level of analysis can correct errors in the other. If a particular aspect of molecular theory violates the second law of thermodynamics, we know that that aspect probably contains a mistake. probably a mistaken assumption since logical errors are not likely to escape notice. And as Martin Taylor has said to me in other contexts, it is not necessary to trace the paths of individual molecules to come up with the useful notions of temperature and pressure.

>There is a direct relevance to PCT here: Microeconomics
>involves consideration of purposeful, intentional actors.

I couldn't agree more and my father couldn't agree less -- he is adamant that "individual characteristics" have nothing to do with macroeconomics. However, I can put that disagreement aside and see that his global analysis is still correct, although he denies what I see as its actual basis.

>The intersection between psychology and economics
>is in the economic actors as individually generating aims which produce
>their behavior.

Yes, indeed. These actors, however, operate within constraints that they themselves have constructed. There is a social covenant that says you must pay for goods or services received if payment is demanded, and that when you supply goods or services you must be paid for them if that is what you want. And the regulation of money and credit requires bookkeeping, so that income and expenditures must balance; money or its equivalent is the means of ensuring that balance as nearly as possible. There is nothing in PCT that could predict this way of playing the game; it was invented by its players.

This is the convenant that makes sure that all of the income of a producer comes from some kind of consumer, and that all of the income of the consumer comes from some kind of producer -- to a close approximation. Nobody is permitted to spend money that is not taken from income or a corresponding debt: that is, counterfeiting and stealing are against the law. What all these constraints add up to is the circular flow of macroeconomics. The price for which the output is sold is balanced against the wages of those who do the producing plus the income of those who are paid by virtue of ownership or investment. When we blow away the fog of other macroeconomic theories, I think we are left with a rather straighforward picture of how the system functions. We don't end up with an _explanation_, but we do see more clearly what needs explaining.

>Anyway, suppliers *must* arbitrarily control the level of output, but this
>fact is clouded by imagining "the composite supplier," the aggregation of
>which presumes away the solution to problems of economic calculation. The
>mechanics of economic calculation must be *revealed* in our economic
>models, not relied on as hidden presumptions.

Yes, again. But the microeconomic models must reveal what is actually observed, or they must go back to the drawing board. No matter how meticulous their construction or how inevitable their mathematics, if micro theories predict something other than what is observed at the macro level, they are wrong.

>Any given supplier controls for a certain magnitude of product output over
>a duration.

I can see a production manager doing that, but not the highest levels of management. What the highest levels of management are concerned with is quantity of producion times price per unit produced, and that in relation to cost of production in terms of both wage and capital income (which includes profits, however calculated, for those who receive them).

>The trend is for the aggregate of such output to reflect the
>realities of demand, but only because miscalculation involves loss to the
>supplier, and every supplier is enacting a higher-level control for
>economic gain. None of these drivers are visible in your macro-loop
>descriptions, and their absence makes those models useless.

It's true that these drivers are not visible in the macro picture, but that doesn't make the macro picture useless. The macro picture, if properly constructed, simply describes the outcome of all these micro relationships. In fact, it describes the observable outcomes which any micro theory, if it is correct, must predict. Remember that I am talking about an analysis of the macroeconomic system that is derived from observation, not theory or

theoretical assumptions. As far as I know, my father is the only person who has actually tried to construct an analysis like this (he refuses, with gestures, to hear it referred to as a "theory". At the age of 96, he has a right).

Just as an example, would it not help the microeconomic theorist to know that the proportion of income of the composite producer devoted to capital investment (depreciation, plant expansion, etc.) has been essentially constant at 20% for 100 years? Or that the markup of constant-dollar prices over costs, as a consequence, has been constant at a factor of 1.25 plus or minus a very small deviation? This means that if one producer uses a larger markup, for some reason others must use a smaller one. This says something about interactions at the micro level that might be very hard to derive from first principles. The microeconomist might be able to come up with those interactions -- but only if he knows there is a phenomenon there to be explained, a constraint that must be met. There are other interesting relationships that can be found in the historical record, and each one of them is a hint as to what any microeconomic theory must explain.

Best,

Bill P.

Date: Thu, 12 Dec 1996 16:30:43 -0700
From: "T. B. Harms" <tbh@TESSER.COM>
Subject: economics & HPCT

>From Tracy Harms 1996;12,12.16:

Bill Powers (961212.1245 MST)

>What the classical demand curve says that for that any good, if you offer it
>free the individual will consume an infinite amount of it (or consume it at
>an infinite rate). Do you think that's really true?

No, but I don't think the classical demand curve need be interpreted in that way. That's why I spoke earlier of "the portions of the curves which are where the practical action is." I think it is also reflected by the fact that demand curves are universally drawn only in a center section, not as a zero-to-infinity function. Possible consumption does not go to infinity, but as price changes pretty much never involve something going to zero cost, that doesn't matter for the specific relationship which supply-and-demand diagrams are suited to communicate.

>The PCT view would be
>that for each good or service, the person has a specific reference level for
>the amount wanted, and will not put out any effort to get more than that.

That is one possible application of PCT, but I think there are better ways to see it. PCT suggests to me that actors have high-level aims (reference levels) which they will act to satisfy by some combination of choices, including purchases. But while any given act of purchasing involves a short-term satisfaction of a reference level to obtain an item in the market, because those purchases are behavior instrumental to a higher aim they are in a more important way uncontrolled.

Where this fits nicely in economic theory is the question of substitutability and rankings of preference. The famous examples involve what individuals choose when a contemplated contender for purchase is rejected; the subsequent option often has no clear comparison with the first. PCT makes such facts seem very natural.

For instance: A salesman for high-cost European automobiles was asked about his competition, and said that the competition which worried him the most was travel cruises, yachts, and vacation homes. That shows a clear intelligence regarding what it is that his customers are controlling for, and dispells the naive presumption that if the price of a Mercedes is a bit too high, a Volvo (etc.) will be purchased instead. This example shows the weakness in your claim "that for each good or service, the person has a specific reference level for the amount wanted." The potential Mercedes buyer may well *not* have a reference level to buy a car. The purchase of a car is but one possible behavior to satisfy a more abstract reference level. It would be a mistake to think of reference levels as being *product specific*, or even category specific. In regard to need-satisfaction consumption, product purchasing is a behavior, and behavior is not being controlled. The short duration where the sale is actually negotiated and concluded does entail a temporary reference level for "buy one of these", but that occurs only once the decision to buy has been made and so does not illuminate the larger question of economic choice.

Tracy Bruce Harms tbh@tesser.com
Boulder, Colorado

"The world has arrived at an age of cheap complex devices of great reliability; and something is bound to come of it."
Vannevar Bush

Date: Thu, 12 Dec 1996 15:32:00 -0800
From: Philip Runkel <runk@OREGON.UOREGON.EDU>
Subject: Reactions of others to PCT

This is from Phil Runkel.

Rick Marken wrote as follows on 11 Dec 96:

>I've had the same experience as well. I find that the people who accept
>PCT most readily are those who are outside of academia, using behavioral
>theories to solve "real world" problems. I am thinking, in particular,
>of clinicians and educators.

>... applied people are often so busy solving the problems at hand that they
>have no time to carefully test and expand the theory.

>I think the tools used by the applied people -- the systematic procedures
>that are used in a PCT-based approach to clinical treatment or education --
>have to be developed by the academic (research) types.

Your description of your experience is an exact description of my experience, also. And I agree with you that the research types will usually be the ones to produce the systematic procedures.

Keep at it, please.

Best wishes for holidays and all other days, too, and to all to whom these presents may come.

--Phil R.

Date: Thu, 12 Dec 1996 17:13:38 -0700
From: "T. B. Harms" <tbh@TESSER.COM>
Subject: observation and theory

<From Tracy Harms 1996;12,12.14:40

Bill Powers (961212.1345 MST)

>Remember that I am talking about an analysis of
>the macroeconomic system that is derived from
>observation, not theory or theoretical assumptions.

No analysis is derived from observation in contrast with theory and theoretical assumptions. Theory always comes first. Observations are only possible within a theoretical framework. Your father may refuse to hear that, but his age will not exempt him from error.

Tracy Bruce Harms tbh@tesser.com
Boulder, Colorado

Tracy Bruce Harms tbh@tesser.com
Boulder, Colorado

"The world has arrived at an age of cheap complex devices of great reliability; and something is bound to come of it."
Vannevar Bush

Date: Thu, 12 Dec 1996 17:13:40 -0700
From: "T. B. Harms" <tbh@TESSER.COM>
Subject: macroeconomic outcomes

<From Tracy Harms 1996;12,12.17

Bill Powers (961212.1345 MST)

>The macro picture, if properly
>constructed, simply describes the outcome of all these micro relationships.
>In fact, it describes the observable outcomes which any micro theory, if it
>is correct, must predict.

As an example, I would be interested in how the macro theory you have in mind describes the change in the USA over the past century in the ratio of hours of agricultural labor to calories of food.

Tracy Bruce Harms tbh@tesser.com
Boulder, Colorado

Date: Fri, 13 Dec 1996 13:06:18 +1100
From: Avery Andrews <andaling@PRETTY.ANU.EDU.AU>
Subject: Re: Modelling, Apes & Self

[Avery Andrews 961213]
(Bill Powers (961212.0630 MST)

>Gee, Avery, I would have thought it was the contribution of the BODY to the
>configuration of the branches that had to be modeled. Since the Self weighs

A good change of terminology - it is specifically an own-body concept that the apes seem to have or be able to get (when exposed to a mirror for the first time, they eventually wind up using it to look at their genitals and other places that they can't normally see - monkeys and other kinds on non-human animals never do this, and treat the mirror-image animal as another animal, threatening it, etc.

The apes don't seem to show any awareness of states of mind, or other aspects of the self beyond the body.

>People always refer to "sophisticated modeling" when they don't have any
>idea WHAT model they're talking about. I don't think there's any modeling
>necessarily going on. Control of perception might be adequate to do the job.
>Of course I'd alway be willing to look at a model that works.

Would it sound better if I said `sophisticated imagination loop'? It is of course also possible that there is a simple trick that can be used, but humans do seem to use imagination to help decide what to do in a way that most other animals don't, and it would be nice to have some plausible and potentially testable ideas about how this got started. Otoh the Discover article mentions a primatologist who says that the dumb monkeys he studies locomote in similar ways to orangutans, without benefit of an own-body-concept, so maybe the whole thing is just wrong. Anyway, I'd call anything that can figure out the best of the 22 interesting choices of how to change your grasp/let-go references when you're hanging on to 4 different branches with 4 different limbs sophisticated.

- Avery.Andrews@anu.edu.au

Date: Fri, 13 Dec 1996 13:22:46 +1100
From: Avery Andrews <andaling@PRETTY.ANU.EDU.AU>
Subject: primates, animal behavior

[Avery Andrews 961213]

The recent history of primate & animal behavior in general seems to have been to have lurched from mindless behaviorism to profligate attribution of human-like mental states to all sorts of creatures (that silly book on the `secret life of dogs' is I suppose the nadir of that trend), now being followed by a more considered re-appraisal, to which PCT might be able to contribute by providing a space of hypotheses in between

SR-mechanisms and human-like mentality.

For example, a decade or so ago people seemed to be thinking that chimpanzees and bonobos had awareness of each others mental states, and attempted to control them, whereas it seems that what they actually do is try to control each others visible behavior, although often in clever ways, e.g. one male bonobo uses a `come hither to copulate' gesture to get another one to come over to the bush where he is, so that both can corner and capture a pig. The caller can be seen as controlling for the position of the call-ee, not sure how to explain the call-ee;s behavior, but at any rate the hunt is said to have been successful. (anecdote out of Barbara King, the _the information continuum_).

-Avery

Date: Thu, 12 Dec 1996 21:11:46 -0700
From: Bill Powers <powers_w@FRONTIER.NET>
Subject: Re: economics & HPCT

[From Bill Powers (961212.1940 MST)]

Tracy Harms 1996;12,12.16 --

>Bill Powers (961212.1245 MST)
>
>>The PCT view would be
>>that for each good or service, the person has a specific reference level
>>for the amount wanted, and will not put out any effort to get more than
>>that.
>
>That is one possible application of PCT, but I think there are better ways
>to see it. PCT suggests to me that actors have high-level aims (reference
>levels) which they will act to satisfy by some combination of choices,
>including purchases. But while any given act of purchasing involves a
>short-term satisfaction of a reference level to obtain an item in the
>market, because those purchases are behavior instrumental to a higher aim
>they are in a more important way uncontrolled.

That _is_ a better way to see it. A "good" isn't a good in itself, but has value only with respect to the goals it satisfies at higher levels (except for bodily needs such as food, and even then there are "junk" foods and "prestige" foods).

>Where this fits nicely in economic theory is the question of
>substitutability and rankings of preference. The famous examples involve
>what individuals choose when a contemplated contender for purchase is
>rejected; the subsequent option often has no clear comparison with the
>first. PCT makes such facts seem very natural.

Yes, you have to see what higher-order desires are being satisfied before you can understand how a substitution makes sense. If I can't have the diamonds, then I want to go to Monaco. I must say, you are getting pretty good at applying PCT to economics. You make my attempts sound a little superficial.

>For instance: A salesman for high-cost European automobiles was asked
>about his competition, and said that the competition which worried him the

>most was travel cruises, yachts, and vacation homes. That shows a clear
>intelligence regarding what it is that his customers are controlling for,
>and dispells the naive presumption that if the price of a Mercedes is a bit
>too high, a Volvo (etc.) will be purchased instead. This example shows the
>weakness in your claim "that for each good or service, the person has a
>specific reference level for the amount wanted."

Too true. The reference level is not for the good, but for a perceived
effect of obtaining the good. I bow to your more complete insight.

Tracy Harms 1996;12,12.14:40

>No analysis is derived from observation in contrast with theory and
>theoretical assumptions. Theory always comes first. Observations are only
>possible within a theoretical framework. Your father may refuse to hear
>that, but his age will not exempt him from error.

That's more true of some analyses than others. There's always some level of
theory at work, but if it's at a low enough level I don't think it's worth
quibbling over. What my father did was go over the Statistical Abstracts and
other materials to see how expenditures and income were reported since 1895,
using whatever sources were available. This is how he discovered the
constancy of capital expenditure as a fraction of total producer income, and
the independence of this fraction from the rate of growth (or contraction)
of GNP. There was no theoretical reason for anticipating this finding; it
simply became evident. Of course the data themselves are subject to
criticism, and concepts like "GNP" have a theoretical tinge, but since this
is the only data base available, one can analyze it without getting into
questions like that.

I see considerably more theory in my father's work that he will admit to,
but on the other hand I don't object to much of it, so that's sort of beside
the point. In part, his rejection of the word "theory" is the result of
having been born in 1900, and growing up in a time when theory was generally
considered to mean "wild unsubstantiated guess." I think I'd say that the
word "model" is closest to what he calls analysis.

My father's name is Treval C. Powers, known far and wide as TCP. It's a
damned good thing that I wasn't the one who thought up the label PCT for
what I do.

>As an example, I would be interested in how the macro theory you have in
>mind describes the change in the USA over the past century in the ratio of
>hours of agricultural labor to calories of food.

I'm pretty certain that my father would not consider this a macroeconomic
question. It deals only with one sector of the economy. His entire approach
is based on dealing with the nation as a whole, and with only composite
entities -- ALL the producers, ALL the consumers. The regularities and
relationships he has found are found only at that level, not in the
distributions of income or production among different sectors. In fact, one
of TCP's criticisms of other approaches to macroeconomics is that they
confuse micro phenomena with macro phenomena. This was Keynes' mistake when
he tried to extrapolate from a family to the nation as a whole, without
taking into account the fact that the _composite_ family is of no particular
age, economic status, or occupation, but rather all of them combined.

He also realized, quite on his own, that macroeconomic relationships can't
be understood as a sequence of events, but must be treated as simultaneous

interactions occurring everywhere at once. He explained this principle to me at length, and like a dutiful son I said, "Yes, Dad." Our relationship is not such that I could have said "I've been thinking like that for 40 years, Dad." I don't think he believes I could have been _alive_ for 40 years.

Best,

Bill P.

End of CSGNET Digest - 12 Dec 1996 to 13 Dec 1996
****

Date: Sat, 14 Dec 1996 08:07:53 -0600
Subject: CSGNET Digest - 13 Dec 1996 to 14 Dec 1996

There are 9 messages totalling 875 lines in this issue.

Topics of the day:

1. Early language learning (2)
2. Standing at the crossroads
3. Critique of Pure Reason
4. theory as dispositional bias
5. Popper's Preposterous Propositions (2)
6. Aristotle and Contingent Truths
7. Aristotle and Contingent Truths,Gifts

Date: Fri, 13 Dec 1996 11:58:20 EST
From: Bruce Gregory <gregory@CF4.HARVARD.EDU>
Subject: Early language learning

[From Bruce Gregory (961213.1200 EST)]

While driving to work this morning I heard a piece on NPR about an experiment on children's learning of language. The experimenters invented an artificial language consisting of four words of three syllables each. Infants of age eight months were exposed to a continuous stream of a computer generated voice speaking these words in all permutations for two minutes. Following this exposure, the children were able to identify the four "words" as different from other combinations of the syllables. My reaction was, "Smells like purposeful behavior. Wonder what the infants might be controlling for?" (Hint to Rick.)

Needless to say, my reaction was different from that of the experimenters. They are convinced that they have demonstrated the existence of sophisticated statistical analysis being performed by the infants to identify the "words". Statisticians seem to have no trouble discovering sophisticated statistical analyses in the most unlikely places...

Bruce Gregory

Date: Fri, 13 Dec 1996 10:34:43 -0700
From: Bill Powers <powers_w@FRONTIER.NET>
Subject: Standing at the crossroads

[From Bill Powers (961213.1000 MST)

In celebration of Christmas, Channukah, and other solsticial celebrations in Northerly climes to encourage the return of light, I enclose here a paper written for the 6th annual meeting of the Control Systems Group and published in Living Control Systems II. I suggest, subject to approval of all concerned, that it be substituted in the Introduction part of our Web page for my other materials presented there. I might make one or two changes and additions in the light of events of the past six years, but being lazy I shall not do so.

Best to all,

Bill P.

Standing at the crossroads

William T. Powers

Presented at an open session at the
Sixth Annual Meeting of
The Control Systems Group
Indiana University of Pennsylvania
August 16, 1990

Preliminary note. In keeping with the tradition of past meetings of the Control Systems Group, I will distribute this paper to those attending this presentation but will neither read it aloud, speak it from memory, nor summarize it. I will assume that you have read it. In my actual talk, I will show a number of phenomena of control that conventional theories either ignore or fail to explain. I will show a working computer model of a behaving system in which a little man points to a moving target, without any computation of the required movements. If you have read this paper, these demonstrations will make the paper clearer. If you have not, the demonstrations will make it easier to understand later. In either event, you will not have to sit through the drone of someone reading to you what you can read four times as fast by yourself.

I'd like to try today to give you the sense that psychology is standing at a crossroads -- and not only psychology, but all the sciences of life. We are about to experience the advent of something for which many people have searched, an organizing scheme that pulls together all the disparate schools of thought, specializations, movements, and evanescent fads that make up various fragmented branches of the life sciences.

The organizing scheme is called "control theory." This theory explains a phenomenon, as theories are supposed to do. The phenomenon in question is called control. Everyone has heard this

word, and most people have occasion to use it from time to time, but in science it has become part of the metalanguage rather than designating a subject of study. A scientist does a control experiment, or demonstrates how manipulation of stimuli and rewards can control an animal's movements, or advocates a proper diet to control cholesterol level, or competes for control of a department. This word is used as part of a background of ordinary language, but it has not been part of the technical language of the life sciences.

The reason is quite simple: nobody in or out of science understood the process of control until about the beginning of World War 2. By understanding the process, I mean being able to define it, characterize it, measure its parameters, predict how it will proceed, and recognize it in a real system. This doesn't mean that control was impossible to accomplished before World War 2: after all, most people accomplish digestion without understanding any biochemistry. But control is just as natural a process as digestion, and like digestion can be understood in a scientific way only by studying it and learning how it works.

World War 2 started only about 50 years ago. Perhaps you can see why this fact implies some problems with studying control as a natural process. If control is a natural process, it was occurring in 1840, 1740, 1640, and so on back to the primordial ooze. In 1940, the sciences of life were already something like 300 years old (and their prehistory was far older than that). If nobody understood control until 1940, it's clear that these sciences went through a major part of their development without taking it into account. The next question is obvious: how did they explain the phenomena that arise from processes of control?

Many of the puzzles and controversies that occupied early researchers could have been resolved if scientists had realized that they were dealing with control processes. Purpose could have been studied scientifically instead of merely theologically. We can see now that all these early researchers, not recognizing a control process when they saw one, were drastically misled by some side-effects of control. The principal side-effect that deceived them resulted from the way control systems act in the presence of disturbances of the variables they control. When a disturbance occurs, a control system acts automatically to oppose the incipient change in the controlled variable. But if this opposition is not recognized (it's not always obvious), the observer will inevitably be led to see the cause of the disturbance as a stimulus and the action opposing its effects as a response to the stimulus. Furthermore, this opposition results in stabilizing some aspect of the environment or organism-environment relationship. That stabilization conceals the role of the stabilized variable in behavior; the better the control, the lower will be the correlation between the controlled variable and the actions that stabilize it. The variable under control is the one that is actually being sensed, but the logic of control makes it seem that the disturbance is the sensory stimulus.

Donald T. Campbell has proposed a "fish-scale" metaphor of scientific progress. Each worker constructs just one small scale that overlaps those already laid down by others. Eventually the whole fish will be covered completely. But what if the fish is a

red herring? Then all these patient workers will devote their lives to covering the wrong fish. The converse of the fish-scale metaphor is that a person who is concentrating on fitting one little scale to others already laid down is bound to have a very localized view of the problem. Seeking to extend the accomplishments of others, a single worker can make what seems to be progress -- but it is unlikely that a single worker will discover that something is wrong with the whole design. The result can easily be the diligent application of fish-scales to a giraffe.

I submit that something like this has happened in the life sciences. A fundamental misconception of the nature of behavior, natural but nevertheless horrendous, has pointed the life sciences down the wrong trail. Nearly all life scientists, particularly those who try to achieve objectivity and uniform methodology, have interpreted behavior as if it were caused by events outside an organism acting on a passive mechanism. This hypothesis has become so ingrained that it is considered to be a basic philosophical principle of science. To explain behavior, one varies independent variables and records the ensuing actions; to analyze the data, one assumes a causal link from independent to dependent variable and calculates a correlation or computes a transfer function. This leads in turn to models of behaving systems in which inputs are transformed by hypothetical processes into motor outputs; those models lead to explorations of inner processes (as in neurology and biochemistry) predicated on the assumption that one is looking for links in an input-output chain. One assumption leads to the next until a whole structure has been built up, one that governs our thinking at every level of analysis from the genetic to the cognitive.

Control theory, by showing us an alternative way of understanding this entire structure, therefore threatens the integrity of practically every bit of knowledge about behavior that has ever been set down on paper.

This is, of course, a message of the type that leads to a high mortality among messengers. That is why you are listening to a person with no reputation to lose and no fame to protect, instead of a Nobel Prize winner. In an utterly predictable way, scientists have for the last 50 years gone to great lengths to avoid learning control theory or else to assimilate it into the existing picture of behavior. Failing that, they have simply declared it irrelevant to their own fields, with the result that the authoritative literature of behavioral control theory is almost completely insulated from the mainstream. It appears in publications like proceedings of the Institute of Electrical Engineers division on Man, Machines, and Cybernetics, or in human factors and manual control publications, or in Xeroxed papers passed from hand to hand. There is a scattered literature on control theory in the life sciences, but nothing on this subject gets past the referees into a standard journal without first having its teeth pulled.

Despite all the defenses, the concepts of control theory are spreading. When our descendants look back on the latter half of the 20th Century, they will probably be amazed at the speed with which control theory became accepted: 50 years in the course of a

science is nothing. We control theorists have nothing to complain about. Our greatest successes have come not through pounding at locked doors, but through continuing to explore the meaning of this new approach and learning how to apply it in many different disciplines. If we do our job correctly, acceptance will take care of itself. That job is not something one can toss off overnight, nor can it be done by just a handful of people. We are coming to a time of rigorous re-evaluation of all that is known or presumed to be known about the nature of organisms. The more people that are involved in this enormous project, the sooner it will be accomplished. That is why we are all so glad to welcome our guests at this session: after the party, you will be invited to help do the dishes.

There has been progress in understanding how organisms work, the wrong model notwithstanding. Biochemical reactions are not going to change because of control theory. Muscles and nerves will continue to operate as they are known to operate. Even at more abstract levels of analysis, many phenomena will continue to be accepted as valid observations; for example, phenomena of perception, of memory, of cognition. If competently observed, these phenomena will still be part of the legacy of earlier workers. When we pull the stopper on the old theories, we must keep a strainer over the drain and let only the bathwater out.

Part of the task of reconstructing the sciences of life consists of separating valid observations of components from invalid conjectures about how they work together. Consider biochemistry as an example. Biochemistry is an odd mixture of solid research and wild leaps of undisciplined imagination. The research reveals chemical processes taking place in the microstructure of the body. The wild leaps propose that the chemical reactions somehow directly produce the behavioral effects with which they are associated. It's as though a specialist in solid-state physics were to propose that electrons flowing through wires and transistors are responsible for the music that comes out of a radio. While it's true that a shortage of electrons will make the music faint, and that without the electrons you wouldn't get any music, the physicist would be laughed out of town for suggesting that electrons cause music, or that you could fix a weak radio just by putting some more electrons into it. You can't understand the role of the electrons without grasping the principles of organization that make the radio different from a radio kit.

In the same way, if shortages or excesses of chemicals like enzymes and neurotransmitters are found to be associated with functional and behavioral disorders, all we then know is that these substances play some role in the operation of the whole system that creates organized behavior. If there's a shortage of some chemical substance, then some other system has reduced its production of that substance, and some other system still has decreased its effect on the driving system, and so on in chains and causal loops. Nothing in a system as complex as the human body happens in isolation. If biochemistry is to have anything to say about the organism at any higher level, biochemists are going to have to study whole systems, not isolated reactions. We need a functional theory to supplement the microscopic laws of chemistry.

There are workers in biochemistry who are investigating feedback control processes. One significant process involves an allosteric enzyme that is converted into an active form by the effect of one substance, and into an inactive form by the effect of another. When these two substances have the same concentration, the transition from active to inactive is balanced; the slightest imbalance of the substances causes a highly amplified offset toward the active or the inactive form. In one example, the active form catalyzes a main reaction, and the product of that reaction in turn enhances the substance that converts the enzyme to the inactive form -- a closed-loop relationship. The feedback is negative, because the active form of enzyme promotes effects that lead to a strong shift toward the inactive form. This little system very actively and accurately forces the concentration of the product of the main reaction to match the concentration of another substance, the one that biases the enzyme toward the active form. This allows one chemical system to control the effects that another one is having on the chemical environment.

A person without some training in recognizing control processes might easily miss the fact that one chemical concentration is accurately controlling the product of a different reaction not directly related to the controlling substance. The effect of this control system is to create a relationship among concentrations that is imposed by organization, not simply by chemical laws. This is the kind of observation that a reductionist is likely to overlook; reductionism generally means failing to see the forest for the trees. Even the workers who described this control system mislabeled what it is doing -- they concluded that this system controls the outflow of the product, when in fact it controls the concentration and makes it dependent on the concentration of a different and chemically-unrelated substance.

To shift through several gears, consider the lines of research that began with Rosenblatt's perceptron. This device was conceived as a behavioral system that could be trained to react to patterns contained in its input information. First this idea was shown, by something of a hatchet job, to be impractical, and then it was shown to be practical again if several levels of training could occur within it (I haven't seen any apologies to Frank Rosenblatt, who died without vindication). In all its incarnations, however, the perceptron has been thought of as a system that learns to "respond correctly" to a stimulus pattern.

From the standpoint of control theory, however, organisms do not respond to stimuli but control input variables. So does that invalidate all that has been learned about perceptrons? Not at all. Control theoretic models desperately need something like a perceptron to explain how abstract variables can be perceived. In a control model, however, the perceptron is only one component: it provides a signal that represents an aspect of some external state of affairs. It's easy to show that behavior can't be explained simply by converting such a signal into an output action. But behavior can be based on the difference between the perceptron's output signal and a reference signal that specifies the state of the perception that is to be brought about. The control-system model shows where the functions that are modeled as perceptrons belong in a model of the whole system.

Shifting gears again: some theorists are trying to model motor behavior in terms of "motor programs" and "coordinative structures." In these models, command signals are presumed to be computed such that when applied to elastic muscles they produce the movements of a real limb. These models contain some impressive mathematics, taking into account the linkages of the limb and the dynamics of movement of the limb masses. But control theory says that behavior is not produced by computing output; it is produced by comparing inputs with desired inputs, and using the difference to drive output. No complicated "motor program" computer is needed. Does this mean that the mathematical analysis by the motor program people is spurious and ought to be discarded?

Again, not at all. At some point in elaborating the control model, we must show how the driving signals actuate muscles to cause the movements we actually see. This entails solving all the physical equations for muscle and limb dynamics, just as the motor programmers have done. If they did their arithmetic right, it will still be right when we substitute the control-system model for the central-computer model. Both models have to produce the same driving signals. The only thing that will change is that control theory will show how the required driving signals arise naturally from perception and comparison against reference signals, instead of being computed blindly from scratch.

Finally, shifting to overdrive, what do we do about Artificial Intelligence? We take advantage of whatever it really has to offer, modifying it only where we know it fails to explain enough. One place where it fails to explain enough is in the way it deals with action. Basically, it doesn't deal with action. It starts its analysis with perception of abstract variables in the form of symbols, constructs models that imitate human symbol-handling processes as well as possible, and finishes by generating more strings of symbols that describe actions to be taken. It says nothing useful about how a description of an action, in symbols, gets turned into just those muscle tensions that will in fact produce an action that fits the description. When devices are built that are run by symbol-processing computers, the critical transformations that make action out of symbols are simply put into the device by its builders. Many of those critical parts turn out to be servomechanisms -- control systems.

The assimilation of control theory into the life sciences will require a lot of this kind of reanalysis. Some old ideas will have to go, some will stay. This job is best done by people who are already competent in existing fields. Of course these also have to be people who can see that there is room for improvement along lines other than the standard ones.

In the current membership of the Control Systems Group we have representatives of at least a dozen disciplines of the life sciences, and a few persons representing some unlikely occupations such as piano teaching and law. When these people meet, there is little difficulty in communicating because all of them have a basic understanding of control theory. But communication isn't the only factor that makes these meetings valuable. The most important lesson comes from seeing how control

theory applies in someone else's field.

The biggest problem with introducing control theory to scientists in conventional disciplines is that each scientist tends to think only of the scientific problems that are defined in that one field. The problem in question may involve behavior, but behavior is generally taken on faith to work the way some other specialist says it works. In fact most scientists tend to dismiss details involving other fields, assuming (often quite wrongly) that somebody else understands them well enough. We therefore find some very detailed biochemistry or neurology or personality-testing, all done competently, being used to explain behavioral phenomena that are very poorly analyzed and in many cases don't actually occur. The sociobiologist concludes that behavior patterns are inherited, not knowing that only the consequences of motor outputs, not the outputs themselves, repeat. What does a geneticist really know about the actions through which a bird catches a bug? You can inherit the control systems that are capable of catching bugs, but you can't inherit acts that happen to take you where a particular bug is going next. The combination of narrow expertise in one field and naive conceptions in every other field leads to facile explanations that are right only at one point.

Specialists must see the need for a model of behavior that applies in all disciplines, even those in which the specialist is not competent. Once the Artificial Intelligence researcher understands exactly why organized behavior cannot be produced by computing outputs, he or she will modify the AI model so it will work correctly with more detailed systems actually capable of organized behavior. Important effects of learning how control theory applies in other fields will occur at the boundaries between disciplines -- exactly where we need to work if we are ever to have a unified science of life. At Control Systems Group meetings, specialists from many fields hear other specialists talking about the way control theory has made them rethink the problems in a different field. Because of the common understanding, this inevitably reveals one's own hasty assumptions, and encourages still more rethinking.

One last remark about the CSG. The CSG does not represent any one scientific discipline. It has no agenda of its own beyond encouraging the application of control theory within existing disciplines -- no agenda, that is, except perhaps lowering the barriers between disciplines. The psychologists in the group are still psychologists, the sociologists are still sociologists, the therapists are still therapists, the engineers still engineers. This is not a political movement nor an alternative to established science. It is simply a vehicle for promoting interaction among people interested in using or learning more about control theory in any specialty whatsoever. When all the branches of the life sciences have assimilated and begun using control theory, the CSG, its work accomplished, will have no further reason to exist.

In this presentation I have talked around control theory, alluding to some of its conclusions without attempting to justify or explain them. Learning control theory can't be done by listening to a half-hour's talk. I hope that some of you will

find the promise of a unifying principle for the life sciences appealing enough to go further into this subject.

Date: Fri, 13 Dec 1996 09:49:43 -0800
From: Richard Marken <marken@AEROSPACE.AERO.ORG>
Subject: Critique of Pure Reason

[From Rick Marken (961213.1000)]

Tracy Harms (1996;12,12.14:40) --

>No analysis is derived from observation in contrast with theory and
>theoretical assumptions. Theory always comes first. Observations are only
>possible within a theoretical framework.

I find this a very strange notion. How do we know what to theorize _about_, let alone know what theories are best, without making observations? I agree that theory is important and that it influences not only what we _think_ we observe but also what we _choose_ to observe. But it seems to me that observation is what theory is all about. Theories are just attempts to explain what we experience. If we take the right _approach_ to making observations -- the _scientific approach_, which is aimed at taking action on both the world _and_ the theory and observing how closely the response of the theory matches that of the world -- we will not only be able to improve our theories but also the observations we make to test them. PCT shows how this works in the behavioral sciences. PCT research and theory have combined to show that psychologists have not only been making observations based on the wrong theory (cause-effect) but that they have also been making the wrong observations (disturbance-action relationships rather than controlled perceptual variables).

I think the idea that "theory always comes first" can be quite dangerous because it can lead people to believe that there is something wrong with observation itself. I've noticed that quite a few people on this net seem to express something like disdain for empirical observation and test. Am I getting this right? Are there really people out there who want to go back to Aristotle's and Aquinus' approach to knowledge -- pure reason? What's this about? If there really are people out there who are actually _opposed_ to the idea of testing theory against observation, could you explain _why_.

Bill Powers asked:

>Did you know that raising interest rates either has no effect on inflation,
>or worsens it?

Tracy Harms replied:

>Yes, and my knowledge of that comes from *pure theory*, not from data. If
>the data are consistent with that idea, that indicates a strength where the
>theory prevailed against an empirical challenge.

Again, could you explain which theory predicts this. My wife works at the Federal Reserve Bank and no one over there seems to know about this. You might want to give them a buzz.

Also, how can a theory _prevail against_ an empirical challenge? I thought theories and observations were supposed to be friends, not enemies;-)

Best

Rick

Date: Fri, 13 Dec 1996 13:07:16 -0700
From: "T. B. Harms" <tbh@TESSER.COM>
Subject: theory as dispositional bias

<[From Tracy Harms (1996;12,13.13)]

Rick Marken (961213.1000).

>If there really are people out there who are actually _opposed_ to the
>idea of testing theory against observation, could you explain _why_.

By no means am I opposed: observations are to be used to provide testing of theories, and indeed the strongest, most clever testing we can devise.

You responded to these words of mine:

>>No analysis is derived from observation in contrast with theory and
>>theoretical assumptions. Theory always comes first. Observations are only
>>possible within a theoretical framework.

Rather than attempt to elaborate on this in my own words, I'll here reproduce section 18 of a paper by Karl Popper entitled "Two Faces of Common Sense: An Argument for Commonsense Realism and Against the Commonsense Theory of Knowledge". This paper stands as chapter 2 of _Objective Knowledge_ (1972, 1979, Oxford) The remainder of this message is therefore by Karl Popper; I have omitted most typographic emphasis which occurs in the original.

-- --

All Knowledge is Theory-Impregnated, Including our Observations

Knowledge in its various subjective forms is dispositional and expectational. It consists of dispositions of organisms, and these dispositions are the most important aspect of the organization of an organism. One type of organism can live only in water today, another only on land; since they have survived so far their very ecology determines part of their 'knowledge'. If it were not absurd to make any estimate, I should say that 999 units out of 1,000 of the knowledge of an organism are inherited or inborn, and that one unit only consists of the modifications of this inborn knowledge; and I suggest, in addition, that the plasticity needed for these modifications is also inborn.

>From this follows the fundamental theorem:

> All acquired knowledge, all learning, consists of the modification (possibly the rejection) of some form of knowledge, or disposition, which was there previously; and in the last instance, of inborn dispositions.

To this can be added at once a second theorem:

> All growth of knowledge consists in the improvement of existing knowledge which is changed in the hope of approaching nearer to the truth.

Because all our dispositions are in some sense adjustments to invariant or slowly changing environmental conditions, they can be described as theory-impregnated, assuming a sufficiently wide sense of the term 'theory'. What I have in mind is that there is no observation which is not related to a set of typical situations--regularities--between which it tries to find a decision. And I think we can assert even more: there is no sense in which anticipatory theories are not genetically incorporated. The eye of a cat reacts in distinct ways to a number of typical situations for which there are mechanisms prepared and built into its structure: these correspond to the biologically most important situations between which it has to distinguish. Thus the disposition to distinguish between these situations is built into the sense organ, and with it the theory that these, and only these, are the relevant situations for whose distinction the eye is to be used.

The fact that all our senses are in this way theory-impregnated shows most clearly the radical failure of the bucket theory [of the mind] and with it of all those other theories which attempt to trace our knowledge to our observations, or to the *input* of the organism. On the contrary, what can be absorbed (and reacted to) as relevant input and what is ignored as irrelevant depends completely upon the innate structure (the 'programme') of the organism.

Karl Raimund Popper, 1970

Date: Fri, 13 Dec 1996 15:09:30 -0700
From: Bill Powers <powers_w@FRONTIER.NET>
Subject: Popper's Preposterous Propositions

[From Bill Powers (961213.1430 MST)]

Tracy Harms (1996;12,13.13) --

You quote Popper:

>All Knowledge is Theory-Impregnated, Including our Observations

>Knowledge in its various subjective forms is dispositional and >expectational. It consists of dispositions of organisms, and >these dispositions are the most important aspect of the >organization of an organism. One type of organism can live only >in water today, another only on land; since they have survived so >far their very ecology determines part of their 'knowledge'. If >it were not absurd to make any estimate, I should say that 999 >units out of 1,000 of the knowledge of an organism are >inherited or inborn, and that one unit only consists of the >modifications of this inborn knowledge; and I suggest, in >addition, that the plasticity needed for these modifications is >also inborn.

>From this follows the fundamental theorem:

>From WHAT follows the fundamental theorem? All I have heard here is a string

of pompous assertions given without the slightest reason, factual support, or even logical defense, as if their mere utterance made them true. Why should I accept a single sentence of that introductory paragraph, except for the sole reason that it was said by a famous man? I don't know what he means by "dispositional" or "expectional," and I don't know what either term is supposed to have to do with "knowledge," assuming that the terms have any meaning worth considering to begin with.

The most charitable reading I can give to that paragraph is that it is intended, satirically, to illustrate how empty knowledge can be when it arises from a sea of ill formed ideas and half-understood assumptions, alluded to rather than described, and asserted rather than examined.

The expansion that follows the "theorem" is little better. After reading it I still do not know what a "disposition" is, or whether Popper's broad usage of the term theory is confined to things that have some similarity to each other, or whether any of his factual-sounding statements are in fact believable. Every subject that might be of important is dismissed with a word, as the role of the senses is dismissed by saying it is to make "distinctions." The interactions of organisms and environments are dismissed by calling them "adjustments" to environmental conditions. The very nature of behavior and behavioral organization is wrapped up by saying "what can be absorbed (and reacted to) as relevant input and what is ignored as irrelevant depends completely upon the innate structure (the 'programme') of the organism," which contains at least three major assertions which I deny completely, yet which Popper seems to think reasonable to assert as foregone conclusions.

This mode of argument is more reminiscent of the bulldozer operator than the scientist or scholar, at least as I imagine them to be. If one is "disposed" to admire anything Popper says simply because he says it, then I suppose one can wring some possible meanings out of all this. If not, a more likely reading would be that these are the pronouncements of an old man who simply assumes that anything he says is incontrovertible.

If I ever get to be like that, will someone please shoot me?

Best,

Bill P.

Best,

Bill P.

Date: Fri, 13 Dec 1996 17:51:33 +0000
From: Peter Cariani <peter@EPL.MEEI.HARVARD.EDU>
Subject: Re: Early language learning

[From Peter Cariani ((961213.1700 EST)]
>
> [From Bruce Gregory (961213.1200 EST)]
>
> While driving to work this morning I heard a piece on NPR about

> an experiment on children's learning of language. The
> experimenters invented an artificial language consisting of four
> words of three syllables each. Infants of age eight months were
> exposed to a continuous stream of a computer generated voice
> speaking these words in all permutations for two minutes.
> Following this exposure, the children were able to identify the
> four "words" as different from other combinations of the
> syllables. My reaction was, "Smells like purposeful behavior.
> Wonder what the infants might be controlling for?" (Hint to
> Rick.)
>
> Needless to say, my reaction was different from that of the
> experimenters. They are convinced that they have demonstrated
> the existence of sophisticated statistical analysis being
> performed by the infants to identify the "words". Statisticians
> seem to have no trouble discovering sophisticated statistical
> analyses in the most unlikely places...
>
> Bruce Gregory

I heard the same NPR piece, promptly put my pacifier down,
and started crying......

No, it was an interesting observation, but of course it's only
surprising because of the assumptions that the cognitivists make
about the nature of speech perception. When we hear running speech
there are time patterns corresponding to the acoustic transients
in the speech waveform and their neural correlates can be found
at all levels of the auditory system (i.e. large numbers of neurons
are following the slower modulations in the waveform).
Each one of these syllables has a characteristic
temporal structure (rhythm, onsets and offsets of vowels, etc).
If the basic mechanisms for the analysis of auditory forms is
an analysis of time structure, then it is not surprising that
babies can do it (they also can discriminate between harmonic
and inharmonic sounds, and between tonal and atonal melodies
at very early ages).

Oliver Selfridge once told me that they made sequences of
"bit noise" -- random sequences of 1's and 0's of different
lengths that were repeated over and over. Really short sequences
give rise to faint pitches, longer ones to whirrings
and motorboatings; he said that a listener could identify
random sequences as long as 12 seconds. One listened to the
sound for a while, gradually one senses some rhythm to it and
pretty soon you're nodding your head with the beat.
Yesterday I was playing with some similar repeated random click
patterns, and sequences of these patterns are not difficult
to hear out (1 Hz is easy, 4 Hz takes a while longer, 7 Hz still
longer). No training is needed. A baby could do it (I bet).

So it's not necessarily baffling that we hear these things.
Our auditory system is made to detect recurring time patterns.
A temporal memory trace and a means of cross-correlating the
incoming (neural) time pattern with what's stored is all you
need.

The physiologists know in the back of their minds that
the time patterns are right there in the spike trains, but

they've been indoctrinated into believing that all of this
must be first analyzed by "feature detectors" so even though
lots of evidence is staring them in the face, especially for the
kinds of acoustic transients that are important in speech,
nobody raises the obvious alternative.

These two phenomena taken together are only surprising
if you take a hard top-down view ("first we atomize the
auditory scene into little time-frequency pixels, then a
really, really sophisticated Central Processor figures out
how to group them and make sense of them"). Surprise is
sometimes an artifact of a faulty theoretical assumption.

Have a good holiday, everyone!

Peter Cariani

Date: Fri, 13 Dec 1996 18:16:53 +0000
From: Peter Cariani <peter@EPL.MEEI.HARVARD.EDU>
Subject: Aristotle and Contingent Truths

[From Peter Cariani (961213.1700)]

> Tracy Harms (1996;12,12.14:40) --
>
> >No analysis is derived from observation in contrast with theory and
> >theoretical assumptions. Theory always comes first. Observations are only
> >possible within a theoretical framework.

This is an all-too-common misconception. One doesn't need a theory to make
a measurement, to write down the value of a pointer reading. One does need
a model, however to make a prediction of the value of some other observable
from the pointer reading. One needs a theory if one is going to make more
general explanations or interpretations of what's going on. Observations
can stand by themselves, as long as one describes what one has done to
make them (how the system has been prepared, how the measurement apparatus
was built and calibrated, etc etc). P.W. Bridgeman was always very clear on
these kinds of operational points.

Theories inform which observables (which measuring devices we use) we choose
for our models, but once that choice is made, the outcomes of the measurements
themselves had bloody well be independent of our theoretical framework
(or we are deceiving ourselves).

Thus, observation is possible without a theoretical framework but
pragmatically "meaningful" observations are only possible within
some broader framework (a model or theory or control system).

[From Rick Marken (961213.1000)]
> Are there really people out there who want to go back to
> Aristotle's and Aquinus' approach to knowledge -- pure reason?

Them's fightin' words. I knew Aristotle. Aristotle was my friend. This aint
the Aristotle that I know. You can say anything you want about Aquinas,
but (as he tells me in my sleep) Aristotle always insisted on
the complementarity of the Necessary and the Contingent,
i.e. of analytic truths and of empirical truths. Kant obviously

was an Aristotelian in this sense, and a major disaster of 20th century philosophy has been Quine's muddying of the distinction between logic and observation. The Catholic church really did a number on Aristotle in the middle ages, platonizing him in the process. Don't trust what the Jesuits taught you in parochial school about Aristotle. Plato's your villan.

Peter

Date: Fri, 13 Dec 1996 15:53:07 -0800
From: Rick Marken <marken@LEONARDO.NET>
Subject: Re: Aristotle and Contingent Truths,Gifts

[From Rick Marken (961213.1600)]

Peter Cariani (961213.1700)

> The Catholic church really did a number on Aristotle in the middle
> ages, platonizing him in the process. Don't trust what the Jesuits
> taught you in parochial school about Aristotle. Plato's your villan.

I am really sorry if I sullied Aristitle's good name. Trust me, though, it wasn't the Jesuits who taught me about Aristotle (nu?). As I've said before, I don't know much about philosophy, but from what I do know about Plato it seems likely that he is, indeed, my villain. (The only thing he has in common with my hero is that his name ends in "o").

Bill Powers (961213.1000 MST), Bill Powers (961213.1430 MST) --

Thanks for the gifts.

Best

Rick

Date: Fri, 13 Dec 1996 17:25:54 -0700
From: "T. B. Harms" <tbh@TESSER.COM>
Subject: Re: Popper's Preposterous Propositions

>From Tracy Harms (1996;12,13.17:30)

Bill Powers (961213.1430 MST)

The severity of your rejection of Popper's comments leaves me largely at a loss as to how to respond. Where you see pomposity, I see confidence. Where you see him being dismissive, I see him being succinct. Given your own admission that you do not understand his use of several important words, I am especially surprised that you are ready to judge his work as characterized by "ill formed ideas and half-understood assumptions." And to think that Popper considered his claims incontrovertible flies in contradiction to the most basic features of his philosophy; this is nothing less than calling him a hypocrite of the first degree. Such judgement is, at the least, irrelevant to the formal questions at hand.

Without engaging your reply in specifics at this moment, I would like to

say that the final paragraph of the section I quoted is probably the best short statement I could produce to indicate why PCT was readily comprehensible and persuasive to me. It was Popper who taught me not to think of knowledge as a function of input to the organism, and here he states that very directly. To my eye the compatability between Popper's epistemology and Powers' system science is complete, or at least enormous. Certainly I am not the only person impressed by PCT who is of a similar opinion. As to whether these theoretical systems can be fruitfully compared, constrasted, and mutually engaged, to do so likely requires a thorough understanding of both. You have left me with the impression that you are predisposed against gaining even a rudimentary familiarity with Popper's work. Lack of acquaintence with a system of ideas may be respectable, and harsh evaluation may be respectable, but I don't consider the conjunction to be respectable.

Tracy Bruce Harms tbh@tesser.com
Boulder, Colorado

End of CSGNET Digest - 13 Dec 1996 to 14 Dec 1996
****

Date: Sun, 15 Dec 1996 08:00:06 -0600
Subject: CSGNET Digest - 14 Dec 1996 to 15 Dec 1996

There are 19 messages totalling 891 lines in this issue.

Topics of the day:

1. Early language learning
2. Popper's Preposterous Propositions (16)
3. Aristotle and Contingent Truths (2)

Date: Sat, 14 Dec 1996 07:29:49 -0700
From: Bill Powers <powers_w@FRONTIER.NET>
Subject: Re: Early language learning

[From Bill Powers (961214.0600 MST)]

Peter Cariani ((961213.1700 EST)--

>When we hear running speech
>there are time patterns corresponding to the acoustic transients
>in the speech waveform and their neural correlates can be found
>at all levels of the auditory system (i.e. large numbers of neurons
>are following the slower modulations in the waveform).
>Each one of these syllables has a characteristic
>temporal structure (rhythm, onsets and offsets of vowels, etc).

This is an interesting post, in that it reminds me strongly of the days when I first became aware that my own perceptions were involved in everything I experienced (instead of just seeing the world as given as I always did before then). When you hear speech, you're obviously noticing "characteristic temporal structures," but it's not so obvious that when you look at the neural correlates of speech, you're ALSO noticing

"characteristic temporal structures." It's a particular way of perceiving that is involved in BOTH cases.

The fact that you're talking about human perception becomes more obvious when you think of trying to devise an instrument that will indicate automatically which characteristic temporal structure is present (whether you're hearing sounds or looking at oscillograph displays). Suddenly you're neck-deep in the problem of automatic pattern recognition, which remains essentially unsolved. And you're faced with the problem of saying what it is to recognize something.

If you listen to speech with a trained ear, so you can hear all the rising and falling pitches, the phonemes, the transitions between phonemes, and so on, you're clearly not listening to "temporal structures;" you're listening at one or more lower levels where you can discern the _elements that are structured_. The same goes for looking at records of neural firings, spread out so you can see their details. When you talk of neural "correlates" you're literally looking at "correlated variations" among the elements that make up the pattern. You're saying "this goes up when that goes up; this pauses when that pauses, this is continuous when that is continuous." What you've done, in fact, is to _eliminate_ the temporal structure and reduce it to a point-by-point comparison.

But that isn't what we mean by a temporal structure. Point by point comparisons don't reveal any _pattern_ unless they're being examined by a human being who _does_ perceive something other than the momentary states of the world. That's why we have no "pattern meters" to put on the shelf beside our voltmeters and light meters and frequency meters and VU meters. We have no meters that will show us, as a pointer position on a scale, the degree to which a given pattern is actually present. We have none, that is, except the one that we carry around in our heads.

Now, what is it to "recognize" a pattern? If you can observes "acoustic transients in the speech waveform and their neural correlates," then you're obviously not looking at the pattern, but at its elements. When you're perceiving a pattern, what you observe is the pattern, a unity; you can say that this pattern is present to some degree or in its ideal form, but that's a one-dimensional measure of pattern-ness: more or less present. When you look at the transients and timings and amplitudes, you're looking at the _inputs_ to a pattern-recognizer. When you get simply the sense of a pattern being present to some degree, you're looking at the _output_ of the recognizer. The space between the inputs and outputs contains that device that we can't yet build, but that exists in working order in our brains.

When you look at neural correlates of acoustic patterns, if you can trace the correlations between the sounds and the neural signals you aren't looking at the outputs of the pattern recognizers, but only at the inputs which have not yet reached these recognizers. You won't have found the temporal pattern recognizer until you find signals that are present when the pattern is present, absent when it is absent, and remain _constant_ as long as the same temporal pattern is going on. YOU may observe the same pattern in the sound and in the neural firings, but that's only because you're using your own pattern recognizer. That's why YOU get a sense of the same pattern recurring in both the sound and the neural firings. What you're really looking for is a place in the nervous system where there's a signal that corresponds to your own _sense of pattern_. That is, this signal should be present while you're experiencing a certain pattern-ness in the sound, and absent when you're not. It should most definitely NOT covary with each transient feature of the pattern, either in the sound or in the neural firings.

>So it's not necessarily baffling that we hear these things.
>Our auditory system is made to detect recurring time patterns.
>A temporal memory trace and a means of cross-correlating the
>incoming (neural) time pattern with what's stored is all you
>need.

That sounds easy, but it's not that easy. To do a cross-correlation between two signals, you have to make sure they're synchronized, which means starting the memory signal at the right instant relative to the perceptual signal. So you need at least some means of locking the repetitions together. And you also have to lock the _speed_ of the memory signal to that of the perceptual signal. The word you remember must be running at the same speed as the word you're hearing; otherwise even if you start the memory-word at the same instant the real word starts, they'll be out of synch before the word ends. And of course the words must be spoken and remembered with identical pitches and inflections. When you think about actually building a cross-correlator that will work with continuous speech, the project becomes considerably more complex than it seems when you just describe what it's suppose to accomplish.

Your example of the cross-correlator does have one essential feature of a pattern recognizer: its output is not a pattern, but a number indicating the degree to which a specific pattern is present. As long as the pattern is present to the same degree, the number will remain CONSTANT. If the pattern changes so it is less like the nominal pattern, the number will get smaller.

>The physiologists know in the back of their minds that
>the time patterns are right there in the spike trains, but
>they've been indoctrinated into believing that all of this
>must be first analyzed by "feature detectors" so even though
>lots of evidence is staring them in the face, especially for the
>kinds of acoustic transients that are important in speech,
>nobody raises the obvious alternative.

What _is_ the "obvious" alternative? If it's to say that the pattern is "right there in the neural signals" I believe you're completely mistaken. You're simply applying your own ability to detect patterns to the neural signals. Without that ability, the signals would just be signals.

If you said the word "Mississippi" to a dog, you could probably find neural correlates of that acoustic input in the dog's brain. But the dog would not perceive the word "Mississippi", would it?

Best,

Bill P.

Date: Sat, 14 Dec 1996 07:29:55 -0700
From: Bill Powers <powers_w@FRONTIER.NET>
Subject: Re: Popper's Preposterous Propositions

[From Bill Powers (961214.0630 MST)]

Tracy Harms (1996;12,13.17:30) --

>The severity of your rejection of Popper's comments leaves me largely at a

>loss as to how to respond. Where you see pomposity, I see confidence.
>Where you see him being dismissive, I see him being succinct. Given your
>own admission that you do not understand his use of several important
>words, I am especially surprised that you are ready to judge his work as
>characterized by "ill formed ideas and half-understood assumptions." And
>to think that Popper considered his claims incontrovertible flies in
>contradiction to the most basic features of his philosophy; this is nothing
>less than calling him a hypocrite of the first degree. Such judgement is,
>at the least, irrelevant to the formal questions at hand.

I didn't judge "his work." I judged the materials you quoted. I like his concept of falsifiability, although I have wondered how you could make that principle falsifiable. And even that concept is subject to serious misinterpretation; I've heard it said that statistical generalizations are better than precise predictions, because statistical generalizations are demonstrably wrong in many cases, showing that they are falsifiable.

My problem with philosophical discourse is that as long as the listener is willing to keep saying "Yeah, that sounds reasonable, I'll go along with that," the development flows smoothly. But the moment the listener asks "How do you know that?" the structure collapses. In philosophy, you don't "know" things; you _say_ things, or _assert_ things, or _propose_ things. The idea of looking for independent verification or corroboration of a proposal seldom comes up, except by way of ancedotes which the reader is suppose to know how to interpret. What you read is just supposed to make so much sense to you that it MUST be true. This is a great way to communicate among people who share the same beliefs, understandings, superstitions, and prejudices, but it's not of much use in science. In science, it doesn't matter how obvious or reasonable any (important) proposal is; you still have to demonstrate that it's true, or at least not false, by means that anyone could use. Why should philosophy be exempt from this, to my mind, very reasonable requirement?

>It was Popper who taught me not to
>think of knowledge as a function of input to the organism, and here he
>states that very directly.

Yes, but I still want to know how he knows that. Does he have some secret wellspring of knowledge he isn't telling us about? As far as I can tell, _everything_ we know about the world comes to us by way of sensory receptors; take those away and there is nothing left to know. It's quite clear to me that what we make of those sensory signals, and the signals themselves, are a function of the organization of our perceptual systems, but the inputs to those functions (when not imagined) are independent of how we interpret them. The world of experience is not a CREATION of the nervous system; it's an INTERSECTION of the incoming information with the properties of our perceptual systems. The incoming information is an essential ingredient of knowledge; we can't do without it. If Popper thinks that knowledge is not a function of inputs to the organism -- that we would have the same knowledge no matter what the inputs were -- then in my opinion he is simply confused, or out of touch. And I wouldn't hire him to fix my TV set. He'd replace the same component no matter what's wrong with the set.

Tracy, Popper was just a guy like you and me. He put his pants on one leg at a time (an old saying meaning that he couldn't levitate). The fact that he wrote something down doesn't make it true. Things are not true because of who said them, or merely because they were said.

>You have left me with the impression that

>you are predisposed against gaining even a rudimentary familiarity with
>Popper's work.

Well, I've read some of it, but mostly with the same impression I get when I read other philosophers: long stretches of empty space, marked here and there by statements I can go along with and by other statements the truth or relevance of which I seriously doubt. It seems to be mostly up to me as to what I agree with or disagree with. I've been wrong so often in my life that when I find myself agreeing with a philosopher I think the chances are good that we're BOTH wrong. :(

Best,

Bill P.

Date: Sat, 14 Dec 1996 09:53:54 -0500
From: Bruce Gregory <bgregory@CFA.HARVARD.EDU>
Subject: Re: Popper's Preposterous Propositions

[From Bruce Gregory (961214.1000 EST)]

Bill Powers (961214.0630 MST)

> I didn't judge "his work." I judged the materials you quoted. I like his
> concept of falsifiability, although I have wondered how you could make that
> principle falsifiable. And even that concept is subject to serious
> misinterpretation; I've heard it said that statistical generalizations are
> better than precise predictions, because statistical generalizations are
> demonstrably wrong in many cases, showing that they are falsifiable.

I believe Popper proposed falsifiablity as a principle of demarcation separating the empirical from the non-empirical. Principles are not empirical, hence not falsifiable. I would argue that principles are best judged by their utility. By this criterion, the falsifiability principle does a good job of mapping what we think of as empirical and shows us why some things (including most philosophical discourse) are so difficult to deal with -- no evidence can ever count against them.

Bruce Gregory

Date: Sat, 14 Dec 1996 14:38:12 -0500
From: jf arocha <francisco@MEDCOR.MCGILL.CA>
Subject: Re: Popper's Preposterous Propositions

[From francisco arocha

>My problem with philosophical discourse is that as long as the listener is
>willing to keep saying "Yeah, that sounds reasonable, I'll go along with
>that," the development flows smoothly. But the moment the listener asks "How
>do you know that?" the structure collapses. In philosophy, you don't "know"
>things; you _say_ things, or _assert_ things, or _propose_ things. The idea
>of looking for independent verification or corroboration of a proposal

>seldom comes up, except by way of ancedotes which the reader is suppose to
>know how to interpret. What you read is just supposed to make so much sense
>to you that it MUST be true. This is a great way to communicate among people
>who share the same beliefs, understandings, superstitions, and prejudices,
>but it's not of much use in science. In science, it doesn't matter how
>obvious or reasonable any (important) proposal is; you still have to
>demonstrate that it's true, or at least not false, by means that anyone
>could use. Why should philosophy be exempt from this, to my mind, very
>reasonable requirement?

Date: Sat, 14 Dec 1996 12:50:04 -0700
From: "T. B. Harms" <tbh@TESSER.COM>
Subject: Re: Popper's Preposterous Propositions

*From Tracy Harms 1996;12,14.13

Bill Powers (961214.0630 MST)

>>It was Popper who taught me not to
>>think of knowledge as a function of input to the organism, and here he
>>states that very directly.
>
>Yes, but I still want to know how he knows that. Does he have some secret
>wellspring of knowledge he isn't telling us about?

Hardly; he is foremost among those who have told us about the origination of knowledge. I'm not up for that particular topic of conversation at the moment. The fact that you ask "how does he know that?" indicates to me that you are operating from a very, very different sense of the world than I. I'm not surprised; I mention this simply to indicate why I'm not volunteering to construct an answer which you would find satisfactory.

You have written that "Control theory, by showing us an alternative way of understanding this entire structure, therefore threatens the integrity of practically every bit of knowledge about behavior that has ever been set down on paper." I agree that control theory has this cataclysmic implication. I bring this up to give appropriate weight when I say that I judge there to be even larger consequences to Popper's correctives to understanding knowledge. I'm sure you know the frustrations of attempting conversations with people who have no inkling of the enormity of difference which PCT involves, who always try to treat PCT-oriented statements and claims in terms of their sequence-processing presumptions. I have a similar experience when dealing with matters where I'm talking from a standpoint within comprehensive fallibilism and others are thinking in terms of justification (or, if talking to a Buddhist, detachment). I can no more *answer* "how does he know that?" than you could *answer* "what stimulus would correct the problem behavior of this student?" The question itself must be exposed as wrong. For this to happen the questioner must reconsider much more than they anticipated when raising the question at hand.

>As far as I can tell,
>_everything_ we know about the world comes to us by way of sensory
>receptors; take those away and there is nothing left to know.

I would say: Everything we experience relies on our sensory receptors; take

those away and there would be no experience.

>It's quite clear to me that what we make of those sensory signals,
>and the signals themselves, are a function of the organization of
>our perceptual systems, but the inputs to those functions (when not
>imagined) are independent of how we interpret them. The world of
>experience is not a CREATION of the nervous system; it's an
>INTERSECTION of the incoming information with the properties
>of our perceptual systems. The incoming information is an essential
>ingredient of knowledge; we can't do without it.

He does not dispute its importance, he affirms it: "All ... learning consists of the modification ... of some form of knowledge ...; and in the last instance, of inborn dispositions." The dynamic properties of a living neuron stand as an example of such inborn disposition.

>If Popper thinks that
>knowledge is not a function of inputs to the organism -- that we would have
>the same knowledge no matter what the inputs were -- then in my opinion he
>is simply confused, or out of touch.

When you write 'knowledge' it seems to me to mean something like 'experience.' When I see 'knowledge' used by Popper *in its subjective sense*, I understand it to mean something which may (and should) include 'perceptual control system'.

Now I ask, is it so preposterous, so CONFUSED, to propose that no perceptual control system is structured as a function of inputs to the organism?

>The fact that he [Popper]
>wrote something down doesn't make it true. Things are not true because of
>who said them, or merely because they were said.

I am trying to presume that you did not intend this to be as insulting as it is. This takes quite an effort. To suggest that I agree with Popper on this basis is nothing less than demeaning.

Tracy Bruce Harms tbh@tesser.com
Boulder, Colorado

Date: Sat, 14 Dec 1996 13:26:11 -0700
From: Bill Powers <powers_w@FRONTIER.NET>
Subject: Re: Popper's Preposterous Propositions

[From Bill Powers (961214.1030 MST)]

Bruce Gregory (961214.1000 EST) --

>I believe Popper proposed falsifiablity as a principle of demarcation
>separating the empirical from the non-empirical. Principles are not
>empirical, hence not falsifiable.

Gee, I don't know about that. Doesn't that depend on which principle you're talking about? "What goes up must come down" is certain falsifiable. Even the principle of conservation of angular momentum is falsifiable, in that

there's no _a priori_ reason it should turn out to be conserved. It just is. I think we derive wrong principles from observations at least as often as right ones. Of course some people would like us to think that just because they state something as a principle, it's exempt from empirical test.

I guess I like the criterion of falsifiablity because I think ANY statement ought to be subject to some kind of test -- which it might fail -- before we give it too much weight as a fact of life.

Best,

Bill P.

Date: Sat, 14 Dec 1996 21:13:41 +0000
From: cr4@AXE.HUMBOLDT.EDU
Subject: Re: Aristotle and Contingent Truths

Peter Cariani wrote:

> This is an all-too-common misconception. One doesn't need a theory to make
> a measurement, to write down the value of a pointer reading. One does need
> a model, however to make a prediction of the value of some other observable
> from the pointer reading. One needs a theory if one is going to make more
> general explanations or interpretations of what's going on. Observations
> can stand by themselves, as long as one describes what one has done to
> make them (how the system has been prepared, how the measurement apparatus
> was built and calibrated, etc etc). P.W. Bridgeman was always very clear on
> these kinds of operational points.

Greetings,

This seems to imply that that measurement of events can be made in some absolute way. They ultimately can't, IMO, because an event can only be measured in respect to some other event, never to some absolute position or instant. What are measured are always space intervals and time intervals. This "relativity" of events implies that 4x coordinate systems (x, y, z, t,) must be FIRSTLY CHARACTERIZED by the setting of an origin. (theoretical framework PRE-defined by observer?)

> Theories inform which observables (which measuring devices we use) we choose
> for our models, but once that choice is made, the outcomes of the measurements
> themselves had bloody well be independent of our theoretical framework
> (or we are deceiving ourselves).

See above.

>
> Thus, observation is possible without a theoretical framework but
> pragmatically "meaningful" observations are only possible within
> some broader framework (a model or theory or control system).
>

Any set of physical data takes its complete sense only when it is accompanied by the measurement errors and uncertainties, and more generally the "resolution" characterizing the system under consideration. Any measurement is made with some finite "resolution"

(change in: x, y, z, t). This resolution should be included in the (pre)definition of of said coordinate system. Being itself a length or time interval, it is subjected to the relativity of SCALES.

Treating measurments as absolute (independent of observer reference frame) assumes that space/time is continuous and DIFFERENTIABLE. Feynman demonstrated that, ultimately, this is may not be the case when he identified quantum "particle" trajectories (real and virtual) as continuous but NON-DIFFERENTIABLE. This suggests that such trajectories may encompass INFINITE geodesics (fractal geometry?). As a result, measurements at the quantum scale are rendered statistical probabilities in a (theoretical) framework of absolute Minkowskian space, whereas they are taken in the assumed context of locally Euclidian (linear)/ globally Rhienmannian (curvilinear) at larger scales. Thus scale dependent.

> [From Rick Marken (961213.1000)]
> > Are there really people out there who want to go back to
> > Aristotle's and Aquinus' approach to knowledge -- pure reason?
>
> Them's fightin' words. I knew Aristotle. Aristotle was my friend. This aint
> the Aristotle that I know. You can say anything you want about Aquinas,
> but (as he tells me in my sleep) Aristotle always insisted on
> the complementarity of the Necessary and the Contingent,
> i.e. of analytic truths and of empirical truths. Kant obviously
> was an Aristotelian in this sense, and a major disaster of 20th century
> philosophy has been Quine's muddying of the distinction between logic
> and observation. The Catholic church really did a number on Aristotle in
> the middle ages, platonizing him in the process. Don't trust what the Jesuits
> taught you in parochial school about Aristotle. Plato's your villan.
>

I'm certainly less a fan of the big A. Methinks, ultimately, that the "Non-Aristotlean"'s approach will have more general application. They seem more at home in a "relative" universe.

Regards

Date: Sat, 14 Dec 1996 16:27:35 -0500
From: Bruce Gregory <bgregory@CFA.HARVARD.EDU>
Subject: Re: Popper's Preposterous Propositions

[From Bruce Gregory (961214.1630 EST)]

Bill Powers (961214.1030 MST)]
>
> Bruce Gregory (961214.1000 EST) --
>
> >I believe Popper proposed falsifiablity as a principle of demarcation
> >separating the empirical from the non-empirical. Principles are not
> >empirical, hence not falsifiable.
>
> Gee, I don't know about that. Doesn't that depend on which principle you're
> talking about? "What goes up must come down" is certain falsifiable.

You are certainly correct. If you want to "falsify" a demarcation principle, you only need to demonstrate that it fails to perform its function in a case not otherwise subject to dispute. If you were

to provide an example of an empirical principle that we could all
agree on ("What goes up must come down") and show that it
could _not_ be falsified, you would indeed have shown the limits
of Popper's demarcation principle. Popper, however, never claimed
that the principle was empirical.

Bruce Gregory

Date: Sat, 14 Dec 1996 21:25:59 +0000
From: cr4@AXE.HUMBOLDT.EDU
Subject: Re: Popper's Preposterous Propositions

. You have left me with the impression that
> you are predisposed against gaining even a rudimentary familiarity with
> Popper's work. Lack of acquaintence with a system of ideas may be
> respectable, and harsh evaluation may be respectable, but I don't consider
> the conjunction to be respectable.
>

Well said Tracy. Out of hand, petty dismissal is, unfortunately, often
the response of those who have their epistomoligical toes trodden on by
visionaries. Such is the sound world views imploding.

Cheers

Date: Sat, 14 Dec 1996 16:55:02 -0500
From: Bruce Gregory <bgregory@CFA.HARVARD.EDU>
Subject: Re: Popper's Preposterous Propositions

[From Bruce Gregory (961214.1700 EST)]

Bill Powers (961214.1030 MST)]

> I guess I like the criterion of falsifiablity because I think ANY statement
> ought to be subject to some kind of test -- which it might fail -- before we
> give it too much weight as a fact of life.

Is there a way to test this principle?

Bruce Gregory

Date: Sat, 14 Dec 1996 15:10:49 -0700
From: Bill Powers <powers_w@FRONTIER.NET>
Subject: Re: Popper's Preposterous Propositions

[From Bill Powers (961214.1345 MST)]

Tracy Harms 1996;12,14.13 --

>>It was Popper who taught me not to
>>think of knowledge as a function of input to the organism, and >>here he

states that very directly.

How did Popper teach you not to think that way without your having to use any inputs from your environment? Telepathy?

>When you write 'knowledge' it seems to me to mean something like >'experience.' When I see 'knowledge' used by Popper *in its >subjective sense*, I understand it to mean something which may >(and should) include 'perceptual control system'.

I agree. But I learned about control systems from other people, and from direct experience with such systems. This knowledge wasn't inherent in my brain. I put some things together myself, but I was also building on what I learned from my environment as I understood it. The most illuminating experiences were those involving building and physically interacting with artificial control systems. The way I _thought_ control systems worked had to change when I experienced the way they actually work -- that is, the way my experiences of them depended on what I did to them. The environment didn't tell me what to think, but it did tell me when I thought wrongly about it. And that all came about through sensory inputs (as well as motor outputs).

>Now I ask, is it so preposterous, so CONFUSED, to propose that no >perceptual control system is structured as a function of inputs to >the organism?

Heavens, I wouldn't use such terms. MOI? But I would say it is incorrect to say that. Especially when we are young, I would say that our perceptual control systems are undergoing continuous reorganization, at first nearly from scratch nervous-system-wise, as result of our interactions with our environments -- the way they react on us as we act on them, and they way they act on us without being prompted. Our ideas about how our environments work (and that includes our bodies) arise as we experiment with them and discover their properties, as perceived. And we are also (in line with Campbell's lens-grinding analogy) continually altering our environments to make them fit with our changing perceptions of them. Just look around you: I would wager that you'll have a hard time finding a single object or arrangement of objects that wasn't shaped by some person to make human control more convenient, or even possible (I'm guessing that because of where computer screens are usually found). Yet every one of these objects was also shaped by the properties of the world, which say you can't make matter do everything you can imagine it doing, nor do you have a completely free choice as to what you must do to achieve a given arrangement of your world.

If you're asking whether I think human control systems are structured ONLY as functions of inputs from the environment, of course I would say no. But your comments, after Popper, suggest that he thinks -- thought -- the brain is basically hard-wired, and I don't believe that, either.

>>The fact that he [Popper]
>>wrote something down doesn't make it true. Things are not true >>because of who said them, or merely because they were said.
>
>I am trying to presume that you did not intend this to be as >insulting as it is. This takes quite an effort. To suggest that >I agree with Popper on this basis is nothing less than demeaning.

Sorry.

Does this mean that we can now drop Popper and talk about the ideas he was proposing? Does it really matter that it was Popper who said whatever he said? I know that it doesn't matter that I have said what I've said about PCT; once it's been said, the ideas are out there to be discussed and improved upon, and if I argue in favor of them it's only as anyone on the same side would argue for them, wherever they came from. I'm just that blank line between "Popper" and "Premak" in the author index, and in a lot of ways I'm happy to keep it that way. Ideas and observations are so much more interesting.

So just what are these ideas of Popper's that you find so convincing? Why are they convincing? And don't get all indignant with me for asking; do I strike you as the doormat type, who will accept anything I'm told vigorously or authoritatively enough?

Let it be known, by the way, that I have no use for people who say "I don't have time for such silly questions; go read Powers and come back when you know what you're talking about." So don't tell me to read Popper and come back when I know what I'm talking about. If he said something worth thinking about, you can surely tell me what it is. And why you go along with it.

Best,

Bill P.

P.S. We're going to be in Boulder from late Christmas day through the following Saturday. Any chance we could get together?

P.P.S. I thought my triply alliterative subject heading was pretty good, but you didn't compliment me for it. I'm hurt.

Date: Sat, 14 Dec 1996 15:10:55 -0700
From: Bill Powers <powers_w@FRONTIER.NET>
Subject: Re: Popper's Preposterous Propositions

Francisco Arocha (961214) --

Hi, Francisco --

Here is the entirety of the post you sent:

>>My problem with philosophical discourse is that as long as the listener is
>>willing to keep saying "Yeah, that sounds reasonable, I'll go along with
>>that," the development flows smoothly. But the moment the listener asks
...
>>could use. Why should philosophy be exempt from this, to my mind, very
>>reasonable requirement?

End of post. Did you have something to say about this quote?

Best,

Bill P.

Date: Sat, 14 Dec 1996 16:02:29 -0700

From: Bill Powers <powers_w@FRONTIER.NET>
Subject: Re: Popper's Preposterous Propositions

[From Bill Powers (961214.1515 MST)]

Bruce Gregory (961214.1700 EST) --

>> I guess I like the criterion of falsifiablity because I think ANY
>>statement ought to be subject to some kind of test -- which it might fail
>>-- before we give it too much weight as a fact of life.
>
>Is there a way to test this principle?

Nice one-level-upmanship. All right, how would I test it? I guess I have to say WHY I think this principle is good. I prefer it over others because when a statement passes a test, it seems more reliable than when it either fails or might fail when tested. Since an untested idea might fail, lack of a test leaves us not knowing whether it would fail. And if we then build on an untested idea, all the effort we devoted to following through on its implication will have been wasted if, on any occasion, the failure should be demonstrated.

All these statements are really summaries, alas, of personal experiences. So I guess I can say that I have, indeed, tested the principle, and have found that so far it hasn't failed. My generalization is that it's a good idea to look for tests of assumptions before you use them (unless you're just playing games, in which case being right or wrong doesn't matter).

Best,

Bill P.

Date: Sat, 14 Dec 1996 16:02:33 -0700
From: Bill Powers <powers_w@FRONTIER.NET>
Subject: Re: Popper's Preposterous Propositions

[From Bill Powers (961214.1530 MST)]

Bruce Gregory (961214.1630 EST) --

>>"What goes up must come down" is certain[ly] falsifiable.
>
>You are certainly correct. If you want to "falsify" a demarcation
>principle, you only need to demonstrate that it fails to perform >its
function in a case not otherwise subject to dispute.

...
>Popper never claimed, however, that the principle was empirical.

What's a "demarcation" principle? If Popper's principle is some other kind, what kind is it? I'm not familiar with taxonomies of principles.

Couldn't Popper's principle be falsified by showing that some theory is true only if it's NOT falsifiable? Maybe somebody who knows should state exactly what this principle is, so at least I could be sure what we're talking about. At this point I don't think I know.

Best,

Bill P.

Date: Sat, 14 Dec 1996 16:02:36 -0700
From: Bill Powers <powers_w@FRONTIER.NET>
Subject: Re: Aristotle and Contingent Truths

cr4@AXE.HUMBOLDT.EDU --
(replying to Peter Cariani)

>This seems to imply that that measurement of events can be made in >some absolute way. They ultimately can't, IMO, because an event can >only be measured in respect to some other event, never to some >absolute position or instant. What are measured are always space >intervals and time intervals. (etc)

These are all things you say ABOUT observations; they are not observations in themselves. An observation is "I see an apple on the table" or "That meter reads 27.34." The theoretical framework comes in only when you start making qualifying or interpretive statements ABOUT the observation. Maybe that's a fake apple. Maybe it's floating a little above the table. Maybe it's just a pear that looks like an apple. Maybe its molecules aren't actually touching the molecules in the table. Maybe if I looked again there wouldn't be an apple there every time, or in exactly the same position. Maybe I'm experiencing a holographic projection of an apple.

None of this changes the observation. You are simply adding statements to the original description. Those added statements could be true or false, according to the criteria of truth you use, but the observation can't be either true or false. It just is.

Best,

Bill P.

Date: Sat, 14 Dec 1996 18:13:11 -0500
From: Bruce Gregory <bgregory@CFA.HARVARD.EDU>
Subject: Re: Popper's Preposterous Propositions

[From Bruce Gregory (961214.1815 EST)]

Bill Powers (961214.1530 MST)
>
>
> What's a "demarcation" principle? If Popper's principle is some other kind,
> what kind is it? I'm not familiar with taxonomies of principles.

Popper said that falsifiability divides meaningful statements into two categories: empirical and non-empirical. The statement "God created the universe" is meaningful, but not empirical because no evidence can count against it.

> Couldn't Popper's principle be falsified by showing that some theory is true
> only if it's NOT falsifiable?

Tautological statements are both true and non-falsifiable. They are,
however, not empirical.

> Maybe somebody who knows should state exactly
> what this principle is, so at least I could be sure what we're talking
> about. At this point I don't think I know.

If you want to know if a statement is empirical, ask what evidence
would count against it. If the set of potentially falsifying statements
is not empty, the statement is empirical. My words, not Popper's.

Bruce Gregory

Date: Sat, 14 Dec 1996 18:22:42 -0500
From: Bruce Gregory <bgregory@CFA.HARVARD.EDU>
Subject: Re: Popper's Preposterous Propositions

[From Bruce Gregory (961214.1830 EST)]

I'm not sure anyone pointed out _why_ Popper developed his
criterion of falsifiability. As I recall he was struggling with his
unhappiness with Freudian psychology and realized that
its robustness grew out of the fact that no potential observation
was incompatible with the theory. No matter how someone
behaved, they were demonstrating one Freudian principle or
another. He contrasted this with a physical theory such as
general relativity, which was powerful _because_ there were
so many ways that it could be disproven.

Bruce Gregory

Date: Sat, 14 Dec 1996 17:33:24 +0000
From: Richard marken <marken@AERO.ORG>
Subject: Re: Popper's Preposterous Propositions

[From Rick Marken (961214.1730)]

Bruce Gregory (961214.1830 EST)]

> He [Popper] contrasted this [Freudian theory] with a physical theory
> such as general relativity, which was powerful _because_ there were
> so many ways that it could be disproven.

How in the world did Popper get from this Rather Reasonable
Ratiocination to the Perfectly Preposterous Position that knowledge is
not a function of input to the organism (I see Bill's alliteration and
raise him one);-)

Best

Rick

Date: Sat, 14 Dec 1996 18:59:23 -0700
From: "T. B. Harms" <tbh@TESSER.COM>
Subject: Re: Popper's Preposterous Propositions

from Tracy Harms (1996;12,14.19)

Rick Marken (961214.1730)

>How in the world did Popper get from this Rather Reasonable
>Ratiocination to the Perfectly Preposterous Position that knowledge is
>_not_ a function of input to the organism (I see Bill's alliteration and
>raise him one);-)

I suppose it would be proper form to go sift through the archives, but instead I'll just post an inquiry. What were the fireworks like when it became known that Gary Cziko had dared to tie such apparently scandalous ideas with PCT? For example, on p121 of _Without Miracles_ he writes

> There is no instruction by the environment, no stamping in of stimulus-response connections within the nervous system. Rather, we find a very Darwinian process of selection, not of behaviors, but of closed, negative-feedback loops ...

Maybe I could save time perusing the archives more efficiently if somebody could help direct me to the months in which Gary was tarred, feathered, and crucified for heresy. ;-)

Tracy Bruce Harms tbh@tesser.com
Boulder, Colorado

> "We won't call it a war. We'll call it the important fulfillment of your professional duties."
> Boris Yeltsin, chastizing a general who had reminded him that Chechnyan rebels were still active despite Yeltsin's declaration of victory.

End of CSGNET Digest - 14 Dec 1996 to 15 Dec 1996
****

Date: Mon, 16 Dec 1996 08:00:17 -0600
Subject: CSGNET Digest - 15 Dec 1996 to 16 Dec 1996

There are 21 messages totalling 1540 lines in this issue.

Topics of the day:

1. Theory as dispositional bias
2. Popper's Preposterous Propositions (10)
3. Popper's demarcation
4. Powers's Previous Positively Popperian Proposal & Post
5. In defense of Lamarck (3)
6. Decision, Precedent, Environment and Perception (2)

  7. Vancouver's experiment
  8. simultaneity
  9. System concepts

Date: Sun, 15 Dec 1996 10:26:15 -0600
From: Bruce Abbott <abbott@CVAX.IPFW.INDIANA.EDU>
Subject: Re: Theory as dispositional bias

[From Bruce Abbott (961215.1125 EST)]

>Tracy Harms (1996;12,13.13), quoting Karl Popper:

>Knowledge in its various subjective forms is dispositional and
>expectational. It consists of dispositions of organisms, and these
>dispositions are the most important aspect of the organization of an
>organism. One type of organism can live only in water today, another only
>on land; since they have survived so far their very ecology determines part
>of their 'knowledge'. If it were not absurd to make any estimate, I should
>say that 999 units out of 1,000 of the knowledge of an organism are
>inherited or inborn, and that one unit only consists of the modifications
>of this inborn knowledge; and I suggest, in addition, that the plasticity
>needed for these modifications is also inborn.

This subject has come up on CSGnet before, as an argument concerning to what extent the form and parameters of control systems (both natural and artificial) are "shaped" by the exigencies imposed by the environments in which they must operate. It was suggested that such adaptations represent a kind of "knowledge" of the environment embodied in the structure of the system. In the current debate, I suspect that conflicting definitions of the term "knowledge" are the basis of the discord, with one side restricting the term to mean something like stored sensory input ("experience") and the other allowing the much wider definition in which "knowledge" can be represented in the structural adaptations of the organism, e.g., a bird's wing reflects "knowledge" of aerodynamic principles in the sense that is has the proper features to generate lift and thrust in an atmosphere of the density found near the earth's surface.

I don't find anything especially radical in what Popper had to say above; he does little more than restate Darwin, and I find nothing in that paragraph to disagree with.

Regards,

Bruce

Date: Sun, 15 Dec 1996 12:28:21 +0000
From: Richard marken <marken@AERO.ORG>
Subject: Re: Popper's Preposterous Propositions

[From Rick Marken (961215.1120)]

Tracy Harms (1996;12,14.19) --

> What were the fireworks like when it became known that Gary Cziko
> had dared to tie such apparently scandalous ideas [Popper's notion

> that knowledge is _not_ a function of input] with PCT? For example, > on p121 of _Without Miracles_ he writes

> There is no instruction by the environment, no stamping in of
> stimulus-response connections within the nervous system.

There was considerable wringing of hands and rending of garments but we made it through OK;-)

Actually, what Gary says here seems quite reasonable to me -- and quite different from the claim that "knowledge is _not_ a function of input". All Gary seems to be saying is that the environment is not an active agent, "telling" organisms how to act with respect to it.

Recall that there are a substantial number of life scientists who believe that the environment "shapes" or "controls" behavior. This suggests that the inanimate environment is a purposeful agent, doing whatever it does to organisms in order to get them produce particular behavioral results. It is easy to show that the inanimate environment has no such purpose.

Bruce Abbott has suggested that when conventional psychologists say "environmental control of behavior" they really mean "environmental influence on behavior". This more benevolent interpretation of "environmental control" correctly removes agency from the inanimate environment but it also removes it from the animate environment (organisms) where it belongs; it says that the environment doesn't control behavior but that the environment is the ultimate cause of behavior.

PCT shows that both views of "environmental control" ("control" and "cause") are wrong; the behavior of organisms is neither caused nor controlled by the environment. Rather organisms control the environment.

The title of the basic text on PCT -- Behavior: The control of perception -- was meant to highlight the fact that the PCT view of behavior is exactly the _opposite_ of the conventional view. Where the conventional view is that the environment (as perceived) controls or causes behavior, the PCT view is that behavior controls the environment (as perceived). Unfortunately, the word "perception" in the title seems to have led some people to (mistakenly) conclude that PCT justifies what I would call a "solipsistic" or "Platonic" view of human nature; one that views observation and experience as somehow "tainted" and suspect; one that suggests that people, being condemned to looking through a glass darkly, should seek knowledge through reason rather than the senses.

Perhaps this problem could have been avoided if Bill had called his book "Behavior: The control of the environment". After all, when conventional psychologists say that the environment causes (or controls) behavior they are using a shorthand; they say this instead of "the environment, as perceived by the organism, causes [or controls] behavior". All conventional psychology texts recognize that it is not the environment _outside of the organism_ that is controlling its behavior; it is the environment _as perceived_. That's why there are always chapters on perception and cognition in conventional texts.

Conventional psychologists seem to _always_ use "environment" as the shorthand for "environment as perceived" when talking about the causes

of behavior. I don't think Skinner, for example, ever said "behavior is controlled by perception", though this is clearly what he must have meant (unless he was even dumber than I thought) when he said "behavior is controlled by the environment".

Given the penchant of conventional psychologists to use the word "environment" to mean "perception", perhaps Bill's "answer" to the conventional claim that "behavior is controlled by the environment" should have been "behavior: the control _of_ the enviroment".

Anyway, in this context you can see that Gary's ststement that "there is no instruction [which I take as a synonym for "control"] by the environment" is perfectly consistent with the PCT view of life.

Gary is saying that _instruction_ is not a function of input (the environment as perceived). This is quite different from saying (as Pooper does) that _knowledge_ is not a function of input (the environment as perceived). Instruction is an _active_ process; instruction is done by a purposive agent with the goal of giving knowledge. The (inanimate) environment can't do this -- it can't instruct. But people _are_ active agents; they can instruct themselves and others. When they instruct themselves by doing experiments to test models they are doing precisely what Popper seems to say is impossible -- they are gaining knowledge as a function of input.

Best

Rick

Date: Sun, 15 Dec 1996 12:37:25 -0700
From: Bill Powers <powers_w@FRONTIER.NET>
Subject: Re: Popper's Preposterous Propositions

[From Bill Powers (961215.0945 MST)]

Bruce Gregory (961214.1815 EST) --

Thanks for the clarification, but as usual it may take several iterations before I think I understand.

>Popper said that falsifiability divides meaningful statements into
>two categories: empirical and non-empirical. The statement "God
>created the universe" is meaningful, but not empirical because
>no evidence can count against it.

Why is it that no evidence can count against it? Is it simply that the purported event took place in the past, which we can't revisit? Or is it that the statement is framed so that _even in principle_ there couldn't be any evidence against it? If the latter, I would wonder whether we can call such statements "meaningful" in any sense having to do with experience (as opposed to imagination).

I can answer my own question in part. If I say "The capital letter at the end of this quotation looks to me like an 'A'," I think I am making a meaningful but non-empirical statement, if by "empirical" we mean something that is publicly reproducible or supported by evidence we can share. If I _describe an observation_, the observation is the meaning of the description

of it, yet nobody, not even I, can offer evidence that I have not had the experience I describe. Is that the sort of thing that would qualify as a "meaningful non-empirical" statement?

By the way, you didn't directly answer my question about a "principle of demarcation." What are the characteristics of this kind of principle that distinguish it from other kinds?

So far I can distinguish between decriptions of experiences, which are not falsifiable, and _statements about_ descriptions, which are falsifiable at least in principle. Since "God created the universe" is neither a description of an experience nor a statement about such decriptions, it must belong to some other category. Any ideas?

Best,

Bill P.

>
>> Couldn't Popper's principle be falsified by showing that some theory is true
>> only if it's NOT falsifiable?
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>is not empty, the statement is empirical. My words, not Popper's.
>
>
>Bruce Gregory
>
>

Date: Sun, 15 Dec 1996 12:37:30 -0700
From: Bill Powers <powers_w@FRONTIER.NET>
Subject: Re: Popper's Preposterous Propositions

[From Bill Powers (961215.1020 MST)]

Bruce Gregory (961214.1830 EST)--

>No matter how someone
>behaved, they were demonstrating one Freudian principle or
>another. [Popper] contrasted this with a physical theory such as
>general relativity, which was powerful _because_ there were

>so many ways that it could be disproven.

That's pretty much how I have interpreted the idea of falsifiability, although before I heard of Popper's term I didn't think of it so succinctly. I think of this as the "selling a dead parrot" principle, which you may recall from Monty Python. No matter what evidence John Clees came up with to prove it was dead ("Look, its feet are nailed to the perch!"), Michael Palin had a plausible rejoinder ("It's sleeping. That's just for its safety."). So the theory that the parrot was actually alive became part of the explanation for any supposed deviation from that theory, and the theory was, in effect, unfalsifiable. That may not be a very clean version of the principle, but it's in the same family.

I accused my good friend Bruce Abbott of using this method when he said that children don't all respond alike to operant conditioning in the classroom because they have different "histories of reinforcement." In this sense, reinforcement theory is much like Freudian theory, in that no matter what happens, it fits the theory: given the "history of reinforcement" to fall back upon, no statement based on reinforcement theory is falsifiable.

This is something that _ALL_ theoreticians have to watch out for. Friends of PCT, note well.

Best,

Bill P.

Date: Sun, 15 Dec 1996 12:37:34 -0700
From: Bill Powers <powers_w@FRONTIER.NET>
Subject: Re: Popper's Preposterous Propositions

[From Bill Powers (961215.1045 MST)]

Tracy Harms (1996;12,14.19) --
(replying to Rick Marken)

>What were the fireworks like when it
>became known that Gary Cziko had dared to tie such apparently scandalous
>ideas with PCT? For example, on p121 of _Without Miracles_ he writes
>
> There is no instruction by the environment, no stamping in of
> stimulus-response connections within the nervous system. Rather,
> we find a very Darwinian process of selection, not of behaviors,
> but of closed, negative-feedback loops ...
>
>Maybe I could save time perusing the archives more efficiently if somebody
>could help direct me to the months in which Gary was tarred, feathered, and
>crucified for heresy. ;-)

By "very Darwinian process" Gary actually meant "By a process like Darwinian natural selection but very different from it." (I hope). This usage of "very" actually has an effect opposite to the literal interpretation. Example: suppose a photo-realist artist shows you a painting, and you say "That's lifelike!" The artist would be pleased, because that is the intent. This is quite different from saying "That's _very_ lifelike." When you say it's "very" lifelike you introduce a scale from zero to maximum and place the work somewhere toward the high end of the scale -- but not at the

highest end. The artist would not be pleased. You might as well be saying, "That's very lifelike, I'm sure." The added emphasis, "I'm sure," is intended to weaken, not strengthen, the praise, just as the use of "very" does. To the winner, you say ""Well done." To the second-place runner, you say "Very well done!"

Gary may not accept that way out, but in fact the process he was talking about is different from natural selection, because the organism doesn't have to die in order to change its species' organization. He was speaking of reorganization, which is something that the organism, not the environment, does.

However, our problem here is the role of input from the environment. Popper doesn't seem sure of how to handle it. On the one hand, he says that input can modify existing knowledge, but on the other hand he says that the input goes not give the organism any knowledge -- so "modification" of knowledge doesn't seen to count as new knowledge. He seems to be straddling two extreme positions: one, that the environment provides all knowledge that instructs the organism as to how to behave, and the other, that all knowledge is inherent in the organism and is simply brought out by interactions with the environment (the Socratic position, as apparently espoused by Hans Blom). While he acknowledges that inputs have SOME effect, he isn't willing to say that knowledge depends on them.

The PCT position is that knowledge arises from the interaction of the organism's deepest goals with the properties of the external world, as perceived. What comes out of this interaction is knowledge -- knowledge of the particular world into which you were born, not the archetypic world of evolution. The organism reorganizes itself to make sense of its world, and the outcome of that process is knowledge.

That's what I think.

Best,

Bill P.

Date: Sun, 15 Dec 1996 14:45:00 -0700
From: "T. B. Harms" <tbh@TESSER.COM>
Subject: Re: Popper's Preposterous Propositions

*From Tracy Harms 1996;12,15.14:43

Rick Marken (961215.1120)

>Actually, what Gary says here seems quite reasonable to me -- and quite
>different from the claim that "knowledge is _not_ a function
>of input". All Gary seems to be saying is that the environment is not
>an active agent, "telling" organisms how to act with respect to it.

Again, these claims impress me as identical.

>Recall that there are a substantial number of life scientists who
>believe that the environment "shapes" or "controls" behavior. This
>suggests that the inanimate environment is a purposeful agent, doing
>whatever it does to organisms in order to get them produce particular
>behavioral results. It is easy to show that the inanimate environment

>has no such purpose.

This characterization is (I believe) inadvertant, the consequence of an inept attempt to make the life-sciences more scientific by purging presumptions of intentionality from humans. Given the historical tie between intentionality and notions of extra-natural spirits, this is understandable. The emergence of implied intentionality in the environment was a side-effect of purging it from models of animals, while needing to explain animal behavior.

>PCT shows that [...] the behavior of organisms is neither caused nor
>controlled by the environment. Rather organisms control the environment.

{Polite applause inserted here.}

>Conventional psychologists seem to _always_ use "environment" as the
>shorthand for "environment as perceived" when talking about the causes
>of behavior. I don't think Skinner, for example, ever said "behavior is
>controlled by perception", though this is clearly what he must have
>meant (unless he was even dumber than I thought) when he said "behavior
>is controlled by the environment".

Very interesting, and counterintuitive for non-psych folk like myself. I must similarly admit that the appropriate meaning of 'perception' has been the single hardest aspect of PCT in my own comprehension of it.

>Given the penchant of conventional psychologists to use the word
>"environment" to mean "perception", perhaps Bill's "answer" to the
>conventional claim that "behavior is controlled by the environment"
>should have been "behavior: the control _of_ the enviroment".

I'm eager to hear Bill's own reaction to this suggestion.

>Anyway, in this context you can see that Gary's ststement that "there is
>no instruction [which I take as a synonym for "control"] by the
>environment" is perfectly consistent with the PCT view of life.
>
>Gary is saying that _instruction_ is not a function of input (the
>environment as perceived).

I disagree. I read him as saying that _learning_ is not a function of input.

>This is quite different from saying (as
>Pooper does) that _knowledge_ is not a function of input (the
>environment as perceived).

Popper's primary topic was the growth of knowledge. That is, in a wide sense, learning. Gary Cziko's assertion is exactly in line with Popper's claim: "All acquired knowledge, all learning, consists of the modification [...] in the last instance, of inborn dispositions. [...] The fact that all our senses are in this way theory-impregnated shows most clearly the radical failure of [...] theories which attempt to trace our knowledge to our observations, or to the *input* of the organism."

When Popper talks of attempts to trace our knowledge to our observations he refers to the origination of knowledge. He does not reject the contribution of input (including observation), he simply insists that its contribution is restrained to a *critical* one. That is, it allows error to be culled. But in this neo-Darwinian role input cannot be mapped to

increased fit by anything resembling a *function*.

Tracy Bruce Harms tbh@tesser.com
Boulder, Colorado

"The transmission theory of education is both false and immoral. Education is not, nor could it ever be, a process of transmission. Moreover, when teachers try to convert education into a process of transmission they demean the humanity of their students and they themselves become authoritarian."
` Henry J. Perkinson

Date: Sun, 15 Dec 1996 16:45:16 -0500
From: Bruce Gregory <bgregory@CFA.HARVARD.EDU>
Subject: Re: Popper's Preposterous Propositions

[From Bruce Gregory (961215.1645 EST)]

Bill Powers (961215.0945 MST)

> Bruce Gregory (961214.1815 EST) --

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> purported event took place in the past, which we can't revisit? Or is it
> that the statement is framed so that _even in principle_ there couldn't be
> any evidence against it? If the latter, I would wonder whether we can call
> such statements "meaningful" in any sense having to do with experience (as
> opposed to imagination).

I think the is framed in such a way that those who would affirm it would be unwilling to accept any evidence as counting against it. It is meaningful in the sense of not being meaningless. That is we all have some idea as to what it means.

> By the way, you didn't directly answer my question about a "principle of
> demarcation." What are the characteristics of this kind of principle that
> distinguish it from other kinds?

I'm no expert on Popper (Rick: note spelling ;-)). As I recall, he was addressing interpretations that non-falsifiable statements were "meaningless", by maintaining that falsifiability is a demarcation within the domain of meaningful statements. Popper was trying to distance himself from postivists who want to deny meaning to "non-verifiable" statements. I'm probably guilty of introducing the word "principle" in this regard, rather than Popper. I don't know that he ever characterized principles.

> So far I can distinguish between decriptions of experiences, which are not
> falsifiable, and _statements about_ descriptions, which are falsifiable at
> least in principle. Since "God created the universe" is neither a
> description of an experience nor a statement about such decriptions, it must

> belong to some other category. Any ideas?

Consider the following:

"The PCT position is that knowledge arises from the interaction of the organism's deepest goals with the properties of the external world, as perceived. What comes out of this interaction is knowledge -- knowledge of the particular world into which you were born, not the archetypic world of evolution. The organism reorganizes itself to make sense of its world, and the outcome of that process is knowledge."

I find this paragraph both clear and compelling. Strictly speaking, it is not empirical, although it one day might be. At the moment we cannot subject it to test, but it serves to guide our practice and to suggest explanations. The statement, "God created the universe" is unlikely to ever be empirical, but it probably serves to guide the practices of many more people than does the paragraph I quoted.

Bruce Gregory

Date: Sun, 15 Dec 1996 16:45:50 -0500
From: Bruce Gregory <bgregory@CFA.HARVARD.EDU>
Subject: Re: Popper's Preposterous Propositions

[From Bruce Gregory (961215.1645)]

Bill Powers (961215.1020 MST)]

> Bruce Gregory (961214.1830 EST)--
>
> >No matter how someone
> >behaved, they were demonstrating one Freudian principle or
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> although before I heard of Popper's term I didn't think of it so succinctly.
> I think of this as the "selling a dead parrot" principle, which you may
> recall from Monty Python.

Indeed I do.

> No matter what evidence John Clees came up with to
> prove it was dead ("Look, its feet are nailed to the perch!"), Michael Palin
> had a plausible rejoinder ("It's sleeping. That's just for its safety."). So
> the theory that the parrot was actually alive became part of the explanation
> for any supposed deviation from that theory, and the theory was, in effect,
> unfalsifiable. That may not be a very clean version of the principle, but
> it's in the same family.

I think Popper would agree. I certainly do.

> I accused my good friend Bruce Abbott of using this method when he said that
> children don't all respond alike to operant conditioning in the classroom
> because they have different "histories of reinforcement." In this sense,

> reinforcement theory is much like Freudian theory, in that no matter what
> happens, it fits the theory: given the "history of reinforcement" to fall
> back upon, no statement based on reinforcement theory is falsifiable.

Exactly.

> This is something that _ALL_ theoreticians have to watch out for. Friends of
> PCT, note well.

Indeed. Someone once said that a theory is a license to conduct an
experiment. I've always found it valuable to remind myself of this.
A conjectured PCT explanation of some behavior is the beginning,
not the ending of a search. The TEST provides the experimental
framework that converts the conjecture into something more.

Bruce Gregory

Date: Sun, 15 Dec 1996 15:21:20 -0700
From: "T. B. Harms" <tbh@TESSER.COM>
Subject: Popper's demarcation

*From Tracy Harms 1996;12,15.15:15

Bruce Gregory (961215.1645 EST)

>As I recall, he [Karl Popper] was
>addressing interpretations that non-falsifiable statements were
>"meaningless", by maintaining that falsifiability is a demarcation
>within the domain of meaningful statements. Popper was trying
>to distance himself from postivists who want to deny meaning to
>"non-verifiable" statements.

Just so. Far too often it is mistakenly claimed that Popper was proposing
an alternative theory for demarcating meaningful from meaningless
statements, but as you say he was proposing an alternative to that
demarcation, and indeed one which deals not with statements, but with
propositions.

>I'm probably guilty of introducing the word
>"principle" in this regard, rather than Popper. I don't know that he
>ever characterized principles.

I believe the word which has been most used is 'metaphysical'.

Tracy Bruce Harms tbh@tesser.com
Boulder, Colorado

Tracy Bruce Harms tbh@tesser.com
Boulder, Colorado

"The transmission theory of education is both false and immoral.
Education is not, nor could it ever be, a process of transmission.

Moreover, when teachers try to convert education into a process of transmission they demean the humanity of their students and they themselves become authoritarian."
` Henry J. Perkinson

Date: Sun, 15 Dec 1996 15:44:58 -0700
From: "T. B. Harms" <tbh@TESSER.COM>
Subject: Re: Popper's Preposterous Propositions

*From Tracy Harms 1996;12,15.15:45

Bill Powers (961215.1045 MST)

>The PCT position is that knowledge arises from the interaction of the
>organism's deepest goals with the properties of the external world, as
>perceived. What comes out of this interaction is knowledge -- knowledge of
>the particular world into which you were born, not the archetypic world of
>evolution. The organism reorganizes itself to make sense of its world, and
>the outcome of that process is knowledge.
>
>That's what I think.

An alternative PCT position is that knowledge is comprised of the goals, proto-goals, and meta-goals of an organism, all of which arise from sifting and winnowing by blind variation and selective retention among more-or-less viable units. One consequence of such knowledge is perception, which arises in every case as an interaction of a goal with the properties of the external world, as known. What comes out of this interaction is experience which is true to the organism's knowledge of the particular world into which it was born. The organism reorganizes itself to make sense of its world, and the outcome of that process is original knowledge.

That's the way I'd put it.

Tracy Bruce Harms tbh@tesser.com
Boulder, Colorado

Moreover, when teachers try to convert education into a process of transmission they demean the humanity of their students and they themselves become authoritarian."
` Henry J. Perkinson

Date: Sun, 15 Dec 1996 18:35:54 -0600
From: Gary Cziko <g-cziko@UIUC.EDU>
Subject: Powers's Previous Positively Popperian Proposal & Post

[from Gary Cziko 961216.0030 GMT]

"The idea of reorganization is an essential part of this model, and has been since its beginnings. It was suggested--laid out pretty

completely--by W. Ross Ashby in his notion of "ultrastability," and independently by Donald T. Campbell as "blind variation and selective retention." The basic idea is simple, and older than either Asbhy or Campbell."

Powers, William T. (1989). _Living control systems: Selected papers of William T. Powers_. Gravel Switch, KY: Control Systems Group, Inc. page 287

[Bill Powers (961214.1345 MST)}

>The way I _thought_ control systems worked had
>to change when I experienced the way they actually work -- that is, the way
>my experiences of them depended on what I did to them. The environment
>didn't tell me what to think, but it did tell me when I thought wrongly
>about it. And that all came about through sensory inputs (as well as motor
>outputs).

I think these should make Tracy Harms feel a bit better about his linking of PCT reorganization to Popper.

Note in particular that Bill says . . .

"the environment didn't tell me what to think, but it did tell me when I thought wrongly."

. . . and how this describes a selectionist process very much like biological evolution in which the environment does not in any way "tell" or "instruct" or cause specific mutations to arise, but does have a crucial role in determining which variatons will be reproductively successful. This is in contrast to Lamark's "instructionist" theory by which the environment somehow causes adapted variations to arise.

But, hey, this is all in _Without Miracles_, so I'm not going to do it all again here!--Gary

---

Date: Sun, 15 Dec 1996 20:25:11 -0500
From: Bruce Gregory <bgregory@CFA.HARVARD.EDU>
Subject: In defense of Lamarck

[From Bruce Gregory (961215.2030 EST)]

Gary Cziko 961216.0030 GMT

> This is in contrast to Lamark's "instructionist" theory by which the
> environment somehow causes adapted variations to arise.

Darwin of course did not hold Lamarck in the contempt that is now popular. In natural selection the organism does not reorganize in order to accomplish anything. In a sense, mutations are instructions from the environment to change in a random fashion. This same environment then does the selecting. Organisms seem to play a very passive role -- inheriting the genes that the environment favors or discards. Lamarck, on the other hand, emphasized the goal-oriented nature of organisms as they shaped themselves to best an indifferent environment. Sounds quite consistent with PCT to me...

Bruce Gregory

Date: Sun, 15 Dec 1996 20:45:57 -0600
From: Gary Cziko <g-cziko@UIUC.EDU>
Subject: Re: In defense of Lamarck

[from Gary Cziko 961216.0212 GMT}

responding to Bruce Gregory (961215.2030 EST)

>Darwin of course did not hold Lamarck in the contempt that is
>now popular.

You probably know that Darwin was actually quite Lamarckian, especially toward the end of his life. But that's because he hadn't read Mendel's papers about the particulate nature of genetic inheritance (Darwin was apparently pretty bad at non-English languages as well as math) and was worried about Fleeming Jenkins's arguments about blending inheritance and Lord Kelvin's short estimate on the age of the earth which didn't give enough time for natural selection to work (Kelvin didn't understand that nuclear fusion fueled the sun). But neo-Darwinian's today are for the most part Darwinian and selectionist and not Lamarckian and instructionist.

>In natural selection the organism does not
>reorganize in order to accomplish anything.

Yes, that's the traditional neo-Darwinian view. But Bill Powers's has made some good arguments that organisms may be able to vary their mutation rate in response to intrinsic error. So they would be accomplishing something (some offspring with less intrinsic error), in the same way that PCT reorganization accomplishes control.

>In a sense,
>mutations are instructions from the environment to change
>in a random fashion.

I suppose you could see it this way "in a sense," but I see mutations as products of the organism itself, as are the genetic reshufflings that go in sexual reproduction. Of course, things like radiation and chemical mutatgens can increase the mutation rate, but to me "instructions" imply forming the mutations so that they are more likely advantageous than not, which is not the case.

>This same environment then does the
>selecting. Organisms seem to play a very passive role --
>inheriting the genes that the environment favors or discards.

I disagree with your interpretation. The organism comes up with all the variations, not the environment. The organism is the active agent in evolution. Organisms are not shaped by the environment in the way that pennies are struck at the Denver mint.

>Lamarck, on the other hand, emphasized the goal-oriented
>nature of organisms as they shaped themselves to best
>an indifferent environment. Sounds quite consistent with

>PCT to me...

Except that Lamarck was wrong in his belief that characteristics acquired in this manner could be inherited by the next generation.

There are lots of connections that could be made among Popper, Darwin, evolution, and PCT. But I think the most important is the understanding that a one-way, push-pull concept of causality does not work very well for certain aspects of life, including evolution, behavior, and the acquisition of knowledge. (a) Evolution is not the result of direct environmental causes. (b) Behavior is similarly not at the end of a causal chain beginning with the environment and its effects on the organism. And (c) the acquisition of human knowledge is likewise not the direct result of incoming sensory information (which Popper has criticized as the "bucket theory of knowledge"). These one-way causal theories have been referred to by some (including me) as "instructionist" theories of evolution, behavior, and knowledge. Selectionism sees these processes as involving the spontaneous (not directly caused or instructed by the environment) generation of variations and their differential survival and reproduction (selective retention).

Biologists understand (a) but creationist do not. Perceptual control theorists understand (b) but most behavioral scientists do not. Popper and some other philosophers understand (c) but other philosophers and virtually all educators do not. Very few people understand (a), (b), and (c), but I like to pretend that I do and I'm pretty sure that Tracy Harms does.

Selectively yours,
Gary

Date: Sun, 15 Dec 1996 20:11:19 -0700
From: Bill Powers <powers_w@FRONTIER.NET>
Subject: Re: Popper's Preposterous Propositions

[From Bill Powers (961215.2000 MST)]

Tracy Harms 1996;12,15.15:45 --

>An alternative PCT position is that knowledge is comprised of the goals,
>proto-goals, and meta-goals of an organism, all of which arise from sifting
>and winnowing by blind variation and selective retention among more-or-less
>viable units.

Goals, in PCT, are particular values selected from the range of variation of perceptions. They specify _which value of the perception you want to recreate_. This doesn't fit my concept of knowledge. The form of a perceptual input function would fit better, and perhaps the form of an output function -- if you're talking about implicit knowledge. But I don't use this term "knowledge" very much, so I'm not the one to talk about it.

As to the idea that goals at various levels arise from blind variation and selective retention, I think I'd just oppose that idea. Most goals are adjusted very systematically according to the error in higher-level systems. What would arise from trial and error would be the connections from higher systems to lower ones -- that is, _which_ lower-order goals should be varied as a means of controlling some higher-level perception. Goals are not fixed things in themselves; they are variables.

>One consequence of such knowledge is perception, which
>arises in every case as an interaction of a goal with the properties of the
>external world, as known.

I think we'd better discuss perception. This statement doesn't make much sense to me in terms of PCT.

Best,

Bill P.

Date: Sun, 15 Dec 1996 20:11:11 -0700
From: Bill Powers <powers_w@FRONTIER.NET>
Subject: Re: Popper's Preposterous Propositions

[From Bill Powers (961215.1800 MST)]

Tracy Harms 1996;12,15.14:43 --

>Rick Marken (961215.1120)
>
>>Actually, what Gary says here seems quite reasonable to me -- and quite
>>different from the claim that "knowledge is _not_ a function
>>of input". All Gary seems to be saying is that the environment is not
>>an active agent, "telling" organisms how to act with respect to it.
>
>Again, these claims impress me as identical.

I think that a good deal of our difficulty with Popper arises from the fact that he was most closely associated with the _life_ sciences, where knowledge has been largely statistical in nature (for a number of reasons, including wrong theories). When you transfer his statements about theories to the hard-science or engineering world, they don't make nearly as much sense -- not because there's any thing terribly wrong with them, but because they're simply taken for granted in that world. In physics and engineering, people are not taught to make unfalsifiable statements about the physical world. If you interrupted an engineer doing strength-of-materials tests of a design for a bridge to tell him "God created the universe," he would probably look impatiently at you and say "So?" This piece of unfalsifiable "knowledge," even if true, would be useless with regard to the problem at hand; it simply wouldn't make any difference, one way or the other. I think that's pretty typical of unfalsifiable statements, at least of a certain kind. If a PCTer said "You're really controlling a perception of the strength of that test piece," he would get the same answer: "So?" The engineer already knows that all the evidence he has is in the form of meter readings. And knowing that proposed "fact" wouldn't change what he is doing in the least.

What this engineer is doing is trying to wring some knowledge out of the environment: just how is the applied load, in pounds per square inch of pressure, related to the deflection and ultimate strength of this frame member? This knowledge is missing from his head, and his bridge design can't be completed without it.

Obviously, the frame member didn't just show up in the engineer's office and say "measure me." It doesn't care if the engineer knows how strong it is; it

doesn't care whether it collapses or not. So the frame member isn't actively putting knowledge into the engineer's head. The engineer has to seek out that knowledge, for some purpose of his own. But he can't get that knowledge without interacting with the frame member; the knowledge is there, waiting to be got if he wants it, but it will stay there forever if nobody looks for it.

Furthermore, the knowledge that is there to be got depends to a large degree on how the engineer shaped the frame member, and combined the alloys of which it is made, before testing it. If he made a box girder he will find one deflection factor and ultimate strength; if an I-beam, a different one. He's really testing different ways of _interacting_ with the materials. From all this testing he will come up with generalizations -- not statistical generalizations, but generalizations in the sense of physics and engineering: statements that are true in general, under all conditions, and ALWAYS true when the basic conditions are met. He will come up with formulas that predict how much a beam of _any_ cross-section, made of a given material, will compress, elongate, twist, and bend under ANY load, including no load.

Also, the engineer isn't concerned with "falsifiability." The process of designing a bridge that will support the traffic isn't one of simply eliminating or "culling" wrong theories or designs. Saying that you can only _disprove_ theories would be meaningless to him. The engineer wants POSITIVE EVIDENCE THAT THE BRIDGE WILL BEHAVE AS DESIRED. This means making detailed predictions of the way the bridge members will compress, elongate, twist, and bend under various kinds of applied loads. These predictions are compared, non-destructively, against the actual behavior of a physical model of the bridge under the loads actually applied. And the results are not just pass-fail. If the calculations predict a deflection of 0.0132 inches, and the measured deflection is 0.0139 inches, is the theory "wrong?"

The question to be answered by such tests is not the sort of question that the life sciences have asked, such as "did the load affect the bridge or didn't it?" The theory involved isn't statable as a proposition that is either true or false. The questions can be answered only by stating the relationships of given loads to various measures of the bridge's behavior. The answer will be in the form of a graph, showing, say, the displacement of a frame member as a function of load, from zero load to the maximum design limit (which will be considerably short of the failure limit).

In this milieu, the idea that you can't prove a positive statement makes no sense at all. The idea isn't to "prove statements" but to _analyze relationships_. If you asked the engineer, when his tests were done, what the probability is that the result he obtained could have occurred by chance, he would look at you as if you were crazy. If you insisted, asking "Well, is this bridge going to fail or not?" he'd ask "What, the whole bridge?" Even after the Tacoma Narrows bridge "failed", most of it was still standing. The only part that actually failed was the um, well, the roadway.

Naturally, unpredicted failures do occur, but they don't occur by chance. They occur because of either lack of knowledge, or deliberately ignoring knowledge (often to save money). Isaac Kurtzer described the role of chance in this context nicely: the problem isn't random variation, but epistemological misunderstandings. His point, by the way, was that when you take the mean of variations of that kind, what you get is not average behavior but garbage.

Sorry for getting out the fire hose when all you wanted was a drink of water.

>I must similarly admit that the appropriate meaning of 'perception' has
>been the single hardest aspect of PCT in my own comprehension of it.

I just thought of one way to explain it, which may or may not help. All of experienced reality is Virtual Reality. Actually, maybe that one belongs to Dag Forssell. If that doesn't help maybe you could amplify on your difficulty.

>Given the penchant of conventional psychologists to use the word
>>"environment" to mean "perception", perhaps Bill's "answer" to the
>>conventional claim that "behavior is controlled by the environment"
>>should have been "behavior: the control _of_ the enviroment".
>
>I'm eager to hear Bill's own reaction to this suggestion.

That's sort of a stop-gap way of putting it, if you want to avoid the question of how we know about the environment. You'd get into trouble with this way of saying it the moment two people perceived the same environment differently. Tell me, Rick, do you think my living room is neat and clean? Don't you think that peanut-butter and piccalilli sandwiches are delicious? Can't you see how reinforcing that food is to that rat? Look through this hole and see the giant girl and the miniature man.

The trouble with speaking in what you know to be half-truths is that people are going to remember the half-truths.

Best,

Bill P.

Date: Sun, 15 Dec 1996 22:49:22 +0000
From: Richard marken <marken@AERO.ORG>
Subject: Decision, Precedent, Environment and Perception

[From Rick Marken (961215.2150)]

A belated reply to Stefan Balke (961212.1200 CET) re: applied PCT.
Stefan said:

>I would like to have a PCT-based model about decision making.

There already is one: conflict. Decision making refers to an experience where we consciously try to decide whether we should produce perception A (say, eating an ice cream cone) or perception B (not eating it). We wouldn't be weighing these alternatives if there were no conflict: if getting close to producing perception A didn't create error in a system trying to produce another perception (of ourselves thin, for example). I think decision making is internal chatter driven by error in a control system that has the goal of finding a _rational_ solution to the conflict.

>I don't think that it's all random reorganization

I don’t think so either. Once you are able to see the conflict from the point of view of the systems that are setting the goals that create the conflict, you can solve the conflict non-randomly, by simply changing the conflict producing goals.

Gary Cziko (961216.0030 GMT) --

[various quotes from Bill Powers]

>I think these should make Tracy Harms feel a bit better about his
>linking of PCT reorganization to Popper.

Many people become interested in PCT because of its apparent consistency with the ideas of their current favorite "wise person".
But we've seen that attempts to understand PCT in terms of these ideas typically lead to Procrustean distortions of PCT. It's nice that people such as Popper, Dewey, Skinner, and Simon have led people to PCT. But once people have found PCT, why shouldn't they drop off the "wise men" at the college center while they attend classes on their own?

Me:

>Given the penchant of conventional psychologists to use the word
>"environment" to mean "perception", perhaps Bill's "answer" to the
>conventional claim that "behavior is controlled by the environment"
>should have been "behavior: the control _of_ the enviroment".

Tracy:

>I'm eager to hear Bill's own reaction to this suggestion.

Bill Powers (961215.1800 MST) --

>You'd get into trouble with this way of saying it the moment two
>people perceived the same environment differently.

I know. I was only suggesting the change in title (facetiously, of course) because of problems that the word "perception" often causes for newcomers to PCT. What we mean by "perception" is what most ordinary human beings mean by "the environment" which at the moment for me consists of books, desk, lamp, computer, etc". The fact that the environment I experience is (as you note) a "virtual reality" is, of fundamental importance in understanding how people behave (control). That's why my suggested title revision was _facetious_.

>Tell me, Rick, do you think my living room is neat and clean?

Of course not (as I recall);-) I know this. I made the _facetious_ suggestion that you call you book "Behavior: The control of the environment" if your only goal were to contrast the PCT view of behavior with the conventional view (where the word "environment" is often used as a surrogate for "perception").

>The trouble with speaking in what you know to be half-truths is
>that people are going to remember the half-truths.

OK. So leave the title as is, already;-)

Best

Rick

P.S. I have added a new Java demo to the set available at

http://www.leonardo.net/Marken/demos.html

I have also made some small changes in one or two other demos. The new demo is a replication of the "Levels of Intention" study described in the paper by that title by Marken and Powers and reprinted in _Mind Readings_. It's pretty hard to do but I think its worth it is you can learn to deal with the polarity reversal.

Date: Mon, 16 Dec 1996 02:03:43 -0700
From: Bill Powers <powers_w@FRONTIER.NET>
Subject: Re: Decision, Precedent, Environment and Perception

[From Bill Powers (961216.0145 MST)]

Rick Marken (961215.2150)--

>P.S. I have added a new Java demo to the set available at
>
>http://www.leonardo.net/Marken/demos.html
>
>I have also made some small changes in one or two other demos. The new
>demo is a replication of the "Levels of Intention" study described in
>the paper by that title by Marken and Powers and reprinted in _Mind
>Readings_. It's pretty hard to do but I think its worth it is you can
>learn to deal with the polarity reversal.

You should mention that the Marken in your WWW address is case-sensitive. If you type marken the result is the equivalent of "file not found."

The new demo works very nicely. I notice that the exponential runaway of the model is uniformly about twice as fast as my own. This is probably because of the size of the "dt" that's appropriate for the (slow) repetition rate of the Java program on my machine (100 MHz 486). If others see the same effect, maybe you could compensate in the model program.

It might be nice if you could devise a calibration program -- say, ask the user to press the mouse button twice, counting "PRESS - and - PRESS", while the program iterates. The idea is to try to establish how many iterations occur in one second on the user's machine. Then you could adjust the size of dt accordingly. This wouldn't be exact but it might improve the fit. As I recall, in our previous data the model's runaway lies essentially on top of the real subject's.

It's too bad that Java is so slow. My tracking is a little unstable because of the lag. Any Java experts out there who have a fix for that?

Best,

Bill P.

Date: Mon, 16 Dec 1996 05:38:03 -0500
From: Bruce Gregory <bgregory@CFA.HARVARD.EDU>
Subject: Re: In defense of Lamarck

[From Bruce Gregory (961216.0540 EST)]

Gary Cziko 961216.0212 GMT
>
> responding to Bruce Gregory (961215.2030 EST)
>
> >In natural selection the organism does not
> >reorganize in order to accomplish anything.
>
> Yes, that's the traditional neo-Darwinian view. But Bill Powers's has made
> some good arguments that organisms may be able to vary their mutation rate
> in response to intrinsic error. So they would be accomplishing something
> (some offspring with less intrinsic error), in the same way that PCT
> reorganization accomplishes control.

Cairns's work suggests this does go on.

> >In a sense,
> >mutations are instructions from the environment to change
> >in a random fashion.
>
> I suppose you could see it this way "in a sense," but I see mutations as
> products of the organism itself, as are the genetic reshufflings that go in
> sexual reproduction. Of course, things like radiation and chemical
> mutatgens can increase the mutation rate, but to me "instructions" imply
> forming the mutations so that they are more likely advantageous than not,
> which is not the case.

Cairns's work seems to call this admittedly reasonable conjecture
question.

> >This same environment then does the
> >selecting. Organisms seem to play a very passive role --
> >inheriting the genes that the environment favors or discards.
>
> I disagree with your interpretation. The organism comes up with all the
> variations, not the environment. The organism is the active agent in
> evolution. Organisms are not shaped by the environment in the way that
> pennies are struck at the Denver mint.

I'm not sure that a "traditional" neo-Darwinian such as Williams or
Dawkins would necessarily agree with you. Computer models of neo-
Darwinian evolution also seem to incorporate passive, rather than active,
organisms. At least as I understand them.

> >Lamarck, on the other hand, emphasized the goal-oriented
> >nature of organisms as they shaped themselves to best
> >an indifferent environment. Sounds quite consistent with
> >PCT to me...
>
> Except that Lamarck was wrong in his belief that characteristics acquired
> in this manner could be inherited by the next generation.

I know we believe this quite firmly, especially in the light of the "central
dogma." But I am not convinced that we understand the process well
enough to rule out other mechanisms. Again, Cairns's work is suggestive.
>
> There are lots of connections that could be made among Popper, Darwin,
> evolution, and PCT. But I think the most important is the understanding

> that a one-way, push-pull concept of causality does not work very well for
> certain aspects of life, including evolution, behavior, and the acquisition
> of knowledge. (a) Evolution is not the result of direct environmental
> causes. (b) Behavior is similarly not at the end of a causal chain
> beginning with the environment and its effects on the organism. And (c)
> the acquisition of human knowledge is likewise not the direct result of
> incoming sensory information (which Popper has criticized as the "bucket
> theory of knowledge"). These one-way causal theories have been referred to
> by some (including me) as "instructionist" theories of evolution, behavior,
> and knowledge.

I agree with these points.

> Selectionism sees these processes as involving the
> spontaneous (not directly caused or instructed by the environment)
> generation of variations and their differential survival and reproduction
> (selective retention).

My point is that these processes may be non-random. I always
differed with Popper and his students because theory modification
is distinctly non-random. Special relativity is _not_ the result of random
changes to Newtonian physics with the best variant surviving.

Bruce Gregory

Date: Mon, 16 Dec 1996 12:47:59 +0100
From: "J.A. Blom" <J.A.Blom@ELE.TUE.NL>
Subject: Re: Vancouver's experiment

[Hans Blom, 961216]

Running far behind in my replies...

(Bill Powers (961210.1415 MST))

>>If the mouse gets stuck, the loop gain decreases to zero; no
>>controller, model-based or not, will be able to cope with this
>>situation immediately.

>Not true. The response of a simple control system will be to
>increase the output immediately, which in the case of the stuck
>mouse ball will usually free it up and permit control to continue.

That happens, yes. But sometimes (often?) not.

>The great advantage of the PCT model is that when an unexpected
>disturbance does occur, the system will automatically increase or
>decrease its output to oppose the effects of the disturbance,
>without requiring any prediction that the disturbance is going to
>occur.

In the case that you refer to above, an "unintended side effect" is
that this resolves the problem. It doesn't always; if it doesn't, how
long should one stick to this "method"? When the door hinges are a
bit rusty, you will still open the door with this "unintended"
method, but not when it is locked. In some cases such a strong

response will actually be harmful (to door or muscles). If that can be foreseen, a different solution method, based on a more extended knowledge of cause-effect relationships, would be better. What you present as a solution here is not really one.

>You describe what must be done to make such systems work, without >apparently seeing how impractical these requirements are for any >_general_ model of behavior. You're talking about complex >troubleshooting and cognitive processes that are typical of the way >a whole intelligent human being might approach a problem with a >piece of machinery -- not about the sort of process that would >account for walking on a slippery surface or manipulating a mouse on >a table with slippery spots.

Oh yes, I am. If you can see the spots that are slippery, you can avoid them. If not, you can try to find a way of walking that won't let you slip on those spots. It's not too difficult to learn to walk on ice. It's not too difficult either to learn to walk on a sidewalk that has (sometimes invisible) patches of ice; you only have to assume that there is/could be ice _everywhere_. That may be less easy in case of the mouse, where you may not be allowed to slow down. My point is that it is not necessarily _perceptions_ that govern how we act; _anticipated_ perceptions can be (and are!) used as well.

>What's most troublesome for the model-based approach is predicting >_when_ it will get stuck.

That is right. Models cannot be comprehensive and fully accurate. Surprises cannot be excluded. The world is not fully predictable, and even if it were, modelling it would lead to a far too complex model. What _can_ be included into a model, however, is methods of how to handle particular classes of surprises when one is perceived.

>A more complex model would clearly be required when a simpler one >leaves significant parts of the behavior in question unexplained, or >predicts them incorrectly. If a model-based control system could >explain behavior that a simpler model couldn't explain, it would not >only "fulfill a meaningful role," but would be the only available >explanation. We would have no choice but to use it.

I guess that in any particular situation you might come up with a demonstration that "learning" isn't necessary, because you have built in a mechanism that lets the controller perform equally well. In such cases I would say that _you_ have done the learning for the system -- probably through a lengthy process of trial and error -- and implemented only its end result, either in hardware or software. I'm not interested in that approach because it cannot be generalized. I want to discover good generalizations that can be implemented not in one specific case but much more generally. That seems to be where our interests diverge, as you indicate:

>>Thus far, you have been pretty unclear about when it would serve >>the larger scheme of things (successful control) to collect and >>store knowledge about the world, and what exactly the impact of >>such knowledge might be on the quality of our behavior. That issue >>needs to be resolved, I think.

>I don't really care about that problem because I don't know enough >about higher-level processes in human beings to speculate about it.

>I know this is a big philosophical deal to you, but it isn't to me.

It's not only philosophical. It's extremely practical as well. Given a "system to be controlled" that has only partly predictable behavior a priori, how can we discover -- in the most general sense -- its more specific, individual "behavioral laws" in real time, such that subsequent control can be better.

>> ... How to adjust the pre-compensation TF1? By the usual
>>mechanisms of random trial-and-error and hill-climbing, I
>>guess. For the sake of efficiency preferably the latter.

>That answer is sufficiently vague that it could be applied to any
>model at all. Conspicuously lacking is any attempt to say what sort
>of mechanism would DO this hill-climbing. In the Extended Kalman
>Filter model, at least you supply a specific mechanism to do one
>interesting part of the job. That mechanism will not handle the
>discovery and adjustment of the required form of TF1.

Why not? If the system is (sufficiently) linear, the EKF approach would be able, for instance, to discover the system's impulse (or step) response. Either is a fully general description of the system. An engineer would probably transform a step response into a different (more succinct) model, but that would neither change the acquired "knowledge" nor the controller's behavior.

Greetings,

Hans

Date: Mon, 16 Dec 1996 13:47:44 +0100
From: "J.A. Blom" <J.A.Blom@ELE.TUE.NL>
Subject: Re: simultaneity

[Hans Blom, 961216b]

(Bill Powers (961210.1600 MST))

>>>>>Flipping a light switch is not supposed to be followed instantly
>>>>>by a loud bang. In fact, it is supposed NOT to be followed by
>>>>>anything but a light turning on or off.

>>>>I would call that a mental model: in my experience, flipping a
>>>>light switch does not cause anything but turn a light on or off
>>>>(this may not be true for some electrical engineering students).

>>>You can see that as a mental model, or simply as a perceptual
>>>signal. The perception would be that of a particular sequence:
>>>switch, light. The reference condition is to perceive that
>>>sequence.

>>I think that is stretching the meaning of perception. We're talking
>>here about a process that unfolds over time.

>It is precisely the kind of thing I mean by perception.

Semantic problems again?

>You say that "in your experience" flipping a light switch causes a
>light to turn on and off. Is that not a perception?

I would not call it a perception but (a certain type of; causal)
relationship between an action ("flip the switch") and a perception
("light on/off"). It is not the _perception_ of flipping the switch
that I relate to light on or off but the _action_. This relationship
is so indubitable (normally) that I may even conclude/infer that the
switch has been toggled if I see the light go on or off. I do not
normally have to perceive that the switch was actually toggled. I
reach such a conclusion on the basis of my model, the often perceived
result of toggling that switch.

Is such a model useful? Yes indeed. Is such a model error-free? No,
not at all: other causes of the light going off or on exist as well.
Yet, these may occur so infrequently that I do not reckon with them.

>If it's not, how do you know about it? Are you saying that if two
>events occur a second or two apart, A and B, you can't perceive
>which came before the other? How do you "experience" things, if not
>in the form of perceptions?

In the form of deductions, inductions, relationships, conclusions or
whatever you may want to call them. Internally, I seem to have a
whole bunch of suppositions that link things together, even if one of
those things has not been perceived. Not always nice and clean and
one-to-one. If the light goes out, somebody must have switched it
off. Or a bulb must have burnt out. Or it must be 1 p.m. and the
timer came into action. But I do _not_ entertain causes like "Bill
Powers went to bed" or "a fox on Alpha Centauri's third planet caught
a rabbit".

>But how do you know that the action you call "switch" is taking
>place? Don't you have to perceive it?

No, we normally don't. You can easily be tricked if someone acts as
if he actuates the switch, yet someone/something else actually causes
the light to go out. How often do you carefully monitor whether the
switch was truly activated? I guess that almost universally you rely
on (very) incomplete information to arrive at your conclusion. Yet
your conclusions will very, very frequently be right.

>>If the model were perfect (which it is not and cannot be), the
>>imagined perceptions would be accurately in step with the real-time
>>"perceptions" of the world (whether perceived or not). In that
>>case, it would not matter whether we control on the basis of
>>real-time or imagined perceptions. Since the model is not perfect
>>and the world changes, the model must be recalibrated regularly.
>>This is particularly acute when predictions turn out to be
>>incorrect.

>That's your problem, caused by the model you have adopted. Your
>model creates conditions that have to be met; if they aren't met,
>your model has difficulties. The PCT model is not without
>difficulties of its own, but they are not these difficulties.

That is right. I'm focussing on the problems of learning and how we
come to trust the reliability of certain relationships. In (non-

adaptive) control, you take those for granted because they have been discovered already.

>>>Of course in your model you still have to explain how it is that
>>>the person perceives sequence as a variable that can be correct or
>>>incorrect.

>>An incorrect prediction automatically leads to readjustment of the
>>model. The standard adaptation mechanism takes care of that.

>I have seen nothing to indicate that an Extended Kalman Filter could
>adjust a world model that predicts one sequence of occurrances
>rather than another.

The problem of "choice" plays no role in EKF approaches, because the systems to be modelled are assumed to be "smooth" and (almost) linear. If they are not, EKF approaces are in trouble. They rely on hill-climbing, which will only consider the nearest "top" of the optimization landscape. The only escape that I can think of is to entertain more than one model and/or system state simultaneously and control on the basis of the most "likely" one. But that goes beyond my expertise.

>In fact, your model completely begs the question of how "sequence"
>itself could be perceived.

Incorrect. The existence of an action-perception sequence, for instance, can be inferred from the presence of a difference equation of a form such as

  perception (t) = F (action (t - T))

which tells that a perception at time t is due to an action T seconds earlier.

>>Small prediction errors are normal, and they hardly deteriorate the
>>quality of control.

>Unless they involve one or more successive time integrations, as
>most real behaviors do. Then small prediction errors can cause
>extremely large deviations from reality.

Surprisingly, this is not always the case. Very frequently, the prediction error does not go to infinity as time increases but reaches some upper bound. This is far from intuitive, and it was one of EKF theory's real eye-openers for me. When tracking a satellite in orbit, for instance, we may be fully unable to predict its actual geometric position, yet we may be able to accurately predict the orbit it is in. The same with an electron in orbit around an atomic nucleus. The same, maybe, with the position of your partner (unknown, but certainly within the house/city/country). The same, maybe, with spiral tracking. Etc.

>The world, as you have probably heard, is basically chaotic,

I've heard about it. Should I believe it?

>>Detection of large prediction errors indicates that the
>>model isn't even approximately correct; in such cases, learning --

>>building a new model -- may be forced to restart from scratch.

>That's what would be required. Unfortunately, the model you have
>presented contains no method for starting from scratch.

My demo did. After observing an unanticipated large prediction error, it started learning from scratch, or at least went back to a starting point that was compatible with the new, deviant perception. Several approaches/mechanisms are possible (of which I implemented one), but all will enlarge the model's inaccuracy so much that the new perception can again be "rationally" accounted for. To be more specific: the internally maintained parameter (co)variances are enlarged to values such that the unanticipated perception lies again within the two (or three) standard deviation bounds. As demonstrated, this results in (too?) rapid reacquisition of correct parameter values when a sign inversion, for instance, occurs in the "world".

>You must begin with a model of at least the right general form
>before the Kalman Filter approach can work at all.

Of course. Any method requires that.

>>Since it is often impossible to decide immediately which knowledge
>>is still correct and which not, this is a critical situation which
>>is difficult to handle by simple adaptive techniques.

>Yes, that's true. I advocate a model in which this problem is
>minimized.

And thus cannot be studied.

Greetings,

Hans

Date: Mon, 16 Dec 1996 13:53:56 +0100
From: "J.A. Blom" <J.A.Blom@ELE.TUE.NL>
Subject: Re: System concepts

[Hans Blom, 961216c]

(Rick Marken (961210.2300))

>Let me give you a clue, Hans.

You do. Thank you. I lift this from your post:

> ... I am a PCTer.

> ... very few behavioral scientists have had the courage to leave
>the conventional fold and become PCTers.

>Becoming a PCTer ...

> ... the very few (and very dispersed) PCTers in the world to get
>together ...

>People who are not full tilt PCTers ...

> ... a PCTer like me ...

You _do_ control for very different systems concepts. Let's leave it
at that.

Greetings,

Hans

End of CSGNET Digest - 15 Dec 1996 to 16 Dec 1996
****

Date: Tue, 17 Dec 1996 08:00:21 -0600
Subject: CSGNET Digest - 16 Dec 1996 to 17 Dec 1996

There are 48 messages totalling 3713 lines in this issue.

Topics of the day:

Date: Mon, 16 Dec 1996 16:10:03 +0100
From: "J.A. Blom" <J.A.Blom@ELE.TUE.NL>
Subject: Re: model-based tradeoff

[Hans Blom, 961216d]

(Bill Powers, 961210?)

>>I guess it is these uninformed misunderstandings that I'm tired of.

>Actually. that is also my "uninformed misunderstanding." I had
>understood you to say that in setting up a Kalman filter system one
>of the factors that the analyst has to adjust is the rate of
>convergence of the model, which is done by cut and try rather than
>any strictly analytical method.

That is incorrect. What the analyst (or a higher hierarchical level) has to provide are 1) an estimate of the accuracy of the perception ("measurement noise") and 2) an estimate of the rapidity with which the parameters of the model (as reflecting the "laws of the world") must change ("system noise"). Given these two, the learning mechanism -- and its speed of convergence -- is fully autonomous.

>If the model converges very rapidly, it will be able to keep up with
>rapid changes in the properties of the external world, but in doing
>so it will lose the ability to predict far ahead and thus the
>ability to carry on during protracted losses of input.

That is incorrect. The ability to predict far ahead is lost if the model is told that the "laws of the world" can change rapidly. Even if they don't, the model will assume that they do. This loss of predictive accuracy is realistic, given the presupposition; it is a feature, not a bug.

>On the other hand, if you adjust the model to converge slowly but
>surely, it will be able to continue operating in nearly the same way
>for a considerable time after loss of input, but will be unable to
>keep up with rapid changes in the external world.

It is the other way around. If the model is told that the "laws of the world" do not change, and they indeed do not, it can provide accurate long-term predictions. If the model is told that the "laws of the world" do not change, yet they do, it still will provide long-term predictions, but these will of course be completely incorrect. Finally, if the model is told that the "laws of the world" change rapidly, yet they are constant, it will not provide accurate long-term predictions -- although in principle it could.

>I don't think this problem is peculiar to the Kalman filter
>approach; it's inherent in the nature of prediction.

Prediction is possible only if there are regularities that you can rely on.

>The accuracy of a prediction depends on the length of the sample you
>work with. If you want to predict successfully for a long time
>ahead, you have to use a long sample, and you have to count on the
>characteristics of the world remaining constant during that sampling
>time as well as during the prediction time.

It is the latter, not the former, that is important. The "length of the sample you work with" must be enough to let you estimate the model parameters. Basically, the number of measured variables must be (at least) as large as the number of independent parameters of the model. Equal numbers would allow us to solve N equations in N unknowns. A larger number of observations allows some noise or uncertainty elimination in that it allows a "best" curve fit.

>This means that you have to average over variations that take place
>within the sampling time, and lose the ability to correct for them
>in detail.

This is the problem of changing "world laws" yet an assumption that they do not change.

>I think this tradeoff is a natural aspect of any prediction scheme.
>Either you predict the details very accurately for a short time into
>the future, or you predict the long-term trends accurately at the
>expense of short-term accuracy. I don't believe you can do both at
>the same time.

It is impossible to predict individual noisy observations. It is, however, frequently possible to accurately predict long-term trends, whatever your belief in the contrary.

>In "challeng5" you adjusted the Kalman filter computations to
>correct essentially all the prediction error for the disturbance in
>a single iteration of the program, in effect extrapolating one
>iteration ahead. As a result, when input was cut off the errors of
>prediction started growing very rapidly on each successive
>iteration, and there was essentially no long-term prediction
>capability.

I don't know which simulation you have in mind here. But if you look more closely you will see that, when the input was cut off, the action became pretty regular -- after a transition period. This indicates that the model parameters were pretty stable as well, and that the prediction error did not grow without bound.

>While I can't untangle the complexities of your computations in
>detail, I'm quite sure that the result of this rapid response is to
>convert your "model-based" system into a simple closed-loop feedback
>control system witb a slight amount of first-derivative feedback
>(the one-iteration extrapolation).

Incorrect. The extrapolation (it is exactly that!) is based on much more than just first derivatives. It is based on everything that the model has learned about the regularities that link its actions with the ensuing perceptions of the results of those actions.

>Suppose that the disturbance had been a slow sine-wave, a regular
>waveform with a period of several hundred iterations of the
>program. To take advantage of this regularity, you could now use a
>sine-wave generator in the world-model, adjusted appropriately in
>frequency, phase, and amplitude, to model the disturbance. To
>achieve accuracy it would be necessary to sample over at least one
>full cycle, so as to establish the required frequency and phase.

No. A sine-wave has 3 parameters, so 3 measurements would be the minimum. Three _independent_ measurements, that is; in practice: not too close together, depending upon noise, computational accuracy and the phase of the sinus. Adaptation can be much faster than you suggest, but depends on circumstances. If the (slow) sine-wave disappears when it crosses zero, discovery of its amplitude going to zero will be much slower than when the sine-wave disappears at a maximum excursion of the sine-wave. In the first case, it will take some time before the prediction error starts to deviate from zero; in the latter case, the discovery may be essentially instantaneous.

>However, if the perceptual input were lost, a _simple one-level_ PCT
>model would immediately fail, while the disturbance being simulated
>in the model-based system would continue for some time, perhaps
>hundreds or thousands of iterations, to match and nearly cancel the
>real sine-wave disturbance. The model-based system is the clear

>winner in this case.

Correct.

>But consider what will happen with the perceptual input intact if
>the real sine-wave disturbance suddenly ceases to change and becomes
>constant.

What did we tell the model? That the "laws of the world" are constant? In that case it will not believe that this can happen. That they (in this case: the sine-wave's amplitude) are mostly constant but can change suddenly and unpredictably? In that case it needs perceptions in order to be able to detect when those changes occur. No perceptions, no detection of a new situation that comes about at an unpredictable time, of course. Give it perceptions and it will rapidly readjust -- if the knowledge handed it from the higher level is correct.

>The PCT model, which does not predict, simply adjusts its output as
>usual to cancel the effect of the now-constant disturbance.

If it cannot perceive?

>It continues to contain the same small error. The world-model based
>system, however, continues to produce the same sine-wave modeled
>disturbance, so it now is generating output to cancel a non-existent
>external disturbance.

Only if it cannot discover this. It appears to me that you compare apples and oranges here: a PCT controller that can perceive and a model-based one that cannot.

>Do you consider this analysis to be the result of "uninformed
>misunderstandings"?

Well, conclude for yourself...

Greetings,

Hans

Date: Mon, 16 Dec 1996 08:33:22 -0700
From: "T. B. Harms" <tbh@TESSER.COM>
Subject: Re: In defense of Lamarck

*From Tracy Harms 1996;12,16.07:40

Bruce Gregory (961216.0540 EST)

>I always
>differed with Popper and his students because theory modification
>is distinctly non-random. Special relativity is _not_ the result of random
>changes to Newtonian physics with the best variant surviving.

A thoroughly selectionist model does not require that the changes be random. Systems might be biased to respond to novel situations in

particular, even intricate ways. But what can account for these ways being better than random? The most elegant solution is -- prior selection. (See Cziko's diagram on p309 of _WM_) An alternative which is truly independent of BVSR requires an instruction mechanism which explains accuracy or fit in organic response wholly in terms of the input: it must be inherently true, not just eventually apparently or largely true.

Setting aside the non-random properties of the intellect (which in fact I think are legion), a replacement theory such as special relativity may well be the result of BVSR. The variations and rejections would have occurred in Einstein's mind as he wrestled with how to think of fundamentals of physics, including space and time. Who knows how he shifted the contemplations of mass, energy, etc., who knows how many probings failed in the process? He was not about to articulate a new physical theory without having it stack up in his own mind. Because of the nature of authorship, in effect he could not; a theory must be largely complete in order to be written out. In fact one of the most wonderful things I recognized, in the first hour I really saw how PCT works, was that a major intellectual reference is for theoretical coherence and consistency. Lacking satisfaction of that reference level, ideas keep getting re-examined--and *altered*. An enormous amount of learning is a process of fitting a new theory into an "ecosystem" of old theories. A sound intellect maximizes consistency, a poor one permits mere juxtaposition.

Tracy Bruce Harms tbh@tesser.com
Boulder, Colorado

"The transmission theory of education is both false and immoral. Education is not, nor could it ever be, a process of transmission. Moreover, when teachers try to convert education into a process of transmission they demean the humanity of their students and they themselves become authoritarian."

` Henry J. Perkinson

Date: Mon, 16 Dec 1996 16:41:36 +0100
From: "J.A. Blom" <J.A.Blom@ELE.TUE.NL>
Subject: Re: Solving "loss of input" problem

[Hans Blom, 961216d]

(Bruce Abbott (961211.1720 EST))

>Redundant inputs is an excellent idea if the problem is sensor
>reliability,

In technical systems, regrettably, it is most often the sensors (and the actuators) that fail, _not_ the computational mechanism. So we tend not to favor quasi-solutions that tell us to use more sensors, however "good" those solutions may appear theoretically.

>but it will do you no good at all if the problem is a genuine loss
>of signal (e.g., driving into a fog on the Santa Anna at 70 mph, the
>fact that your right eye has a back-up left eye is of no help).

That is right. Your example is a bit extreme, however. Think of a more gradual one. A fox chases a rabbit and thus controls for zero

distance between fox and rabbit. PCT tells us that, besides this reference, the fox needs to perceive the rabbit and its vector with respect to it. Yet the perception of the rabbit can be lost, partially or completely, for shorter and longer time periods. Do foxes blink their eyes? I would guess so. How about part of the rabbit disappearing behind a small tree? How about the whole rabbit disappearing behind a wider tree or a bush? How about the fox having to jump across a gully which it needs to temporarily perceive with all its attention? In all cases, the rabbit -- or part of it -- is not perceived. Yet the fox is still controlling for chasing the rabbit. I do not care for a fundamental and strict separation between control, where the fox sees (all of?) the rabbit and non-control ("computation of output"), when it does not. I think the difference, and its behavioral consequences, are gradual -- not fundamental, and I also think that there is a quite gradual change from one to the other "strategy". And I finally think, in contrast to PCTers, that pretty much the same mechanism underlies both. Anyway, I see no switching transients.

>It seems to me that a PCT savvy control engineer ... not to
>generate computed _outputs_, but to generate a computed substitute
>_input_ to what otherwise would be an ordinary PCT-style controller.

That is also possible: somehow extrapolate the input rather than the output. But that is, both fundamentally and computationally, much the same thing: as long as the "laws of the world" (the environmental feedback function, in PCTese) do not change, either can be expressed as a function of the other. If the "laws" do change, all bets are off in either case. What we rely on, it seems to me, is that normally the "laws of the world" either do not change (or change in a predictable way, which is the same thing) or change only slowly. If the rabbit is behind a wide bush and invisible to the fox for more than a moment, and if it picks that moment to reverse in it tracks, the fox's "prediction"/control would be suboptimal. But only gradually worse than when it reverses in plain sight.

>The system would also have to generate (or be given) a model of the
>environmental feedback function, so that the expected effect of the
>controller's own actions on the model CV could be computed. To the
>extent that the behavior over the short term of the actual CV can be
>modeled (and in the absence of unexpected serious disturbances to
>that CV during that time), such a system would be capable of
>maintaining at least some degree of control during brief loss of
>input.

>Hans, is this conception different from your model-based controller?

No, it is exactly what I mean.

Greetings,

Hans

Date: Mon, 16 Dec 1996 10:52:02 EST
From: Bruce Gregory <gregory@CF4.HARVARD.EDU>
Subject: Re: In defense of Lamarck

[From Bruce Gregory (961216.1100 EST)]

Tracy Harms 1996;12,16.07:40

> Setting aside the non-random properties of the intellect (which in fact I
> think are legion), a replacement theory such as special relativity may well
> be the result of BVSR. The variations and rejections would have occurred
> in Einstein's mind as he wrestled with how to think of fundamentals of
> physics, including space and time. Who knows how he shifted the
> contemplations of mass, energy, etc., who knows how many probings failed in
> the process? He was not about to articulate a new physical theory without
> having it stack up in his own mind. Because of the nature of authorship,
> in effect he could not; a theory must be largely complete in order to be
> written out. In fact one of the most wonderful things I recognized, in the
> first hour I really saw how PCT works, was that a major intellectual
> reference is for theoretical coherence and consistency. Lacking
> satisfaction of that reference level, ideas keep getting re-examined--and
> *altered*. An enormous amount of learning is a process of fitting a new
> theory into an "ecosystem" of old theories. A sound intellect maximizes
> consistency, a poor one permits mere juxtaposition.

Thanks. An excellent description of learning as I have experienced it.

> "The transmission theory of education is both false and immoral.
> Education is not, nor could it ever be, a process of transmission.
> Moreover, when teachers try to convert education into a process of
> transmission they demean the humanity of their students and they
> themselves become authoritarian."
> ` Henry J. Perkinson

How true. Boy is hard to convince teachers of this. Most of them
see themselves as the product of a "transmission" process. They
become teachers because they want to continue this process...

Bruce Gregory

Date: Mon, 16 Dec 1996 17:17:44 +0100
From: "J.A. Blom" <J.A.Blom@ELE.TUE.NL>
Subject: Re: Solving "loss of input" problem

[Hans Blom, 961216e]

(Bill Powers (961211.1640 MST))

>There are all kinds of possible answers as to how we manage to
>maintain a semblance of control when specific inputs are temporarily
>lost. Hans' way is just one of them, and considering its
>computational complexity and the amount of knowledge about the
>environment and body that are needed to make it work, one of the
>less plausible ones.

It is certainly only one of the _possible_ mechanisms. But that is
not my point; that is what the _PCT_ mechanism is, or how to find
one. I'm not advocating this particular method as _the_ (let alone
the only) way we humans adjust to changes in the environment in
real time, even though the tendency here has been to do just that.

I'm sorry about the black-and-white tendency here to make anyone who presents a different theory, maybe one with useful mechanisms, into an "unloyal" non-PCTer. Sounds familiar to others? :-).

>You have to realized that the basic idea for the Kalman filter was
>published in the 1960s (1963 if I remember right), and that tens of
>thousands of modeler-hours have been put into applying it to the one
>particular architecture that Hans has described. When I looked up
>references for the Kalman filter with Altavista, I got 20,000 hits.

That indicates that this is a very active area of research, which has already found useful applications (that's why...).

>And of course none of these papers was concerned with how to apply
>similar principles to the adaptation of other kinds of control
>architectures like HPCT.

HPCT, regrettably, neither has the same theoretical nor the same practical breadth, it seems. Why so much advance around Kalman's theory (since 1963) and so little around HPCT (since 1973)?

>Here's my point. If the same amount of labor had been put into
>applying these adaptive methods to a PCT model, it would be much
>easier to put aside the distraction of "adaptiveness" and simply
>look at the basic organization being proposed.

I'm sorry, Bill, but "adaptiveness" won't go away.

>The "modern control theory" approach is basically an attempt to
>conform to a top-down, command-driven type of system in which the
>point is to compute the right output.

(H)PCT models _also_ need to compute the right output. You may object against the word "compute" because the underlying PCT mechanism is so simple, but what's the fundamental difference?

>That basic concept absolutely REQUIRES an internal world-model, and
>also the ability to compute the INVERSE of the same world-model.

Upon closer inspection, you will find the same in (H)PCT models. I'm sorry that you cannot see the correspondences between what is internal in "models" and the PCT notions of gain, slowing factor and such.

>A modular hierarchical negative feedback architecture offers all
>kind of alternatives to the "modern control theory" approach.

That is a belief which remains to be demonstrated. One of the current criteria of a "successful" approach is what it can demonstrate in the real world, i.e. in real, useful control systems. We are currently much more aware of the difference between a real system (such as a physically existing robot) and its simulation. The latter is based on a usually crude model which may be pretty different from the real thing, amongst other things due to the accumulation of small systematic errors over time due to stick-slip, friction, temperature coefficients, etc. I guess that we now live in a time where you have to demonstrate actual rather than simulated behavior in order to be accepted as a scientist. Otherwise, the theory may be considered to be elegant but needy of "demonstration". The Test, where the rubber

meets the road ;-).

>Give us a world in which you will get 20,000 hits in looking up
>"control of perception" and the status of the PCT model will be
>greatly different.

Another belief. When you get to that point, there may be something in it. Start to demonstrate a useful, real (not simulated) HPCT-based system and the number of articles will soar, I assure you.

>The well-developed approaches have all the advantages of incumbency,
>which as we know have nothing to do with merit.

Are you serious? Time-proven methods have no merit?

Sorry to be so "disloyal" but I sometimes get the idea that you need to get your feet back on the floor.

Greetings,

Hans

Date: Mon, 16 Dec 1996 11:09:14 -0500
From: Martin Taylor <mmt@HERMES.DCIEM.DND.CA>
Subject: Re: Auditory temporal structure

[Martin Taylor 961216 10:40]
>Bill Powers (961214.0600 MST)
>
> Peter Cariani ((961213.1700 EST)--
>
> >When we hear running speech
> >there are time patterns corresponding to the acoustic transients
> >in the speech waveform and their neural correlates can be found
> >at all levels of the auditory system (i.e. large numbers of neurons
> >are following the slower modulations in the waveform).
> >Each one of these syllables has a characteristic
> >temporal structure (rhythm, onsets and offsets of vowels, etc).
>
> This is an interesting post, in that it reminds me strongly of the days when
> I first became aware that my own perceptions were involved in everything I
> experienced (instead of just seeing the world as given as I always did
> before then). When you hear speech, you're obviously noticing
> "characteristic temporal structures," but it's not so obvious that when you
> look at the neural correlates of speech, you're ALSO noticing
> "characteristic temporal structures." It's a particular way of perceiving
> that is involved in BOTH cases.
> ...
> If you listen to speech with a trained ear, so you can hear all the rising
> and falling pitches, the phonemes, the transitions between phonemes, and so
> on, you're clearly not listening to "temporal structures;" you're listening
> at one or more lower levels where you can discern the _elements that are
> structured_.

I'd say "higher levels", not "lower levels." Once you get to pitch, and even more so when you get to phonemes and phoneme transitions, you are well beyond the level about which Peter is talking. He's talking about

a phenomenon that has been extensively studied by Roy Patterson and
his colleagues at the MRC Applied Psychology Unit in Cambridge.

I've listened to Roy's acoustic material a little. The waveforms are made
as white noise (Gaussian independent samples), repeating at a period T.
If T is very long, you just hear white noise. If it's shorter than some
threshold value, you hear patterns, such as "bump squeak whine bump squeak
whine bump squeak whine...". As T gets shorter, you hear fewer of these
individuated sounds, and when T gets very short, they merge into a single
pitched continuous sound. It's tempting to suggest that the duration of
one of these individuated sounds is in the range of durations of phonemes
or phones, but I don't know the work well enough to be able to say it is so.

> The same goes for looking at records of neural firings, spread
> out so you can see their details. When you talk of neural "correlates"
> you're literally looking at "correlated variations" among the elements that
> make up the pattern. You're saying "this goes up when that goes up; this
> pauses when that pauses, this is continuous when that is continuous." What
> you've done, in fact, is to _eliminate_ the temporal structure and reduce it
> to a point-by-point comparison.
>
> But that isn't what we mean by a temporal structure.

No. Nor, I think, is it what Peter means.

> ...
> When you look at neural correlates of acoustic patterns, if you can trace
> the correlations between the sounds and the neural signals you aren't
> looking at the outputs of the pattern recognizers, but only at the inputs
> which have not yet reached these recognizers. You won't have found the
> temporal pattern recognizer until you find signals that are present when the
> pattern is present, absent when it is absent, and remain _constant_ as long
> as the same temporal pattern is going on.

These are the characteristics of Patterson's "Auditory Stabilized Image",
his model of the neural structure that recognizes patterns. If he is right,
then there is a place, quite early in the auditory system, where the
signal depends precisely on the temporal patterning _as a human perceives
it to be_. His model is computational, and the "ASI" can be displayed on
a screen. He claims that all variations that a human can detect in an
auditory pattern can be seen as variations in the screen display, and
all variations that can be seen in the screen display can be heard as
variations in the auditory pattern. Same criterion as is used for testing
simulation control models against human performance.

> What you're really
> looking for is a place in the nervous system where there's a signal that
> corresponds to your own _sense of pattern_. That is, this signal should be
> present while you're experiencing a certain pattern-ness in the sound, and
> absent when you're not. It should most definitely NOT covary with each
> transient feature of the pattern, either in the sound or in the neural firings.

Right. That's what the ASI is claimed to do.
>
> >So it's not necessarily baffling that we hear these things.
> >Our auditory system is made to detect recurring time patterns.
> >A temporal memory trace and a means of cross-correlating the
> >incoming (neural) time pattern with what's stored is all you
> >need.

>
> That sounds easy, but it's not that easy.

No. If it had been easy, other people would have done it long ago.

> The word you remember must be running at the same speed
> as the word you're hearing; otherwise even if you start the memory-word at
> the same instant the real word starts, they'll be out of synch before the
> word ends. And of course the words must be spoken and remembered with
> identical pitches and inflections. When you think about actually building a
> cross-correlator that will work with continuous speech, the project becomes
> considerably more complex than it seems when you just describe what it's
> suppose to accomplish.

This is quite a different issue. To bring this in is like saying that
a sequence perception has to be done at the transition level. Exactly
the same issues arise. The recognition of a word has very little to do
with the pitch or inflection or speed (something to do with each, but
not much) of the sound pattern. Most computerized recognizers start
with some analysis of the spectrum and then go with the sequence of
spectral patterns identified (perhaps tentatively). Patterson's ASI
might be a better place to start, but it's computationally very expensive.
Easy to do in a neural system, hard in a computer (as is correlation).

>
> If you said the word "Mississippi" to a dog, you could probably find neural
> correlates of that acoustic input in the dog's brain. But the dog would not
> perceive the word "Mississippi", would it?
>
Now you have to ask yourself what it would mean if you were to answer
that question either "yes" or "no."

Incidentally, if you want to play with Patterson's ASI, it used to be
available to interested researchers. This was some years ago, but
a Web search for the UK Medical Research Council Applied Psychology Unit
might well show up something (I haven't tried it, so I'm guessing). When
I was concerned about such things, the Patterson group was trying hard
to make it available on many different computing platforms.

Martin

Date: Mon, 16 Dec 1996 17:40:13 +0100
From: "J.A. Blom" <J.A.Blom@ELE.TUE.NL>
Subject: Re: Popper's Preposterous Propositions

[Hans Blom, 961216f]

(Bill Powers (961215.1045 MST))

>However, our problem here is the role of input from the environment.
>Popper doesn't seem sure of how to handle it. On the one hand, he
>says that input can modify existing knowledge, but on the other hand
>he says that the input goes not give the organism any knowledge --
>so "modification" of knowledge doesn't seen to count as new
>knowledge.

Both positions are valid, I think. Even if we acquire knowledge, the

mechanism to do so must be built-in.

Euclidian geometry -- to use an old example -- is based on just five intuitively obvious axioms, the most difficult one stating that two parallel lines will never touch or cross. That is the "knowledge" of Euclidean geometry: an infinite number of axioms can be derived from those few axioms, but they are all of course tautological with respect to the axioms. Are these theorems "new" knowledge? There are two positions:

No, they're just tautologies.

Yes, much effort has been expended in discovering them, and those discoveries are new: we did not have them before.

Choose and pick. I think both are true. The same for the "knowledge" of organisms: the "axioms" (basic mechanisms) are built-in, but their consequences (the theorems) need to be rediscovered by every new organism individually.

>The PCT position is that knowledge arises from the interaction of
>the organism's deepest goals with the properties of the external
>world, as perceived. What comes out of this interaction is knowledge
>-- knowledge of the particular world into which you were born, not
>the archetypic world of evolution. The organism reorganizes itself
>to make sense of its world, and the outcome of that process is
>knowledge.

Well said, Bill. I agree completely.

Greetings,

Hans

Date: Mon, 16 Dec 1996 16:45:54 +0100
From: Stefan Balke <sbalke@HRZ.UNI-BIELEFELD.DE>
Subject: Re: Decision

>From Stefan Balke (961216.1640 CET)

Rick Marken (961215.2150) --

Me:

>>I would like to have a PCT-based model about decision making.

Rick:

>There already is one: conflict. Decision making refers to an experience
>where we consciously try to decide whether we should produce perception
>A (say, eating an ice cream cone) or perception B (not eating it). We
>wouldn't be weighing these alternatives if there were no conflict: if
>getting close to producing perception A didn't create error in a system
>trying to produce another perception (of ourselves thin, for example). I
>think decision making is internal chatter driven by error in a control
>system that has the goal of finding a _rational_ solution to the
>conflict.

Me:

>>I don't think that it's all random reorganization

Rick:

>I don't think so either. Once you are able to see the conflict from the
>point of view of the systems that are setting the goals that create the
>conflict, you can solve the conflict non-randomly, by simply changing
>the conflict producing goals.

What is a _rational_ solution to the conflict. Do you think that something like experience and observation based probability estimations of the outcomes of the conflicting goal are involved. If I assume something like going up a level and simply changing the conflict producing goals the general decision problem remains, I still have to decide about the priorities of the involved goals.

Best, Stefan

Date: Mon, 16 Dec 1996 17:50:59 +0100
From: "J.A. Blom" <J.A.Blom@ELE.TUE.NL>
Subject: single top-level goal?

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begin 644 CONFLIC2.ART
M_U=00VL"```!"@`!`````/O_!0`R``4!```)``(```!"````!P`1````1```
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M>`#/&;X0I@@````0(%#L`$4S-Q+[`0"Y*U@"4",``=$``-`!#`"P!+`$```")
M!0P``=#0!@8``0`&``;0T`$,````B04``)T$#``!T%M(86YS($)L;VTL(#DV
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M(&%L=V%Y<R`B;VYE#75P<&5R;6]S="!G;V%L(B!T:&%T(&ES(&)E:6YG('!U
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M('=A<R!A;')E861Y(&QA:60@:6X@;7D@<&]S=`U;2&%N<R!";&]M+"`Y,C$P
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M(*FI(&%N9"!L:71T;&4@<F5S:7-T86YC92X@5V4@;VYL>2!N965D('1O(")I
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M+B!-87)T:6XL('EO=2!A<F4@<FEG:'0A(%1H97)E(%]A<F5?('!R;V)L96US
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M;F4-<&]S97,@82!R9:QQ=65S="!T:&%T(&ES(&EN8V]M<&%T:6)L92!W:71H
M(%E/55(@3U=.(&1E<VER97,N($QE="=S(&%N8:UL>7IE('1H:7,@<VET=6%T
M:6]N+@H*5&%K92!A('9E<GD@<VEM<&QE(&5X86UP;&4N(%EO=2!A<F4@9')I
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M:79E(&]N92!Y87)D('1O('1H92!,1494(&]F('1H92!M:61D;&4L('1H92!O
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M9&5V:6%TK&EO;G,L#7QX+7(Q?"!A;F0@?'BI<C)\+"!T:&%T(&]U9VAT('1O
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M870@=V4@871T86-H('1O('1H90UM86=N:71U9&4@;V8@=&AE(')E<W5L=&EN
M9R!E<G)O<G,@;VYC92!W92!S96QE8W0@82!C;W5R<V4Z(&ES(&%N(&5R<F]R
M#6]F(&]N92!Y87)D('1H<F5E('1I;65S(&UO<F4@<V5V97)E('1H86X@86X@
M97)R;W(@;V8@;VYE(&9O;W0_($UO<F4-<V5V97)E/R!,97-S/R!4;R!S:6UP
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M<VEM:6QA<FQY+@H*5&AE('!R;V)L96T@8V%N(&)E(&9O<FUU;&%T960@87,@
M82!M:6YI;6EZ871I;VX@<')O8FQE;3H*"L$"6`)8`@4`P5$@*'@I(#T@1B`H
M?'@M<C%*2`K($8@*'QXJ7(R?"D*"G=H97)E(%$@;75S="!B92!M:6YI;6EZ
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M9G5N8W1I;VX@=&AA="!P<F]V:61E<R!T:&4@=V5I9VAT<R!O9B!T:&4@97)R
M;W)S+B!,970@=7,@;6%K92!A(&=R87!H(&]FU``<```6B"P!`````````"````
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M(&1O97,@;F]T(&%L;&]W(&YI8V4@9W)A<&AS+"!O;FQY('%U86QIK'1A=&EV
M92!P;&]T<RX@26X@=&AE('!L;W1S#6)E;&]W($D@:&%V92!C:&]S96X@1B!A
M<R!A('-Q=6%R92P@82!U;FET(&9U;F-T:6]N(&%N9"!A('-Q=6%R92!R;V]T
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M<C(I"@IU;FET.B`@("`@("`@42`H>"D@/2!\>"UR,7P@*R!\>*ER,GP*"G-Q
M=6%R92!R;V]T.B!1("AX*2`]('-Q<G0@?'@M<C%\("L@<W%R="!\>*ER,GP*
M"B`@("`@7"`@("`@("`@+R`@("`@(%P@("`@("`@("`@("`O("`@("`@
M7"`@("`@("`@("`@("`@+PH@42`@("!<("`@("`@+R`@("`@("`@7"`@("`@
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M;W)E('-E=F5R96QY('1H86X@<VUA;&P-97)R;W)S.R!I="!A<G)I=F5S(&%T
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M455!3"!M:6YI;6$N($EN('-U8V@@80UC87-E('EO=2!M:6=H="P@:68@>6]U
M(&%R92!N;W0@8V%R969U;"!I;B!S;VQV:6YG('EO=7(@;6EN:6UI>F%T:6]N
M#7!R;V)L96TL(&=E="!S='5C:R!I;B!A(&QO8V%L(&UI;FEM=6TN"@I(;W<@
M9&\@<&5O<&QE(')E8V]N8VEL92!I;F-O;7!A=&EB;&4@9V]A;',_($ET('-E
M96US('1H870@9&EF9F5R96YT#7-O;'5T:6]N<R!A<F4@8VAO<V5N(&EN(&1I
M9F9E<F5N="!C:7)C=6US=&%N8V5S+B!3;VUE=&EM97,@:70@:7,-<&]S<VEB
M;&4@=&\@<F5A8V@@82!W96QLJ61E9FEN960@8V]M<')O;6ES92X@26X@;W1H
M97(@8V%S97,@=V4@;V)S97)V90UT:&%T('=E(&AA=F4@;&]T<R!O9B!F<F5E
M9&]M(&QE9G0@9F]R(&]U<B!A8W1I;VYS+B!!;F0@<V]M971I;65S('=E(&UU
M<W0-<F5J96-T(&]N92!G;V%L(&EN(&9A=F]R(&]F(')E86-H:6YG(&%N;W1H
M97(@;VYE(&5X86-T;'DN"@I(;W<@9&]E<R!T:&ES('!R;V)L96T@9V5N97)A
M;&EZ93\@5V5L;"P@=7-U86QL>2!W92!H879E(&$@9W)E870@;6%N>0UD:69F
M97)E;G0@<VEM=6QT86YE;W5S+"!B=70@8V]N9FQI8W1I;F<L(&=O86QS+B!9
M970@=&AE(%194$4@;V8@=&AE#7-O;'5T:6]N('-P86-E(&1O97,@;F]T(&-H
M86YG93L@:70@:7,@:G5S="!O9B!A(&AI9VAE<B!D:6UE;G-I;VXN"@I7:&%T
M(&ES('1H92!L97-S;VX@;V8@=&AI<R!E>&5R8VES93\@5V5L;"P@:70@;75S
M="!B92!C;&5A<B!B>2!N;W<Z#6-O;F9L:6-T(*FI(&EN('1H92!S96YS92!O
M9B!H879I;F<@=&\@8VAO;W-E(&)E='=E96X@9&EF9F5R96YT(&UI;FEM82!O
M9@U1(*FI(&-A;B!A<FES92!O;FQY(&EF('EO=2!D:7-C;W5N="!L87)G92!E
M<G)O<G,@9F]R(&]N92!G;V%L(&)Y#69O8W5S<VEN9R!O;B!T:&4@;W1H97(@
M;VYE(&]N;'DL(&]R(&EN('!R969E<F5N8V4N($EF('EO=2!S=6-C965D(&EN
M#6-H;V]S:6YG(&$@:VEN9"!O9B!B96AA=FEO<B!W:&5R92!Y;W4@=&%K92!L
M87)G92!E<G)O<G,@=F5R>2!S97)I;W5S;'D-8G5T('-M86QL(&5R<F]R<R!L
M:6=H=&QY+"!Y;W4@=VEL;"!A;'=A>7,@87)R:79E(&%T(&$@=6YI<75E('-O
M;'5T:6]N#71H870@<V%V97,@>6]U('1H92!T<F]U8FQE(&]F(&AA=FEN9R!T
M;R!D96-I9&4@=VAI8V@@;V8@<V5V97)A;"!M:6YI;6$-:7,@=&AE('-M86QL
M97-T+B!);B!F86-T+"!T:&4@;6]R92!S97)I;W5S;'D@>6]U('1A:V4@;&%R
M9V4@97)R;W)S(&%N9`UT:&4@;6]R92!Y;W4@8V%N(&1I<W)E9V%R9"!S;6%L
M;"!O;F5S+"!T:&4@;6]R92`B<F]B=7-T(B!Y;W5R('-O;'5T:6]N#6)E8V]M
M97,N(%1H:7,@97AE<F-I<V4@:7,@;&5F="!F;W(@=&AE(')E861E<CH@<&QO
```

```
M="!T:&4@9W)A<&@@;V;4`!P``!:(+`(```````(```!P+7`M<"VP!+`$'```
MU(R9("`@("`@("`@("`@("`@("`@("!.("`@("`@("`@3@H@("`@("!1("AX
M*2`](`QX+7(Q?"`@*R!\>*ER,GP*"F9O<B!.(#T@-"P@-BP@."P@+BXN(%=H
M870@9&\@>6]U(&1I<V-O=F5R/PH*22!L96%V92!A;&P@=&AE('!H:6QO<V]P
M:&EC86PL(&UO<F%L(&%N9"!E=&AI8V%L(&EM<&QI8V%T:6]N<R!O9B!T:&ES
M#61I<V-U<W-I;VX@=&\@=&AE(')E861E<BX*"E1H:7,@96YD960@;7D@<')E
M=FEO=7,@<&]S="X@3F]W(&QE="!U<R!C;VYS:61E<B!S;VUE(')A;6EF:6-A
M=&EO;G,@;V8-=&AI<R!S:71U871I;VXL('=H:6-H(')E<V5M8FQE<R!M86YY
M(')E86P@;&EF92!P<F]B;&5M<R!S=6-H(&%S('1H870@;V8-=&AE(&%S<R!B
M971W965N('1H92!T=V\@:&%Y<W1A8VMS+B!7:&EC:"!S=')A=&5G>2!D;R!P
M96]P;&4@9F]L;&]W/R!4:&%T#6ES(&]P96X@=&\@=&5S="X@02!W96ER9"!T
M97-T+"!M87EB92P@8G5T(&ET(&ES(&5A<WD@=&\@9&\N(")(&%M(&=O:6YG
M#71O('!R97-E;G0@='=O('=H:71E(&-I<F-L97,@;VX@=&AE(&-O;7!U=&5R
M)W,@9&ES<&QA>2!A="!T:&4@<V%M90UV97)T:6-A;"!P;W-I=&EO;BX@66]U
M<B!T87-K(&ES('1O('5S92!T:&4@;6]U<V4L('=H:6-H(&1E=&5R;6EN97,@
M=&AE#6AO<FEZ;VYT86P@<&]S:71I;VX@;V8@=&AE(&=R965N(&-I<F-L92P@
M<W5C:"!T:&%T('EO=7(@9W)E96X@8VER8VQE(&ES#6%S(&-L;W-E(&%S('!O
M<W-I8FQE('1O(%]B;W1H7R!W:&ET92!C:7)C;&5S+B!.979E<B!M:6YD('1H
M870@>6]U(&-A;FYO=`UH879E('IE<F\@97)R;W([('EO=2!A<F4@;VYL>2!A
M<VME9"!T;R!D;R!A<R!W96QL(&%S('EO=2!C86XN(B!)="!M:6=H=`UB92P@
M:&]W979E<BP@=&AA="!W:&%T('1H92!S=6)J96-T(&1O97,@=VEL;"!B92!Q
M=6ET92!S96YS:71I=F4@=&\@=&AE#7!H<F%S:6YG(&]F('1H:7,@=6YU<W5A
M;"!R97%U97-T(&%N9"!O9B!W:&%T(&AE(&UA:V5S(&]F(&ET+@H*5&AE(&5A
M<FQI97(@<&]S="!I;F1I8V%T97,@=&AA="!T:&5R92!C;W5L9"!B92!T:')E
M92!S=')A=&5G:65S+"!B97-I9&5S#6YO;JEP87)T:6-I<&%T:6]N+B!);B!A
M;&P@8V%S97,L('1H92!S=6)J96-T)W,@8F5H879I;W(@=VEL;"!B92!F;W5N
M9"!A=`UO<B!A<F]U;F0@=&AE(&UI;FEM=6T@;V8@82!1J7!L;W0L('=H97)E
M('1H92!T;W1A;"!E<G)O<B!I<R!S;6%L;&5S="X@26X-=&AE(&9I<G-T('-T
M<F%T96=Y+"!T:&4@9W)E96X@8VER8VQE('=I;&P@8F4@9F]U;F0@870@82!M
M:62I<&]S:71I;VX-8F5T=V5E;B!B;W1H('=H:71E(&-I<F-L97,N($EN('1H
M92!S96-O;F0@<W1R871E9WDL(&%N>2!P;W-I=&EO;B!B971W965N#6)O=&@@
M=VAI=&4@8VER8VQE<R!W:6QL(&1O+B!!;F0@:6X@=&AE('1H:7)D('-T<F%T
M96=Y(&]N92!W:&ET92!C:7)C;&4-=VEL;"!B92!T<F%C:V5D(&)U="!N;W0@
M=&AE(&]T:&5R(&]N92P@=VET:"!M87EB92!A;B!O8V-A<VEO;F%L('-W:71C
M:`UB971W965N(&)O=&@N($%N(&5X=&5R;F%L(&]B<V5R=F5R+"!W:&\@:VYO
M=W,@=&AE('1W;R!C;VYF;&EC=&EN9R!G;V%L<RP-8V%N(&]F(&-O=7)S92!E
M87-I;'D@9&ES8V]V97(@=&AE('-U8FIE8W0G<R!S=')A=&5G>2!B>2!A;B!A
M;F%L>7-I<R!O9@UT:&4@<&]S:71I;VX@;V8@=&AE(&=R965N(&-I<F-L92!W
M:71H(')E<W!E8W0@=&\@=&AE('1W;R!W:&ET92!C:7)C;&5S+@H*0G5T(&AO
M=R!A8F]U="!A;B!O8G-E<G9E<B!W:&\@9&]E<R!?;F]T7R!K;F]W(&%B;W5T
M('1H92!T=V\@9V]A;',@86YD#71H92!F;VQL;W=E9"!S=')A=&5G>3\@5VAA
M="!I9B!Y;W4@8V%N(&]N;'D@;V)S97)V92!T:&4@<W5B:F5C="=S#6)E:&%V
M:6]R(&%N9"!W86YT('1O(%]D:7-C;W9E<E\@=&AE('-U8FIE8W0G<R!G;V%L
M*',I/R!4:&%T(&ES('=H870@5&AE#51E<W0@:7,@;65A;G0@=&\@9&\N"@I4
M:&4@9FER<W0@<')O8FQE;2!I<R!T;R!D96-I9&4@:&]W(&UA;GD@<VEM=6QT
M86YE;W5S(&=O86QS('1O('1E<W0@9F]R+@U);B!T:&4@=7-U86P@<VET=6%T
M:6]N+"!W92!T<GD@=&\@=&AI;FL@;V8@97AP97)I;65N=&%L(&-O;F1I=&EO
M;G,@<W5C:`UT:&%T('=E(&-A;B!T97-T(&9O<B!O;F4@9V]A;"!O;FQY+B!)
M9B!T:&4@<W5B:F5C="!U<V5S('-T<F%T96=Y(#$L('1H90UA<'!A<F5N="!D
M:7-C;W9E<GD@=VEL;"!B92!T:&%T('1H92!S=6)J96-T(")C;VYT<F]L<R!F
M;W(B(&%N(&EN=&5RK6UE9&EA=&4@<&]S:71I;VX[('1H92!T=V\@:6YD:79I
M9'5A;"!G;V%L<R!W:6QL(&YO="!B92!D:7-C;W9E<F5D+B!);@US=')A=&5G
M>2`R+"!T:&4@<W5B:F5C="!W:6QL(&%P<&5A<B!T;R`B8V]N=')O;"!F;W(B
M(&%N>2`H<F%N9&]M/RD-<&]S:71I;VX@:6X@82!V87)Y:6YG(')A;F=E.R!Q
M=6ET92!C;VYF=7-I;F<N($%N9"!I;B!S=')A=&5G>2`S+"!C:&%N8V5S#6%R
M92!T:&%T(&]N;'D@;VYE(&]F('1H92!T=V\@9&ES=&EN8W0@9V]A;',@;6%Y
M(&)E(&1I<V-O=F5R960N(%1H92!497-T#6UA>2!T:'5S('EI96QD('-O;65W
M:&%T(&-O;F9U<VEN9R!R97-U;'1S(&%S('1O('1H92!S=6)G;V%L<RP@=VAA
M=&5V97(-=&AE('-T<F%T96=Y+B!297-U;'1S(&1O(&YO="!B96-O;64@8VQE
```

```
M87)E<B!I9B!W92!A<W-U;64@82!P<FEO<FD@=&AA=`UT:&4@<W5B:F5C="!C
M;VYT<F]L<R!F;W(@='=O('-I;75L=&%N96]U<R!G;V%L<RX@26X@9F%C="P@
M:&4O<VAE(&1O97,-;F]T+B!#;VYT<F]L(&ES(&9O<B!T:&4@;VYE(")S=7!E
M<F=O86PB('1H870@:7,@:6UP;W-S:6)L92!T;R!S=6)D:79I9&4-:6YT;R!I
M;F1I=FED=6%L;'D@86YD('-I;75L=&%N96]U<VQY(')E86QI>F%B;&4@<W5B
M9V]A;',N"@I!("AS:6QL>2D@97AA;7!L92X@07-S=6UE('1H870@=&AE('-U
M8FIE8W0@:&%S('1W;R!G;V%L<SH@8W5R<V]R#71R86-K:6YG(&%N9"!C;V9F
M964@9')I;FMI;F<N(%-T<F%T96=Y(#$Z(&1O('EO=7(@8W5R<V]R('1R86-K
M:6YG('=I=&@-=&AE(&-O9F9E92!M=6<@870@>6]U<B!L:7!S.R!T:&4@<&5R
M9F]R;6%N8V4@;V8@;F5I=&AE<B!D<FEN:VEN9R!N;W(-=')A8VMI;F<@:7,@
M;W!T:6UA;"X@36%Y8F4@<W1R971C:&EN9R!T:&EN9W,L(&]N92!C;W5L9"!S
M87D@=&AA="!T:&ES#7)E<V5M8FQE<R!T:&4@;&]G:6-A;"!!3D0@;W!E<F%T
M;W(N(%-T<F%T96=Y(#,Z(&1O(&5I=&AE<B!C=7)S;W(-=')A8VMI;F<@3U(@
M9')I;FL@>6]U<B!C;V9F964L('!O<W-I8FQY(&%L=&5R;F%T96QY+B!3=')A
M=&5G>2`R(&%P<&5A<G,-=&\@8F4@=&AE(&UO<W0@9FQE>&EB;&4@86YD('!R
M;W9I9&5S('1H92!M;W-T(")F<F5E9&]M(CL@:70@86QL;W=S(&%N>0UI;G1E
M<FUE9&EA=&4@<&]S:71I;VXN(%1H:7,@9&]E<R!N;W0@97AH875S="!T:&4@
M;G5M8F5R(&]F('!O<W-I8FQEU``<```6B"P#```````"````<"UP+7`ML`2P
M!!P``-0+<W1R871E9VEE<R!I9B!O=&AE<B!F86-T;W)S(&%R92!D>6YA;6EC
M86QL>2!T86ME;B!I;G1O(&%C8V]U;G0@87,@=V5L;#L-92YG+B!E;FIO>2!Y
M;W5R(&-O9F9E92!W:&5N('1R86-K:6YG(&ES(&5A<WDL(&%N9"!P=70@9&]W
M;B!T:&4@;75G(&%N9`UC;VYC96YT<F%T92!O;B!T<F%C:VEN9R!W:&5N('1H
M92!L871T97(@8F5C;VUE<R!D:69F:6-U;'0N"@I->2!C;VYC;'5S:6]N/R!%
M=F5N(&EF(&$@<W5B:F5C="!D;V5S(&YO="!D>6YA;6EC86QL>2!S=VET8V@@
M8F5T=V5E;@US=')A=&5G:65S.B!I9B!T:&4@<W5B:F5C="!H87,@;75L=&EP
M;&4@8G5T(&-O;F9L:6-T:6YG+VEN=&5R86-T:6YG#2AS=6(I9V]A;',L(&ET
M(&UA>2!B92!H87)D(&]R(&EM<&]S<VEB;&4@=&\@9&ES8V]V97(@=&AO<V4@
M*'-U8BEG;V%L<PUF<F]M(&]V97)A;&P@8F5H879I;W(N($ET(&%P<&5A<G,@
M=&AA="P@86QT:&]U9V@@:70@:7,@=&5C:&YI8V%L;'D-86QW87ES('!O<W-I
M8FQE('1O(&1E<FEV92!O;F4@;6%T:&5M8:QT:6-A;&QY(&5Q=6EV86QE;G0@
M(G-U<&5R9V]A;"(-9VEV96X@82!N=6UB97(@;V8@9&ES=&EN8W0@<W5B9V]A
M;',L('1H92!U=&EL:71Y(&]F('1H870@8V]N8V5P="!I<PUL:6UI=&5D.B!A
M;'1H;W5G:"!S;VUE(%&I9F]R;75L82`H<V5E(&%B;W9E*2!W:6QL('5N:7%U
M96QY(&5X<')E<W,@=&AE#6]V97)A;&P@9V]A;"P@;F%I=F4@:6YS<&5C=&EO
M;B!O9B!T:&4@9F]R;75L82`H<F%T:&5R('1H86X@86X@97AT96YD960-86YA
M;'ES:7,@;W(@82!S:6UU;&%T:6]N*2!M87D@=V5L;"!T96QL('5S(&QI='1L
M92!A8F]U="!T:&4@<F5S=6QT:6YG#6)E:&%V:6]R+B!!;F0@=&AE(&EN=F5R
M<V4Z(&=I=F5N(&%N(&5X=&5R;F%L(&]B<V5R=F5R)W,@<&5R8V5P=&EO;B!O
M9@UT:&4@<&]S<VEB;'D@=F5R>2!C;VUP;&5X(")O=F5R86QL(B!B96AA=FEO
M<B`H=VET:"!R97-P96-T('1O('1H92!S:6YG;&4-(G-U<&5R9V]A;"(I+"!T
M:&4@86YA;'ES:7,@;V8@=VAI8V@@8V]N9FQI8W1I;F<O:6YT97)A8W1I;F<@
M<W5B9V]A;',@87)E#6-O;G1A:6YE9"!I;B!T:&%T('-I;F=L92!S=7!E<F=O
M86P@;6EG:'0@8F4@:6UP;W-S:6)L92P@870@;&5A<W0@9F%R#69R;VT@=')I
M=FEA;"X*"D%N;W1H97(@8V]N8VQU<VEO;B!C;VYC97)N<R!4:&4@5&5S="X@
M5&AE(%1E<W0@;6%Y(&)E(&1I9F9I8W5L="!T;R!A<'!L>0UO<B!Y:65L9"!U
M;FEN=&5L;&EG:6)L92!R97-U;'1S(&EF(&UU;'1I<&QE(&EN=&5R86-T:6YG
M("AS=6(I9V]A;',@87)E#6-O;G1R;VQL960@9F]R('-I;75L=&%N96]U<VQY
M+B!)="!A<'!E87)S('1H870@=&AI<R!W:6QL(&]F=&5N("AA;'=A>7,_*0UB
M92!T:&4@8V%S92!I;B!R96%L(&QI9F4N(%1H:7,@;6%Y(&5X<&QA:6X@=VAY
M(%!#5"!R97-E87)C:"!H87,@=&AU<R!F87(-9F%I;&5D('1O(&EN9&5N=&EF
M>2`B:&%R9"(@*&-O;G-I<W1E;G1L>2!A;F0@8V]N<W1A;G1L>2!P<F5S96YT
M*2!G;V%L<PUO<B!C;VYT<F]L;&5D('9A<FEA8FQE<RX@179E;B!-87-L;W<@
M:7,@;6]R92!E>'!L:6-I="`[J2DN"@I3=')A=&5G>2`S("AC:&]I8V4[(&5I
M=&AE<B!O;F4@;W(@=&AE(&]T:&5R+"!W:71H('-W:71C:&EN9R!F<F]M(&]N
M90US=6)G;V%L('1O('1H92!N97AT(&]V97(@=&EM92D@<V5E;7,@=&\@<&QA
M>2!A;B!I;7!O<G1A;G0@<F]L92!I;B!T:&4-8F5H879I;W(@;V8@8V]M<&QE
M>"!C;VYT<F]L('-Y<W1E;7,@<W5C:"!A<R!H:6=H97(@;W)G86YI<VUS+B!)
M;B!M>0UC87-E+"!F;W(@:6YS=&%N8V4L(&EF($D@9&\@8W5R<V]R('1R86-K
M:6YG+"!T:&%T("AS=6*I<W5BJ7-U8B$I9V]A;"!I<PUE87-I;'D@9&ES8V%R
```

```
M9&5D('=H96X@<V]M96]N92!S:&]U=',@(F9I<F4A(BP@;W(@979E;B`B=&AE
M(&-O9F9E92!I<PUR96%D>2$B($%N9"!E=F5N('-O;65T:&EN9R!A<R!U<F=E
M;G0@87,@8G)E871H:6YG(&ES('1E;7!O<F%R:6QY('-T;W!P960-=VAE;B!Y
M;W4@;F5E9"!T;R!R96-O=F5R(&$@=V%T8V@@9G)O;2!T:&4@9&5P=&AS(&]F
M('1H92!S=VEM;6EN9R!P;V]L+@H*22!H;W!E('1H:7,@;6%K97,@<V]M92!S
M96YS92!T;R!Y;W4L($UA<G1I;BX@66]U<B!I;G1U:71I;VX@:7,@<FEG:'0N
M($EN#7!A<G1I8W5L87(@<W1R871E9WD@,B`H;6%Y('=E(&-A;&P@:70@(F9R
M965D;VTB/RDL(&)U="!A;'-O('-T<F%T96=Y(#,-*")C:&]I8V4B*2!D;V5S
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------------------------------

Date:    Mon, 16 Dec 1996 09:14:11 -0800
From:    Richard Marken <marken@AEROSPACE.AERO.ORG>
Subject: Computed Output, Java Demo

[From Rick Marken (961216.0910)]

Hans Blom (961216c)

>You _do_ control for very different systems concepts. Let's leave it
>at that.

Sure. Just as soon as you stop wasting CSGNet bandwidth carrying on about the
virtues of a non-falsifiable (to you) computed-output model of behavior.

Most robotics textbooks, journals, newsgroups, professional societies, and
departments of engineering take your computed-output model of behavior for
granted. So there it's quite easy for people to learn your perspective on
behavior, which is the dominant perspective _by far_, without going to
CSGNet. CSGNet is the _only_ place people can learn the control of input
model of behavior. If you want to present your computed output model here
on CSGNet, that's fine. But I think it behooves you to provide data that
shows why we should pay attention to a model that many of us rejected long
ago on the basis of data.

You apparently will neither present data in favor of a computed output model
nor pay attention to data that argues against a such a model. Moreover, your
only argument for considering the merits of the computed output model is that
"everyone else is using it". This appeal to authority carries less than zero
weight with many in this group.

The effectiveness of your advocacy of a computed output model of behavior
is also reduced considerably by your persistant failure to understand even
the model basic facts about how a hierarchical control system model works.
For example, in a post today [Hans Blom (961216e)] you make this remarkably
incorrect statement:
```

>(H)PCT models _also_ need to compute the right output.

And just to show that you weren't kidding, when Bill Powers said:

>That basic concept absolutely REQUIRES an internal world-model, and also
>the ability to compute the INVERSE of the same world-model.

you reply with:

>Upon closer inspection, you will find the same in (H)PCT models.

It is hard to imagine a more complete misconception about how a control system works. It's because of this kind of, well, crap, that we had to create CSGNet in the first place. Your two comments above are the kind of thing we hea from engineers, roboticists and "motor control" theorists who are trying to convince themselves and their colleagues that they are on the right track; that PCT is either nothing new or nothing particularly important. It's the kind of thing that led to the development of CSGNet in the first place. If you keep dumping stuff like this on CSGNet, someone's going to keep trying to clean it up.

Bill Powers (961216.0145 MST) --

>The new demo works very nicely.

Thanks.

>I notice that the exponential runaway of the model is uniformly about twice
>as fast as my own. This is probably because of the size of the "dt" that's
>appropriate for the (slow) repetition rate of the Java program on my machine
>(100 MHz 486). If others see the same effect, maybe you could compensate in
>the model program.

Yes. I'd be interested in hearing how it works for others. The curve fits my data rather well on most runs (when I am paying attention and really trying to control the cursor).

>It might be nice if you could devise a calibration program

That's an excellent idea. I included the model results in this one because I think it really aids understanding of what is being demonstrated. I agree that it's probably best if the model data end up right on top of the subject data. But even if the fit is not perfect, I think it's still worthwhile if the subject can see the exponential runaway pattern of both subject and model after the polarity shift.

>It's too bad that Java is so slow. My tracking is a little unstable because
>of the lag. Any Java experts out there who have a fix for that?

I think the only real fix is the "just in time" compiler that may ship with the next iteration of Netscape. But for now I think a calibration program (or, perhaps, just a program that asks the type of machine you are on and then sets the animation loop lag appropriately) is a good idea. I'll give it a try.

Best

Rick

Date: Mon, 16 Dec 1996 12:32:37 -0500
From: Martin Taylor <mmt@HERMES.DCIEM.DND.CA>
Subject: Re: Popper's Preposterous Propositions

[Martin Taylor 961216 12:25]
> Rick Marken (961215.1120)

Rick's agreed with me for too long. Several days now, at least. Can't have that:-)

>... This
> suggests that the inanimate environment is a purposeful agent, doing
> whatever it does to organisms in order to get them produce particular
> behavioral results. It is easy to show that the inanimate environment
> has no such purpose.
>

Whoops! How, pray? By using the methods of PCT? Applying The Test? What if the "purpose" of the inanimate environment is to get you to believe there is no purpose?

Surely this is one of the things we take on faith, just as the Pantheists take the opposite position, on faith?

How about substituting for "It is easy to show.." the words "To continue any scientific endeavour, we have to assume..."

Martin

Date: Mon, 16 Dec 1996 11:05:16 -0700
From: "T. B. Harms" <tbh@TESSER.COM>
Subject: Re: Solving "loss of input" problem

*from Tracy Harms 1996;12,16.10:40

Hans Blom, 961216d

>[...] In all cases, the rabbit -- or part of it -- is
>not perceived. Yet the fox is still controlling for chasing the
>rabbit. I do not care for a fundamental and strict separation between
>control, where the fox sees (all of?) the rabbit and non-control
>("computation of output"), when it does not. I think the difference,
>and its behavioral consequences, are gradual -- not fundamental, and
>I also think that there is a quite gradual change from one to the
>other "strategy". And I finally think, in contrast to PCTers, that
>pretty much the same mechanism underlies both. Anyway, I see no
>switching transients.

I propose that the appearance of gradualness is an illusion which comes from parallelism and redundancy. Foxes go a long way toward closing the gap with rabbits by smell, and perhaps sound. Sight is necessarily low among control systems because foxes cannot catch rabbits in a race; they must catch them by stealth. (This is why rabbits stand tall when they see

a fox: it lets the fox know that the rabbit knows it is there, and thus the hunt is effectively ended. Rabbits do not stand when they see a dog, they run, for it is not clear that the rabbit will be able to outrun a dog.) Regardless of the particular combination of methods by which the higher level perception is facilitated, the multiplicity of simultaneously relevant perceptions is what gives things such as "hunts" their gradual curves.

I would agree that some of these controllings allow the hunt to continue with cunning effectiveness even when major perceptual channels are blinded, and even that it is precisely to cover for such sporatic and temporary blindings that these "models" exist. But I would guess that the nature of these internally-directed systems is such that they are not parallel to the world which is the subject of attention. Rather than being *models*, these are *strategies* or *tactics* which may increase the effectiveness for the higher-level perception. For example, perhaps a hunting animal responds to the concealment of its prey by pulling itself into the beginnings of a direction-changing posture. If the prey keeps running straight this reaction was a slight loss, but if the prey switched course while hidden it is a relative gain. But that reaction, while reflecting the (statistical) behavior of prey in a vague sense, does not *model* it.

The main problem I have with the proposed model-based control is that it is too expensive. The cost of good models vastly outstrips their possible benefit.

>If the rabbit is
>behind a wide bush and invisible to the fox for more than a moment,
>and if it picks that moment to reverse in it tracks, the fox's
>"prediction"/control would be suboptimal. But only gradually worse
>than when it reverses in plain sight.

No, it is not gradually worse, it is *measurably* worse. If the fox's response time is a function of the duration between the rabbit's direction change and the fox's observation of the rabbit, the fact that this magnitude is correctibly small does not defy the basic claim that a fox cannot respond to a visual observation of its prey without seeing it. (I'm not sure how to improve that statement over the tautological way I put it here.)

Tracy Bruce Harms tbh@tesser.com
Boulder, Colorado

Date: Mon, 16 Dec 1996 10:12:31 -0800
From: Rick Marken <marken@LEONARDO.NET>
Subject: Re: Decision

[From Rick Marken (961216.1010)]

Stefan Balke (961216.1640 CET) --

> What is a _rational_ solution to the conflict.

Oops. I didn't mean to give the impression that I think there IS a rational solution to conflict. I just meant that when we are in a conflict it seems that we are often "arguing with ourselves" the merits

of which course of action to take. This argument is essentially irrelevant to the conflict since it doesn't deal with the real _cause_ of the conflict -- the fact that we have set goals that cannot possibly be satisfied simultaneously. This internal argument is probably generated by error in a control system that is affected, in some way, by the conflict. This internal argument won't solve the conflict but it may change the gain of one side of the conflict long enough to get that side to "win" for the time being (you eat the ice cream) but the conflict is still there.

>Do you think that something like experience and observation based
>probability estimations of the outcomes of the conflicting goal are
> involved.

Sure. They are used in the "argument"; but they don't solve the conflict. The only way to solve a conflict is to change the goals that are creating the conflict. And the only way to change the goals that are causing teh conflict (I think) is to see the conflict from a point of view where the conflict producing goals are seen as _options_ rather than _requirements_.

Best

Rick

Date: Mon, 16 Dec 1996 13:25:24 +0000
From: Peter Cariani <peter@EPL.MEEI.HARVARD.EDU>
Subject: Re: Early language learning

[from Peter Cariani ((961216.1200 EST)]

> [From Bill Powers (961214.0600 MST)]
>
> Peter Cariani ((961213.1700 EST)--
>
> >When we hear running speech
> >there are time patterns corresponding to the acoustic transients
> >in the speech waveform and their neural correlates can be found
> >at all levels of the auditory system (i.e. large numbers of neurons
> >are following the slower modulations in the waveform).
> >Each one of these syllables has a characteristic
> >temporal structure (rhythm, onsets and offsets of vowels, etc).
>
> This is an interesting post, in that it reminds me strongly of the days when
> I first became aware that my own perceptions were involved in everything I
> experienced (instead of just seeing the world as given as I always did
> before then). When you hear speech, you're obviously noticing
> "characteristic temporal structures," but it's not so obvious that when you
> look at the neural correlates of speech, you're ALSO noticing
> "characteristic temporal structures." It's a particular way of perceiving
> that is involved in BOTH cases.
>
> The fact that you're talking about human perception becomes more obvious
> when you think of trying to devise an instrument that will indicate
> automatically which characteristic temporal structure is present (whether
> you're hearing sounds or looking at oscillograph displays). Suddenly you're
> neck-deep in the problem of automatic pattern recognition, which remains

> essentially unsolved. And you're faced with the problem of saying what it is
> to recognize something.

Yes. To tackle the problem of how the nervous system recognizes something is to tackle general problems of pattern recognition. It's harder, of course, because the nervous system may use strategies that no human artefact has ever incorporated. One must be a scientist and an inventor at the same time.

> If you listen to speech with a trained ear, so you can hear all the rising
> and falling pitches, the phonemes, the transitions between phonemes, and so
> on, you're clearly not listening to "temporal structures;" you're listening
> at one or more lower levels where you can discern the _elements that are
> structured_. The same goes for looking at records of neural firings, spread
> out so you can see their details. When you talk of neural "correlates"
> you're literally looking at "correlated variations" among the elements that
> make up the pattern. You're saying "this goes up when that goes up; this
> pauses when that pauses, this is continuous when that is continuous." What
> you've done, in fact, is to _eliminate_ the temporal structure and reduce it
> to a point-by-point comparison.

Basically we observe the responses of auditory neurons and we construct hypotheses about what aspects of those neural responses might carry the information needed to discriminate speech sounds. The strategy is to try to find neural "representations"
(aspects of neural responses of ensembles or populations of neurons) whose features covary with perceptual judgments. In other words, some (metameric) sounds that are not percpetually distinguishable should not be distinguishable in a putative neural representation if that is the information that is being used to subserve the percept. Similarly, all of those sounds that are distinguishable should be distinguishable in the representation.

> But that isn't what we mean by a temporal structure. Point by point
> comparisons don't reveal any _pattern_ unless they're being examined by a
> human being who _does_ perceive something other than the momentary states of
> the world. That's why we have no "pattern meters" to put on the shelf beside
> our voltmeters and light meters and frequency meters and VU meters. We have
> no meters that will show us, as a pointer position on a scale, the degree to
> which a given pattern is actually present. We have none, that is, except the
> one that we carry around in our heads.

There are "pattern meters" like spectrograms and autocorrelograms that incorporate temporal pattern into spatial patterns that can be compared. We could have transformed our patterns into any other kind of sensory display (odors, sounds, shocks, tactile patterns, tastes) and compared them, but "visualization" seems most straightforward for this kind of work.

(There was someone who converted DNA sequences into sequences of musical notes and could, probably for the same reasons that babies hear out the repeated syllables and I hear out the repeated click pattern, hear out common nucleotide sequences. There are whole Santa Fe Institute-sponsored conferences on these issues of "Auditory Display".)

> Now, what is it to "recognize" a pattern? If you can observes "acoustic
> transients in the speech waveform and their neural correlates," then you're
> obviously not looking at the pattern, but at its elements. When you're
> perceiving a pattern, what you observe is the pattern, a unity; you can say
> that this pattern is present to some degree or in its ideal form, but that's
> a one-dimensional measure of pattern-ness: more or less present. When you
> look at the transients and timings and amplitudes, you're looking at the

> _inputs_ to a pattern-recognizer. When you get simply the sense of a pattern
> being present to some degree, you're looking at the _output_ of the
> recognizer. The space between the inputs and outputs contains that device
> that we can't yet build, but that exists in working order in our brains.

Yes, but as I say, we can build putative representations (which are effectively
devices) that give us outputs which we can then compare with the psychophysics.

> When you look at neural correlates of acoustic patterns, if you can trace
> the correlations between the sounds and the neural signals you aren't
> looking at the outputs of the pattern recognizers, but only at the inputs
> which have not yet reached these recognizers. You won't have found the
> temporal pattern recognizer until you find signals that are present when the
> pattern is present, absent when it is absent, and remain _constant_ as long
> as the same temporal pattern is going on. YOU may observe the same pattern
> in the sound and in the neural firings, but that's only because you're using
> your own pattern recognizer. That's why YOU get a sense of the same pattern
> recurring in both the sound and the neural firings. What you're really
> looking for is a place in the nervous system where there's a signal that
> corresponds to your own _sense of pattern_. That is, this signal should be
> present while you're experiencing a certain pattern-ness in the sound, and
> absent when you're not. It should most definitely NOT covary with each
> transient feature of the pattern, either in the sound or in the neural firings.

So the reason that I have such a strong feeling about this is that the temporal
patterns in the neural responses are those stimulus-related aspects of the
neural responses that are most invariant over a wide, wide range of stimulus
conditions (over a huge 40-50 dB dynamic range, in noise, when there are
competing sounds, coming from different directions in audio-space, etc. etc.).

> >So it's not necessarily baffling that we hear these things.
> >Our auditory system is made to detect recurring time patterns.
> >A temporal memory trace and a means of cross-correlating the
> >incoming (neural) time pattern with what's stored is all you
> >need.
>
> That sounds easy, but it's not that easy.

I agree it's not that easy (I'm trying to be light-hearted about this),
but I think it's much easier than any of the ways that
have hitherto been proposed for how we do those things. I know of no
current maintream neural model of speech perception whose
neurophysiological assumptions hold up under scrutiny.

> To do a cross-correlation between
> two signals, you have to make sure they're synchronized, which means
> starting the memory signal at the right instant relative to the perceptual
> signal. So you need at least some means of locking the repetitions together.

A rich set of relative delays does the trick. If you get anything major
out of any relative delay channel, you've detected a similarity.

> And you also have to lock the _speed_ of the memory signal to that of the
> perceptual signal. The word you remember must be running at the same speed
> as the word you're hearing; otherwise even if you start the memory-word at
> the same instant the real word starts, they'll be out of synch before the
> word ends.

This time-warping is easily accomplished by sets of tapped delay lines with different
conduction velocities. I'm planning to write a short paper on these computational primitives in the spring.

> And of course the words must be spoken and remembered with
> identical pitches and inflections. When you think about actually building a
> cross-correlator that will work with continuous speech, the project becomes
> considerably more complex than it seems when you just describe what it's
> suppose to accomplish.

Of course it is. But it may be much easier to do these things in the time domain than by warping time-frequency patterns in degraded spectrograms.

> Your example of the cross-correlator does have one essential feature of a
> pattern recognizer: its output is not a pattern, but a number indicating the
> degree to which a specific pattern is present. As long as the pattern is
> present to the same degree, the number will remain CONSTANT. If the pattern
> changes so it is less like the nominal pattern, the number will get smaller.

We can go into the nuts and bolts of it some other time when there is more time. Just as the output of a filter is not only the magnitude of the output (like an rms of the signal coming out), but also the waveform of the the output signal, the output of a cross-correlator is not only a scalar "similarity" index, but the pattern of the output of the operation. The pattern also tells you what aspects of the two signals are the same.

> What _is_ the "obvious" alternative? If it's to say that the pattern is
> "right there in the neural signals" I believe you're completely mistaken.
> You're simply applying your own ability to detect patterns to the neural
> signals. Without that ability, the signals would just be signals.

First off, the temporal patterns in the neural responses are not exact mirrors of the stimulus, although (I think) they preserve those aspects that are invariant perceptually. I can't read in invariances that covary with percepts that aren't there in the neural responses (I do decide what aspects of the neural response I will look at, my observables, but I don't determine what the values of those observables are -- my observations determine that). All I'm saying is that neural representations based on timing information are viable and constitute strong candidates for how the brain may represent perceptually-important aspects of the speech signal.

> If you said the word "Mississippi" to a dog, you could probably find neural
> correlates of that acoustic input in the dog's brain. But the dog would not
> perceive the word "Mississippi", would it?

Might depend on the dog. I'll quote Ernst Mach on this one:

"What is true of the pendulum is true of every vibrating body.
A tuning fork, when it sounds, also vibrates.
It vibrates more rapidly when its sound is higher;
more slowly when it is deeper. The standard A of our
musical scale is produced by about four-hundred and
fifty vibrations per second.
"...We strike as many [differently tuned] forks as we will,
the fork tuned to A is perfectly indifferent to their notes;
it is deaf, in fact, to all except its own; and if you
strike three, four, five, or any number whatsoever, of
forks all at the same time, so as to make the shocks which

come from them ever so great, the A fork will not join in
the vibrations unless another fork A in the collection struck.
It picks out, in other words, from all the notes sounded,
that which accords with it.
"The same is true of all bodies which can yield notes.
Tumblers resound when a piano is played, on the striking
of certain notes, and so do window panes. Nor is the phenomenon
without analogy in different provinces. Take a dog that answers
to the name "Nero." He lies under your table.
You speak of Domitian, Vespasian, and Marcus Aurelius Antonius,
you call upon all the Roman Emperors that occur to you, but the
dog does not stir, although a slight tremor of his ear tells you
of a faint response of his consciousness. But the moment you call
"Nero" he jumps joyfully towards you. The tuning fork is like your dog.
It answers to the name A." (Mach 1865)

Forgive me for the shagginess of the dog, for "The Dog is Us." *

Peter

*the title of a long-past Rolling Stone article on why the Sixties ended

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Date: Mon, 16 Dec 1996 13:18:22 EST
From: Bruce Gregory <gregory@CF4.HARVARD.EDU>
Subject: Re: Popper's Preposterous Propositions

[From Bruce Gregory (961216.1320)]

Martin Taylor 961216 12:25

> How about substituting for "It is easy to show.." the words "To continue
> any scientific endeavour, we have to assume..."

How about, "We have no evidence..."?

Bruce Gregory

Date: Mon, 16 Dec 1996 13:23:42 EST
From: Bruce Gregory <gregory@CF4.HARVARD.EDU>
Subject: Re: Solving "loss of input" problem

[From Bruce Gregory (961216.1325)]

Tracy Harms 1996;12,16.10:40

> Foxes go a long way toward closing the
> gap with rabbits by smell, and perhaps sound. Sight is necessarily low
> among control systems because foxes cannot catch rabbits in a race; they
> must catch them by stealth. (This is why rabbits stand tall when they see
> a fox: it lets the fox know that the rabbit knows it is there, and thus the
> hunt is effectively ended. Rabbits do not stand when they see a dog, they
> run, for it is not clear that the rabbit will be able to outrun a dog.)

You are a gold-mine of information!

> The main problem I have with the proposed model-based control is that it is
> too expensive. The cost of good models vastly outstrips their possible
> benefit.

It certainly seems that way to me as well.

Bruce Gregory

Date: Mon, 16 Dec 1996 10:38:46 -0800
From: Rick Marken <marken@LEONARDO.NET>
Subject: Re: Popper's Preposterous Propositions

[From Rick Marken (961216.1040)]

Martin Taylor (961216 12:25) --

>Rick's agreed with me for too long. Several days now, at least. Can't
>have that:-)

You ain't got it!

Me:

>It is easy to show that the inanimate environment has no such purpose.

Martin:

> Whoops! How, pray? By using the methods of PCT? Applying The Test?

You got it!

>What if the "purpose" of the inanimate environment is to get you to
>believe there is no purpose?

It would do something to protect the controlled variable (its
perception of my belief that it has no purpose) from the effects of
disturbance (such as my belief that it _does_ have purpose).

> How about substituting for "It is easy to show.." the words
> "To continue any scientific endeavour, we have to assume..."

Because then I would be turning away from experimentalism (which I like)
and toward scholasticism (which I abhor);-)

Best

Rick

Date: Mon, 16 Dec 1996 13:38:40 EST
From: Bruce Gregory <gregory@CF4.HARVARD.EDU>
Subject: Re: Decision

[From Bruce Gregory (961216.1340 EST)]

Rick Marken (961216.1010)

> Sure. They are used in the "argument"; but they don't solve the
> conflict. The only way to solve a conflict is to change the goals that
> are creating the conflict. And the only way to change the goals that are
> causing the conflict (I think) is to see the conflict from a point of
> view where the conflict producing goals are seen as _options_ rather
> than _requirements_.

Think of how simple life would be if we asked whether some action
might lead to conflicting goals _before_ we acted!

Bruce Gregory

Date: Mon, 16 Dec 1996 14:09:59 +0000
From: Peter Cariani <peter@EPL.MEEI.HARVARD.EDU>
Subject: Re: Aristotle and Contingent Truths

>From Peter Cariani (961216.1430)]

> [From cr4@AXE.HUMBOLDT.EDU]:
>
> Peter Cariani wrote:
>
> > This is an all-too-common misconception. One doesn't need a theory to make
> > a measurement, to write down the value of a pointer reading. One does need
> > a model, however to make a prediction of the value of some other observable
> > from the pointer reading. One needs a theory if one is going to make more
> > general explanations or interpretations of what's going on. Observations
> > can stand by themselves, as long as one describes what one has done to
> > make them (how the system has been prepared, how the measurement apparatus
> > was built and calibrated, etc etc). P.W. Bridgeman was always very clear on
> > these kinds of operational points.
>
> Greetings,
>
> This seems to imply that that measurement of events can be made in some
> absolute way. They ultimately can't, IMO, because an event can only be
> measured in respect to some other event, never to some absolute position
> or instant. What are measured are always space intervals and time
> intervals. This "relativity" of events implies that 4x coordinate
> systems (x, y, z, t,) must be FIRSTLY CHARACTERIZED by the setting of an
> origin. (theoretical framework PRE-defined by observer?)

Who said anything about "absolutes"? One doesn't even need a characterized
4x coordinate frame to make a measurement --- what about geiger counters or
the gels of molecular biologists or the brake-fluid light on your car? I
agree that the interaction of the measuring device and its environment takes
place over a finite duration and in some spatial location, but operationally
one does not need to characterize the measuring device or its operation before
one can use it as a measuring device. Early electrical researchers used
frog's legs to detect electrical potentials -- they didn't know how their
measuring devices worked or why -- but they could nevertheless reliably use
them to detect changes in test assemblies.

> > > Theories inform which observables (which measuring devices we use) we choose
> > > for our models, but once that choice is made, the outcomes of the measurements
> > > themselves had bloody well be independent of our theoretical framework
> > > (or we are deceiving ourselves).
> >
> > See above.

See above.

> > > Thus, observation is possible without a theoretical framework but
> > > pragmatically "meaningful" observations are only possible within
> > > some broader framework (a model or theory or control system).
> > >
> >
> > Any set of physical data takes its complete sense only when it is
> > accompanied by the measurement errors and uncertainties, and more
> > generally the "resolution" characterizing the system under
> > consideration. Any measurement is made with some finite "resolution"
> > (change in: x, y, z, t). This resolution should be included in the
> > (pre)definition of of said coordinate system. Being itself a length or
> > time interval, it is subjected to the relativity of SCALES.

You must be either a physicist, mathematician, or philosopher. What does "complete sense" mean? Our knowledge of the material world is always incomplete.

See above.

> > Treating measurments as absolute (independent of observer reference
> > frame) assumes that space/time is continuous and DIFFERENTIABLE.
> > Feynman demonstrated that, ultimately, this is may not be the case when
> > he identified quantum "particle" trajectories (real and virtual) as
> > continuous but NON-DIFFERENTIABLE. This suggests that such trajectories
> > may encompass INFINITE geodesics (fractal geometry?). As a result,
> > measurements at the quantum scale are rendered statistical probabilities
> > in a (theoretical) framework of absolute Minkowskian space, whereas they
> > are taken in the assumed context of locally Euclidian (linear)/ globally
> > Rhienmannian (curvilinear) at larger scales. Thus scale dependent.

What on earth are you talking about here?

I'm not talking about measurements as absolutes. Being able to calibrate a measuring device so that if we set up two measuring devices under the same (apparent) experimental conditions, we get the same results. Measurement does need to be absolute (i.e. you know what you're measuring) in order to be replicable (i.e. you get the same results as I do when you follow my directions for setting up the experiment). The operations of building, calibrating, and preparing measuring devices had better be clear and reliable regardless of what theory one adopts.

See above.

> > [From Rick Marken (961213.1000)]
> > > Are there really people out there who want to go back to
> > > Aristotle's and Aquinus' approach to knowledge -- pure reason?

> >
> > Them's fightin' words. I knew Aristotle. Aristotle was my friend. This aint
> > the Aristotle that I know. You can say anything you want about Aquinas,
> > but (as he tells me in my sleep) Aristotle always insisted on
> > the complementarity of the Necessary and the Contingent,
> > i.e. of analytic truths and of empirical truths. Kant obviously
> > was an Aristotelian in this sense, and a major disaster of 20th century
> > philosophy has been Quine's muddying of the distinction between logic
> > and observation. The Catholic church really did a number on Aristotle in
> > the middle ages, platonizing him in the process. Don't trust what the Jesuits
> > taught you in parochial school about Aristotle. Plato's your villan.
> >
>
> I'm certainly less a fan of the big A. Methinks, ultimately, that the
> "Non-Aristotlean"'s approach will have more general application. They
> seem more at home in a "relative" universe.

There is an excellent book by Graham called Aristotle's Two Systems (Oxford, 1987) (unfortunately Oxford U Press has not kept the paperback version in print -- must be some kind of crypto-platonic conspiracy) in which Graham discusses Aristotle's
"hylomorphic" view of the world. Graham offers a very persuasive interpretation in which Aristotle's first system was an axiomatic-deductive (ontological) system that was observer-independent, but his thinking evolved to his second system, the hylomorphic one, in which the properties of things are generated by their interactions with an observer (i.e. properties are joint properties of observer and the observed material system). This is a relativistic system par excellence, one
that is not so far from that of Bohr and Bridgman.

Again, (arguably) I think the perception of Aristotle was distorted by the Catholic
interpretation. I have no idea who your NonAristotelians are or what they think.

I only hope it's not Popper. The idea of "falsifiability" is reasonable if it's applied to models that make specific predictions (either the prediction matches the experimental outcome or it doesn't), but as Feyerabend showed over and over it can't apply coherently to such ill-defined conceptual constructs as are whole theories. Theories contain many tacit assumptions and images of how things are supposed to hang together. Models, on the other hand, better be completely explicit in their assumptions, methods (which observables, which mathematical operations), and predictions. Much confusion in the philosophy of science has involved the conflation of "scientific theory" and "scientific model". Popper also had the handicap of trying to be a realist and to do epistemology at the same time -- it just doesn't work.

Peter

Date: Mon, 16 Dec 1996 14:26:11 EST
From: Bruce Gregory <gregory@CF4.HARVARD.EDU>
Subject: Re: Aristotle and Contingent Truths

>From [Bruce Gregory (961216.1430 EST)]

Peter Cariani (961216.1430)

> The idea of "falsifiability" is reasonable if it's

> applied to models that make specific predictions (either the prediction matches
> the experimental outcome or it doesn't), but as Feyerabend showed over and over
> it can't apply coherently to such ill-defined conceptual constructs as are
> whole theories. Theories contain many tacit assumptions and images of how things
> are supposed to hang together. Models, on the other hand, better be completely
> explicit in their assumptions, methods (which observables, which mathematical
> operations), and predictions. Much confusion in the philosophy of science
> has involved the conflation of "scientific theory" and "scientific model".

Indeed!

> Popper also had the handicap of trying to be a realist and to do epistemology
> at the same time -- it just doesn't work.

I've often had that thought about Popper. He was a maddening
combination of sweet reasonableness and dogmatism. (Come to
think of it, he's not the only one.... ;-))

Bruce Gregory

Date: Mon, 16 Dec 1996 12:54:02 -0700
From: Bill Powers <powers_w@FRONTIER.NET>
Subject: Re: Vancouver's experiment

[
From Bill Powers (961216.0910 MST)]

Hans Blom, 961216 --
>
>>>If the mouse gets stuck, the loop gain decreases to zero; no
>>>controller, model-based or not, will be able to cope with this
>>>situation immediately.
>
>>Not true. The response of a simple control system will be to
>>increase the output immediately, which in the case of the stuck
>>mouse ball will usually free it up and permit control to continue.
>
>That happens, yes. But sometimes (often?) not.

Actually it's the normal mode of operation of a mouse. A mouse ball is
always slipping a little on the table or pad and relative to the rollers
that it turns inside the mouse; that's one reason I regret having to use a
mouse in our tracking experiments. We're measuring the number of counts from
the mouse quite accurately, but as a tracking experiment proceeds, the mouse
position itself gradually departs from the position indicated by the counts.
If you try to run the experiment longer than a few minutes, the mouse will
probably drift off a small mouse pad. The person doing the tracking knows
nothing of this drift, of course, until the mouse runs off the pad. The
slippage, however, makes no difference in tracking accuracy. It would be
extremely hard to explain how a world-model could compensate for this slippage.

The world is full of little disturbances like this, which we never notice
because we automatically compensate using closed-loop control.

>>The great advantage of the PCT model is that when an unexpected
>>disturbance does occur, the system will automatically increase or

>>decrease its output to oppose the effects of the disturbance,
>>without requiring any prediction that the disturbance is going to
>>occur.
>
>In the case that you refer to above, an "unintended side effect" is
>that this resolves the problem. It doesn't always; if it doesn't, >how long
should one stick to this "method"? When the door hinges >are a
>bit rusty, you will still open the door with this "unintended"
>method, but not when it is locked. In some cases such a strong
>response will actually be harmful (to door or muscles). If that can
>be foreseen, a different solution method, based on a more extended
>knowledge of cause-effect relationships, would be better. What you
>present as a solution here is not really one.

But it is. You seem to keep forgetting that reorganization is part of the PCT model just as much as your model. It's simply applied differently -- to a closed-loop control system instead of to an internal world-model control system. There's no reason that reorganization can't be present and working at all times in the PCT model as it is in yours. Reorganization is NOT the difference between your model and the PCT model.

The main difference is that in your model any unexpected or unpredicted disturbance will be unopposed, because the controlled variable is not being compared directly against the reference signal. ONLY those disturbances which are part of the world-model can be opposed in your scheme. In the PCT model it's not necessary for any disturbance to be represented inside the system; the departure of a controlled variable from the reference condition is itself enough to alter the output to oppose the deviation. It doesn't matter what caused the deviation.

In the case of the PCT system, the problem of the locked door is handled by a higher-level system. The first reaction is, as you say, to pull harder. But if a higher system senses abnormally large efforts going on, it will perceive this as an error and correct for it by selecting some other lower-level control systems, such as those that fish a key out of the pocket and unlock the door. If no existing control system at any level can solve the problem, then and only then is reorganization called for. That's the main point of allo the control systems at all the levels - to act automatically to prevent recurrance of the conditions that lead to reorganization. Your model works the same way: as long as the parameters are appropriate, the Kalman Filter part of the model has nothing to do.

At that point, both your model and mine run into a problem: how does a _new kind_ of organization come into being? As long as we're talking only about varying parameters in a system of a specific form, we can think of many feasible methods of reorganizing, the EKF being a nice one, although computationally expensive. But when we encounter, for the first time, a door that has to be opened by pressing a button on the wall, we can't get to the new organization simply by extending or modifying the old one that used to open the door. In your case, you need a radical change in the FORM of the world-model, and your model can't provide a new form. It can only alter the parameters of the existing form.

>>You describe what must be done to make such systems work, without
>>apparently seeing how impractical these requirements are for any
>>_general_ model of behavior. You're talking about complex
>>troubleshooting and cognitive processes that are typical of the >>way a
whole intelligent human being might approach a problem with >>a piece of
machinery -- not about the sort of process that would

>>account for walking on a slippery surface or manipulating a mouse >>on a table with slippery spots.
>
>Oh yes, I am. If you can see the spots that are slippery, you can >avoid them. If not, you can try to find a way of walking that won't >let you slip on those spots. It's not too difficult to learn to >walk on ice. It's not too difficult either to learn to walk on a >sidewalk that has (sometimes invisible) patches of ice; you only >have to assume that there is/could be ice _everywhere_.

I've seen people who walk on ice this way. They're usually elderly and deathly afraid that if they slip, they'll die. Others, however, are less intimidated and more confident of their own control systems. They slip, but they don't fall. Some of them, when spying a slippery patch, will actually take a run at it and slide across it yelling WHEE! They don't have to take cognitively-directed precautions; they know that if they slip, their control systems will almost instantly correct the error.

>
>>What's most troublesome for the model-based approach is predicting >>_when_ it will get stuck.
>
>That is right. Models cannot be comprehensive and fully accurate. >Surprises cannot be excluded. The world is not fully predictable, >and even if it were, modelling it would lead to a far too complex >model. What _can_ be included into a model, however, is methods of >how to handle particular classes of surprises when one is >perceived.

The hierarchical control model is ideally suited to that kind of behavior. The higher systems can choose lower-level reference signals to manipulate as appropriate to different _classes_ of situations, but they don't have to deal with the details of any situations; the lower-level systems take care of the details. Reorganization isn't necessary as long as all these levels of control contain a solution to the problem. I appreciate your mention of the unpredictability of the world, and of the fact that even if it could be predicted, doing so would require implausibly complex (and knowledgeable) computations.

>
>>A more complex model would clearly be required when a simpler one >>leaves significant parts of the behavior in question unexplained, >>or predicts them incorrectly. If a model-based control system >>could explain behavior that a simpler model couldn't explain, it >>would not only "fulfill a meaningful role," but would be the only >>available explanation. We would have no choice but to use it.
>
>I guess that in any particular situation you might come up with a >demonstration that "learning" isn't necessary, because you have >built in a mechanism that lets the controller perform equally well. >In such cases I would say that _you_ have done the learning for the >system -- probably through a lengthy process of trial and error -- >and implemented only its end result, either in hardware or >software. I'm not interested in that approach because it cannot be >generalized.

Hans, can't we do away with this mistaken idea that the PCT model doesn't adapt? Why do you assume I say that "learning isn't necessary," when there is a whole chapter in B:CP entitled "Learning"? The concept of adaptation and reorganization has been an intrinsic part of the PCT model since the first paper in 1960. You're making this assumption only because the models I have presented most always have no reorganizing capability; that's mainly

because I have no good general model for all the kinds of reorganization that have to happen. I therefore stick, for the most part, to investigating the properties of control systems once they have been learned. That means that I estimate the parameters of the system from data, but don't offer any explicit model that explains how they come to have those values.

Your concept of adaptation works ONLY with a model having a certain given architecture. You have to give the world-model a form before the EKF approach has any parameters to modify: your model doesn't create that world-model from scratch, because you don't know how to make it do that. You can't explain where your world-model comes from, and how it happens that the _inverse_ of the world-model also magically appears, somehow sharing the adjustable parameters with the forward world-model. Setting up the basic world-model, taking its inverse, and transferring the parameters are all things YOU do, as the programmer. Your model has just as much help from the modeler as mine does.

>I want to discover good generalizations that can be implemented not >in one specific case but much more generally. That seems to be >where our interests diverge, as you indicate:
>
>>>Thus far, you have been pretty unclear about when it would serve
>>>the larger scheme of things (successful control) to collect and
>>>store knowledge about the world, and what exactly the impact of
>>>such knowledge might be on the quality of our behavior. That >>>issue needs to be resolved, I think.
>
>>I don't really care about that problem because I don't know enough
>>about higher-level processes in human beings to speculate about >it. I know this is a big philosophical deal to you, but it isn't to >>me.
>
>It's not only philosophical. It's extremely practical as well. >Given a "system to be controlled" that has only partly predictable >behavior a priori, how can we discover -- in the most general sense >-- its more specific, individual "behavioral laws" in real time, >such that subsequent control can be better.

You can't claim the high ground that way. You have certainly not solved this problem "in the most general sense." The difference between us is that I KNOW I can't solve it "in the most general sense" and therefore don't waste my efforts trying. You might LIKE to solve it "in the most general sense," but you're no more capable of doing that than I am. Stating lofty goals doesn't by itself make you able to achieve them.

>>> ... How to adjust the pre-compensation TF1? By the usual
>>>mechanisms of random trial-and-error and hill-climbing, I
>>>guess. For the sake of efficiency preferably the latter.

Yeah, but where's the mechanism in your model for DOING that? All you're doing is describing what needs to be done. That doesn't offer any solution. It's easy to propose something like pre-compensation, but you haven't justified it if the mechanism is just "whatever is required to make it come out that way."

>>That answer is sufficiently vague that it could be applied to any
>>model at all. Conspicuously lacking is any attempt to say what >>sort of mechanism would DO this hill-climbing. In the Extended >>Kalman Filter model, at least you supply a specific mechanism to >>do one interesting part of the job. That mechanism will not handle >>the discovery and adjustment of

the required form of TF1.
>
>Why not? If the system is (sufficiently) linear, the EKF approach
>would be able, for instance, to discover the system's impulse (or
>step) response.

Look, you're just talking about something you think COULD be done. The model you presented doesn't have that something in it. You're alluding to a model you haven't developed yet. I accept your EKF model as a valid model, as far as it goes, because you actually worked it out and got it to operate. Perhaps you had to put things in it that the model itself didn't generate, but at this stage of the game we all have to do that. The important thing is that you demonstrated the mechanism you're talking about. You're not doing that with respect to the series TF1 compensation.

Best,

Bill P.

Date: Mon, 16 Dec 1996 12:54:13 -0700
From: Bill Powers <powers_w@FRONTIER.NET>
Subject: Re: simultaneity

[From Bill Powers (961216.1045 MST)]

Hans Blom, 961216b --

>Semantic problems again?
>
>>You say that "in your experience" flipping a light switch causes a
>>light to turn on and off. Is that not a perception?
>
>I would not call it a perception but (a certain type of; causal)
>relationship between an action ("flip the switch") and a perception
>("light on/off"). It is not the _perception_ of flipping the switch
>that I relate to light on or off but the _action_.

Yes, but how do you know about the action, except through feeling and seeing it?

Best,

Bill P.

Date: Mon, 16 Dec 1996 12:54:16 -0700
From: Bill Powers <powers_w@FRONTIER.NET>
Subject: Re: model-based tradeoff

[From Bill Powers (961216.1050 MST)]

Hans Blom, 961216d --

After vehemently insisting that my comments (and Rick's) on prediction are wrong, you say:

>It is the other way around. If the model is told that the "laws of

>the world" do not change, and they indeed do not, it can provide
>accurate long-term predictions. If the model is told that the "laws
>of the world" do not change, yet they do, it still will provide
>long-term predictions, but these will of course be completely
>incorrect. Finally, if the model is told that the "laws of the world"
>change rapidly, yet they are constant, it will not provide accurate
>long-term predictions -- although in principle it could.

That is exactly what I was talking about. There is a tradeoff. Either you tell the model that the world changes rapidly or that it changes slowly. In the first case, the model can predict short-term changes but not long-term ones. In the second case, it can predict long-term changes but not short-term ones. As you just said above.

The real world, of course, involves both long-term and short-term changes, as in the case of the weather or the stock market or driving on a windy day. If you want to predict a long way ahead, you have to ignore the short-term changes in order to derive the long-term trends. If your system is to predict short-term changes, it can't average them out.

Of course you could have TWO systems, one making short-term predictions and the other long-term ones based on the same input data.

Unfortunately, predicting disturbances entails assuming regularities of a very restricted kind, not like encountering icy patches on the sidewalk or slippery spots under the mouse, or the presence or absence of a pedestrian just as you turn your car around a blind corner. Disturbances come and go; they don't just vary in a nice Gaussian distribution. You can't predict what the crosswind will be as you emerge from a tunnel. You can't predict when the phone is going to ring. You can't predict that your keys will snag on a loop of thread as you pull them from your pocket. You can't predict how heavy a Christmas package will be when you pick it up. You can't predict what you'll have in your hands when the cat jumps into your lap. You can't predict your own errors of prediction.

And you can't achieve good control if you simply treat these unpredictable variations as "uncertainty" and deal with them in terms of means and standard deviations. Real control requires dealing with each disturbance as it arises, and as it varies from moment to moment. A PCT model can do that; an EKF model tuned for medium to long-term prediction can't. It's as simple as that.

>>I don't think this problem is peculiar to the Kalman filter
>>approach; it's inherent in the nature of prediction.
>
>Prediction is possible only if there are regularities that you can
>rely on.

Yes, that's what I said, wasn't it?

>>The accuracy of a prediction depends on the length of the sample you
>>work with. If you want to predict successfully for a long time
>>ahead, you have to use a long sample, and you have to count on the
>>characteristics of the world remaining constant during that sampling
>>time as well as during the prediction time.
>
>It is the latter, not the former, that is important. The "length of
>the sample you work with" must be enough to let you estimate the
>model parameters. Basically, the number of measured variables must be

>(at least) as large as the number of independent parameters of the
>model. Equal numbers would allow us to solve N equations in N
>unknowns. A larger number of observations allows some noise or
>uncertainty elimination in that it allows a "best" curve fit.

Yes, that's exactly what I'm talking about. The world has to retain the same characteristics both during the sampling time and the prediction time; if it remains the same while you're making your estimate, but begins to change immediately afterward, your estimate will be no good. Ditto if the world is variable during the sampling time and constant after it -- or even if the manner of variation is not the same before and after.

A "best fit" is not what we need for control. The average crosswind is zero; you have to deal with the crosswind that exists THIS time, not its average characteristics.

>>I think this tradeoff is a natural aspect of any prediction scheme.
>>Either you predict the details very accurately for a short time into
>>the future, or you predict the long-term trends accurately at the
>>expense of short-term accuracy. I don't believe you can do both at
>>the same time.
>
>It is impossible to predict individual noisy observations. It is,
>however, frequently possible to accurately predict long-term trends,
>whatever your belief in the contrary.

I can't agree with that. The best predictions are for a medium term, although there are exceptions (as in predicting satellite orbits, where disturbances are few, small, and mostly known). I realize that vast sums of money are spent in predicting long-term trends, but the claims of accuracy are more like justifications for spending the money than reports of the actual results. Something always comes up. "My prediction of inflationary trends was perfectly accurate, but how could I have known in advance about the Middle-East oil embargo?" Something ALWAYS comes up, but you never know what it's going to be. The butterfly effect reigns.

I think your logic is flawed, Hans. You have settled on a model that requires prediction. You realize that if this model is to work, the world must be mostly predictable. But instead of looking objectively at the world and concluding that the model is inappropriate, you reason that since the model MUST work, the world MUST be predictable to the extent required by the model. This means that you must shrug off example after example of the variability of the world, the unpredictability of the world. You simply insist that the laws of nature remain constant, so the world must be predictable.

The world is quite lawful; I have no problem in agreeing with that. But it is also monstrously complex, and local conditions are always being affected by hidden or remote conditions of which we know and can know nothing. How could I have predicted that a forty-year-old man would decide to go out to lunch ten minutes early, and thus pop suddenly out of a doorway into my path so I'd have to step aside to avoid a collision? How can I be prepared to mop up the place on the carpet where the cat is going to throw up two minutes from now? How can I predict dropping the house keys as I try to unlock the door, so I will have to bend down down pick them up? Did I plan all that ahead of time, both dropping the keys and bending toward the place on the ground where they bounced off the steps? When I'm reading a book, do I predict that my hand is going to wobble a bit, and plan out the eye movements that will compensate for the movement of the page? When I pull a

piece of Kleenex out of the box, does my model include the fact that the next piece is folded over and won't pop up correctly, and have all ready the movements that will extract the next piece from the box? And if I don't have that folded piece built into my model, does that mean I have to throw the whole box of Kleenex away?

>>In "challeng5" you adjusted the Kalman filter computations to
>>correct essentially all the prediction error for the disturbance in
>>a single iteration of the program, in effect extrapolating one
>>iteration ahead. As a result, when input was cut off the errors of
>>prediction started growing very rapidly on each successive
>>iteration, and there was essentially no long-term prediction
>>capability.
>
>I don't know which simulation you have in mind here. But if you look
>more closely you will see that, when the input was cut off, the
>action became pretty regular -- after a transition period. This
>indicates that the model parameters were pretty stable as well, and
>that the prediction error did not grow without bound.

You're treating this as a thought-experiment and telling me what you imagine the model must have done. Nothing at all like what you say happened. When the input was cut off, the adjustments of the modeled disturbance ceased, and it became constant. The real disturbance kept right on changing, so the model deviated from the reality, very quickly, within limits set only by the limits of the real disturbance. The simulation is the one called "challeng5.pas", which you wrote to compare your model with mine.

>>While I can't untangle the complexities of your computations in
>>detail, I'm quite sure that the result of this rapid response is to
>>convert your "model-based" system into a simple closed-loop feedback
>>control system witb a slight amount of first-derivative feedback
>>(the one-iteration extrapolation).
>
>Incorrect. The extrapolation (it is exactly that!) is based on much
>more than just first derivatives. It is based on everything that the
>model has learned about the regularities that link its actions with
>the ensuing perceptions of the results of those actions.

What model are YOU talking about? There is no such facility in Challeng5.pas. When the input was lost your model ceased to extrapolate the disturbance, or if it did, it did so incorrectly.

>>Suppose that the disturbance had been a slow sine-wave, a regular
>>waveform with a period of several hundred iterations of the
>>program. To take advantage of this regularity, you could now use a
>>sine-wave generator in the world-model, adjusted appropriately in
>>frequency, phase, and amplitude, to model the disturbance. To
>>achieve accuracy it would be necessary to sample over at least one
>>full cycle, so as to establish the required frequency and phase.
>
>No. A sine-wave has 3 parameters, so 3 measurements would be the
>minimum.

You're assume that you know in advance that it's going to be a sine-wave.

>Three _independent_ measurements, that is; in practice: not
>too close together, depending upon noise, computational accuracy and
>the phase of the sinus. Adaptation can be much faster than you

>suggest, but depends on circumstances. If the (slow) sine-wave
>disappears when it crosses zero, discovery of its amplitude going to
>zero will be much slower than when the sine-wave disappears at a
>maximum excursion of the sine-wave. In the first case, it will take
>some time before the prediction error starts to deviate from zero; in
>the latter case, the discovery may be essentially instantaneous.

Not if the sine-wave simply becomes a constant at the amplitude that existed
at the moment of change. You're imagining that the sine-wave suddenly drops
to zero, which is not the case I proposed, if you will read what I said. I
said that the sine-wave suddenly becomes a constant value. I said that
specifically to exclude your "essentially instantaneous" adjustment of the
model (which is exaggerated anyway).

>>However, if the perceptual input were lost, a _simple one-level_ PCT
>>model would immediately fail, while the disturbance being simulated
>>in the model-based system would continue for some time, perhaps
>>hundreds or thousands of iterations, to match and nearly cancel the
>>real sine-wave disturbance. The model-based system is the clear
>>winner in this case.
>
>Correct.

Ah. NOW you agree with me.
>
>>But consider what will happen with the perceptual input intact if
>>the real sine-wave disturbance suddenly ceases to change and becomes
>>constant.
>
>What did we tell the model? That the "laws of the world" are
>constant? In that case it will not believe that this can happen. That
>they (in this case: the sine-wave's amplitude) are mostly constant
>but can change suddenly and unpredictably? In that case it needs
>perceptions in order to be able to detect when those changes occur.
>No perceptions, no detection of a new situation that comes about at
>an unpredictable time, of course. Give it perceptions and it will
>rapidly readjust -- if the knowledge handed it from the higher level
>is correct.
>
>>The PCT model, which does not predict, simply adjusts its output as
>>usual to cancel the effect of the now-constant disturbance.
>
>If it cannot perceive?

No, please try to follow my argument. I am now dealing with the case where
perception is intact, but the nature of the disturbance suddenly changes.
Your model, as you explain above, will take a considerable time to adjust
its modeled disturbance to agree with the new phase, frequency, and
amplitude, while the PCT model will adjust immediately.

>>It continues to contain the same small error. The world-model based
>>system, however, continues to produce the same sine-wave modeled
>>disturbance, so it now is generating output to cancel a non-existent
>>external disturbance.
>
>Only if it cannot discover this. It appears to me that you compare
>apples and oranges here: a PCT controller that can perceive and a
>model-based one that cannot.

No, I'm dealing here with the case where BOTH models can perceive. I'm saying that the PCT model, which does not depend on modeling the sine-wave disturbance, will perform much better than the model-based model, which will require a considerable time to discover that the disturbance has changed and alter its world-model accordingly. While it's in process of discovering the change, it will continue to produce output to oppose the non-existent changes in the disturbance. It will _create_ errors until it gets its model adjusted.

>
>>Do you consider this analysis to be the result of "uninformed
>>misunderstandings"?
>
>Well, conclude for yourself...

I have. My analysis stands. Now that you understand what I was talking about, do you agree?

Best,

Bill P.

Date: Mon, 16 Dec 1996 12:58:20 -0700
From: "T. B. Harms" <tbh@TESSER.COM>
Subject: Re: Aristotle and Contingent Truths

*From Tracy Harms 1996;12,16.13:00

Peter Cariani (961216.1430)

>Popper also had the handicap of trying to be a realist and to do epistemology
>at the same time -- it just doesn't work.
>
>Peter

This implies that you either reject realism, or you reject understanding knowledge. (Or, I suppose, both.)

How might you elaborate the impossibility you have in mind? What might help me share your understanding of a conflict between realism and epistemology?

Tracy Bruce Harms tbh@tesser.com
Boulder, Colorado

"The clear and present danger we face is that those who battle against authoritarianism often themselves become authoritarian. But authoritarianism in any guise prevents improvement--our own intellectual, moral, aesthetic improvement, and the improvement of the social, political, and economic institutions of our society. So if we seek improvement we must not only combat authoritarianism, we must do it in a nonauthoritarian way."
` Henry J. Perkinson

Date: Mon, 16 Dec 1996 13:46:03 -0700
From: Bill Powers <powers_w@FRONTIER.NET>
Subject: Re: single top-level goal?

[
From Bill Powers (961216.1300 MST)]

Hans, your post came through like this:

```
>begin 644 CONFLIC2.ART
>M_U=00VL"```!"@`!`````/O_!0`R``4!```)``(```!"````!P`1````1```
>M``\`5@```%4````,`%H```"K````4[M#;W5R:65R(#$P(%!I=&-H````__^6
>M`#(`>`!X`'@`"@`!`````+DK6`)X`!0>#!>,"@````210,D`A\\!``$`6`)`
>M_____O_____4W1A;F1A<F0@
>M4')I;G1E<B!T;R!!4T-)22Y!4T,``````````%-404Y$05)$+E!24P#;`7@`
```

etc.

Best,

Bill P.

Date: Mon, 16 Dec 1996 13:48:42 -0700
From: "T. B. Harms" <tbh@TESSER.COM>
Subject: Re: model-based tradeoff

*From Tracy Harms 1996;12,16.13:45

Bill Powers (961216.1050 MST)

>Unfortunately, predicting disturbances entails assuming regularities of a
>very restricted kind, not like encountering icy patches on the sidewalk or
>slippery spots under the mouse, or the presence or absence of a pedestrian
>just as you turn your car around a blind corner. Disturbances come and go;
>they don't just vary in a nice Gaussian distribution. You can't predict what
>the crosswind will be as you emerge from a tunnel. You can't predict when
>the phone is going to ring. You can't predict that your keys will snag on a
>loop of thread as you pull them from your pocket. You can't predict how
>heavy a Christmas package will be when you pick it up. You can't predict
>what you'll have in your hands when the cat jumps into your lap. You can't
>predict your own errors of prediction.

And of special importance: you can't predict the disturbances which will result as unintended consequences of your own behavior.

Tracy Bruce Harms tbh@tesser.com
Boulder, Colorado

Date: Mon, 16 Dec 1996 12:52:48 -0800
From: Richard Marken <marken@AEROSPACE.AERO.ORG>
Subject: Role of input, conflict

[From Rick Marken (961216.1300)]

Tracy Harms (1996;12,15.14:43) --

>When Popper talks of attempts to trace our knowledge to our observations he
>refers to the origination of knowledge. He does not reject the contribution
>of input (including observation), he simply insists that its contribution is
>restrained to a *critical* one. That is, it allows error to be culled. But
>in this neo-Darwinian role input cannot be mapped to increased fit by
>anything resembling a *function*.

Now _that_ is something with which I can enthusiastically agree. I'll go back to being my usual warm, cuddly self now;-)

Bruce Gregory (961216.1340 EST) --

>Think of how simple life would be if we asked whether some action might lead
>to conflicting goals _before_ we acted!

The problem is that in many (possibly most) cases the conflict can't be predicted. Goals might turn out to be in conflict even though they don't seem to be. For example, suppose you want to drive to the store to get your wife a treat; you have two goals: 1`) driving to the store and 2) making your wife happy -- and there appears to be no conflict. What you don't know is that your wife has a hair appointment and that she is planning to use the car to get there. So now you find that when you get into the car to go get the treat, your wife is not getting happier; she's getting madder. This is an easy conflict to solve, of course; but it shows that you don't necessarily know that your goals are in conflict until you start trying to produce the perceptions specified by those goals.

I think it's probably impossible to learn to avoid conflict with any degree of reliability. Conflict just goes with the territory; it's the price we pay for our autonomy;-) But I think we can learn how to reliably solve conflicts --using the method of levels--once we notice that we are _in_ them; and just being able to tell that we are in a conflict is often something of an achievement in itself.

Best

Rick

Date: Mon, 16 Dec 1996 13:40:59 -0700
From: "T. B. Harms" <tbh@TESSER.COM>
Subject: Re: Decision

*From Tracy Harms 1996;12,16.13:40

Rick Marken (961216.1010)

>The only way to solve a conflict is to change the goals that
>are creating the conflict. And the only way to change the goals that are
>causing teh conflict (I think) is to see the conflict from a point of
>view where the conflict producing goals are seen as _options_ rather
>than _requirements_.

I don't suppose anybody has yet attempted to model the difference between seeing-as-optional and seeing-as-not-optional, have they?

Tracy Bruce Harms tbh@tesser.com
Boulder, Colorado

Date: Mon, 16 Dec 1996 15:44:36 EST
From: Bruce Gregory <gregory@CF4.HARVARD.EDU>
Subject: Re: Aristotle and Contingent Truths

[From Bruce Greory (961216.1545)]

Tracy Harms 1996;12,16.13:00

> This implies that you either reject realism, or you reject understanding
> knowledge. (Or, I suppose, both.)
>
> How might you elaborate the impossibility you have in mind? What might
> help me share your understanding of a conflict between realism and
> epistemology?

Peter can speak for himself. Here is why I agree with him. If, as Popper maintained, our scientific knowledge of the world gets closer and closer "to the way the world really is," it seems that there must be some privileged position outside our knowledge and outside the world from which the realist observes the narrowing distance between the two. This super-epistemological position creates more problem than it solves. It is much cleaner to say that our theories grow in predictive ability rather than to say that they increasingly correspond to the way the world is. The former is demonstrable, the latter is not.

No one outside of an institution doubts that there is a real external world, but only God knows if our theories "correspond" to that world. And She isn't saying...

Bruce Gregory

Date: Mon, 16 Dec 1996 16:10:13 EST
From: Bruce Gregory <gregory@CF4.HARVARD.EDU>
Subject: Re: Role of input, conflict

[From Bruce Gregory (961216.1610 EST)]

Rick Marken (961216.1300)

> I think it's probably impossible to learn to avoid conflict with any degree
> of reliability. Conflict just goes with the territory; it's the price we pay
> for our autonomy;-) But I think we can learn how to reliably solve conflicts
> --using the method of levels--once we notice that we are _in_ them; and
> just being able to tell that we are in a conflict is often something of an

> achievement in itself.

I think it's a _key_ achievement. Your situation is hopeless until you can (1) see that a conflict exists and (2) start looking for the incompatible goals.

By the way I was thinking of the internal conflicts we generate for ourselves by unthinkingly adopting incompatible goals. Your point is taken, nevertheless.

Bruce Gregory

Date: Mon, 16 Dec 1996 17:12:30 +0000
From: Peter Cariani <peter@EPL.MEEI.HARVARD.EDU>
Subject: Re: Aristotle and Contingent Truths

T. B. Harms wrote:
>
> *From Tracy Harms 1996;12,16.13:00
>
> Peter Cariani (961216.1430)
>
> >Popper also had the handicap of trying to be a realist and to do epistemology
> >at the same time -- it just doesn't work.
> >
> >Peter
>
> This implies that you either reject realism, or you reject understanding
> knowledge. (Or, I suppose, both.)
>
> How might you elaborate the impossibility you have in mind? What might
> help me share your understanding of a conflict between realism and
> epistemology?
>
> Tracy Bruce Harms tbh@tesser.com
> Boulder, Colorado
> ---
> "The clear and present danger we face is that those who battle
> against authoritarianism often themselves become authoritarian.
> But authoritarianism in any guise prevents improvement--our own
> intellectual, moral, aesthetic improvement, and the improvement
> of the social, political, and economic institutions of our
> society. So if we seek improvement we must not only combat
> authoritarianism, we must do it in a nonauthoritarian way."
> ` Henry J. Perkinson

I could not agree more. As Nietsche said, "Those who fight monsters should be very careful not to become them." But I think this is partly what happened to Popper. He wants to be critical, to make things subject to contradiction by empirical evidence (falsifiability) as much as possible. I definitely agree with this striving for critical interaction between theory and data. But as a realist, he thought that there is Absolute Truth, a right way and a wrong way, and it's not hard to see how the arrogance of believing that a particular method is the royal road to truth

leads to scientific tribunals that will judge what is true and what is not. From moral absolutism comes the guillotine. From realism comes the tribunal that judges scientific truth. Feyerabend argued strenuously against Popper on these grounds: that paradoxically, Popper's belief in realism leads him down a totalitarian road that contradicts his other, anti-totalitarian impulses.

There were and are others (I count Feyerabend and van Frassen and Bohr and Bridgman and Mach) who held operationalist positions about the verifiability of particular models (sets of observables) without holding that such models give us access to some unchanging Truth or Reality beyond our observables. I grant that there is a material world out there, but neither I nor anyone else has access to what its 'true' form might be. We only make contact with limited portions, aspects of that realm. It's undifferentiated until we make a measurement or have a percept, but the outcome of the measurement/percept is only one small aspect of the (ill-defined) whole. The only realms in which we have access to an entire "universe" are ones that we construct with symbols and rules, finite mathematical systems, the "toy" micro-worlds beloved by symbolic AI. This world of symbol is the only realm in which "true" and "false" (or "same" and "different") have precise meaning. Our scientific models allow us only partial access -- we convert the pointer readings into symbols with definite values that we as communities of observers can agree on reliably, and then we decide if the symbols derived from observations match those derived from predictions derived from other observations (the Hertzian commutation relation). Whether they are the same or different tells us if our model is "correct" or incorrect in its predictions, but this says very little about the form of the world outside of the symbols. These models are very useful to us -- they allow us to predict the outcomes of measurements -- but they do not give us any kind of unambiguous access to the world beyond the pointers.

Realists, if they are optimists, think that we are coming ever, asymptotically closer to some fixed and eternal Truth. The kinds of pragmatists that I invoke (above) would maintain that our theories and models are constructed for particular purposes and can only be evaluated in the context of those purposes. One can test models, say for how much loading a bridge of a certain design will handle, and such models can be falsified certainly (bridges fall down), but can you compare such a model in any way with a model for how hummingbirds fly? It's even difficult to compare models that are trying to predict the same sorts of things (say whether a cancer will respond to a given treatment) if the basic sets of observables are different (different types of diagnostic tests, different types of cancer).

So, I reject realism. I'm an agnostic. There is no absolute or true knowledge of the world outside of our senses. We use the information that we have from our senses and measuring devices as best we can, and we do improve our performance with time (because there is a selection process going on). I don't even know what "realism" is supposed to mean. I

grew up in Memphis, in the heart of the bible belt and
the fundamentalists were always talking about "literal"
interpretations of the bible (what does it mean in the
end?). What is a literal interpretation of a theory or
model such that it takes you closer to some sort of
Absolute Truth? The concept of literal truth, whether
applied to scientific theories or biblical exegesis,
is incoherent.

I'm neither an optimist nor a pessimist. Good does not
always win out. Better does not always win out. We may
still be plunged into Dark Ages. In my lifetime I've
seen whole paradigms destroyed and/or neglected by those
that dominated (for example cybernetics and symbolic AI).
The dominant paradigm prevailed for bad reasons (the
persuasive power of the up-and-coming digital computer
technologies), but that didn't stop its proponents from
choking off funding to all of the alternatives.
Anyone who has ever had ideas that are sufficiently different
from mainstream opinion knows how fragile the
existence of real alternatives truly is, especially
when there is money or power at stake. Often enough it doesn't
matter at all whether the alternatives are better.

Realism presupposes an ontological stance, that in some way
we can have "true" knowledge of all the processes that make up the
world. In doing so, the realist momentarily takes the role
of the omniscent observer, a God's-eye view. This point of
view, however, is incompatible with the notion of a limited
observer who can be deceived by the appearances of things.
On one hand realism maintains that this omniscient perspective
is possible vis-a-vis the material world (so as to pass
judgement on which theories are closer to the truth than
others); on the other, we ourselves are obviously limited
beings who are constantly bumping into things and who do
not have some sort of privileged access to the world beyond
our senses. How can one maintain these two incompatible
perspectives at the same time, that of limited observer
and omniscient god?

Pragmatism begins with the limited observer and examines the
ways (adaptive mechanisms, variation and selection) that
limited beings are able to gain greater influence (dare I
say control?) over their surrounds. Epistemology is not
the study of how "true knowledge" is acquired; it is the
study of how organisms and devices are able to acquire
pragmatic knowledge, to better adapt to the world, to
improve performance, to achieve goals. No gods. No
orphic mystery cults. No "best seat in the house."
Just good clean fun.

I knew I shouldn't have mentioned Popper......

Over & out,
Peter

Date: Mon, 16 Dec 1996 01:52:58 PDT

From: CCHERPAS <CCHERPAS@CCCPP.COM>
Subject: Re: Early language learning; music; rave; Zanotti

[From Chris Cherpas (961216.1203)]
[re: Peter Cariani ((961213.1700 EST)]

I can't cite any controlled experiments, but I believe my kids' (now 2 & 4) earliest, robust proto-language sequences seemed to favor parts of songs over other "word" sequences. Some people claim that you can train rhythms even to the unborn (not speaking karma here folks, just embryos), but I haven't seen their test for the putative controlled variable.

[rave on]
Others seem to think the "poverty of the stimulus" provides an insight into a wonderful, specialized mind/brain language machine. I prefer to think that some of the problems of _real_ poverty could be overcome through early language (and everything else) education -- provided for every kid by taxing the rich beyond their wildest nightmares.
[rave off]

Finally, to the first ten "callers," I will send a copy of the final chapter of Suppes' & Zanotti's (1996), "Foundations of Probability with Applications." Send your snail-mail address to ccherpas@cccpp.com if interested in probabilistic models and methods applied to computer-based education.

Best regards,
Impoverished Stimulus

Date: Mon, 16 Dec 1996 20:01:57 +0000
From: Peter Cariani <peter@EPL.MEEI.HARVARD.EDU>
Subject: Re: Auditory temporal structure

[From Peter Cariani 961216 20:40]

(These levels of nestings get unwieldy).
> [Martin Taylor 961216 10:40]
> >Bill Powers (961214.0600 MST)
> >
> > Peter Cariani ((961213.1700 EST)--
> >
> > >When we hear running speech
> > >there are time patterns corresponding to the acoustic transients
> > >in the speech waveform and their neural correlates can be found
> > >at all levels of the auditory system (i.e. large numbers of neurons
> > >are following the slower modulations in the waveform).
> > >Each one of these syllables has a characteristic
> > >temporal structure (rhythm, onsets and offsets of vowels, etc).

> Once you get to pitch, and
> even more so when you get to phonemes and phoneme transitions, you are
> well beyond the level about which Peter is talking. He's talking about
> a phenomenon that has been extensively studied by Roy Patterson and
> his colleagues at the MRC Applied Psychology Unit in Cambridge.

I wasn't specific enough about the click trains I was playing. I allowed for a Poisson process in which there were 5 clicks/second average

arrival rate for however many seconds. The pattern would then repeat.
So we're actually talking about the time regimes of sequences of
phonetic events here. These kinds of things (repeated noises, repeated clicks,
etc)
have been studied since the 1940's (at least).

> > When you look at neural correlates of acoustic patterns, if you can trace
> > the correlations between the sounds and the neural signals you aren't
> > looking at the outputs of the pattern recognizers, but only at the inputs
> > which have not yet reached these recognizers. You won't have found the
> > temporal pattern recognizer until you find signals that are present when the
> > pattern is present, absent when it is absent, and remain _constant_ as long
> > as the same temporal pattern is going on.
>
> These are the characteristics of Patterson's "Auditory Stabilized Image",
> his model of the neural structure that recognizes patterns. If he is right,
> then there is a place, quite early in the auditory system, where the
> signal depends precisely on the temporal patterning _as a human perceives
> it to be_. His model is computational, and the "ASI" can be displayed on
> a screen. He claims that all variations that a human can detect in an
> auditory pattern can be seen as variations in the screen display, and
> all variations that can be seen in the screen display can be heard as
> variations in the auditory pattern. Same criterion as is used for testing
> simulation control models against human performance.
>
> > What you're really
> > looking for is a place in the nervous system where there's a signal that
> > corresponds to your own _sense of pattern_. That is, this signal should be
> > present while you're experiencing a certain pattern-ness in the sound, and
> > absent when you're not. It should most definitely NOT covary with each
> > transient feature of the pattern, either in the sound or in the neural
firings.
>
> Right. That's what the ASI is claimed to do.

The kind of correlational analysis that I'm interested in is very much in line
with that of Roy Patterson's, although I think that he is thinking of this more
in terms of peripheral representations, whereas I think that these kinds of
representations could be analyzed more centrally. Patterson, as far as I know,
doesn't take the time window way out to cover tens or hundreds of milliseconds
and the running time patterns of whole phonetic elements and syllables. I think
we need to do that and make recognizers for those patterns. Never been done
as far as I know, although see below.

> > >So it's not necessarily baffling that we hear these things.
> > >Our auditory system is made to detect recurring time patterns.
> > >A temporal memory trace and a means of cross-correlating the
> > >incoming (neural) time pattern with what's stored is all you
> > >need.
> >
> > That sounds easy, but it's not that easy.
>
> No. If it had been easy, other people would have done it long ago.

Well as it happens, people did do it long ago. I have a book on correlational
analysis from the mid-1960's and it reviews various kinds of analyses that
were done in the 1950's-- heyday of auto- and cross-correlation, before
digital signal processing. There were analog autocorrelographs that made
running autocorrelations (I have one on my wall of the sentence "He beats his

head against the posts", which nowadays takes on new meaning). Licklider and Cherry had proposed various binaural hearing models based on similar kinds of operations (but I think they lacked the computational power to realize them). Papers were written on autocorrelation patterns of consonants.

I think it's not a matter of being easy or hard, it's what you're trained to think about and how you think about things. All the signal processing people who have come up in the last 30 years have been trained to take an FFT first and ask questions later.

> > The word you remember must be running at the same speed
> > as the word you're hearing; otherwise even if you start the memory-word at
> > the same instant the real word starts, they'll be out of synch before the
> > word ends. And of course the words must be spoken and remembered with
> > identical pitches and inflections. When you think about actually building a
> > cross-correlator that will work with continuous speech, the project becomes
> > considerably more complex than it seems when you just describe what it's
> > suppose to accomplish.
>
> This is quite a different issue. To bring this in is like saying that
> a sequence perception has to be done at the transition level. Exactly
> the same issues arise. The recognition of a word has very little to do
> with the pitch or inflection or speed (something to do with each, but
> not much) of the sound pattern. Most computerized recognizers start
> with some analysis of the spectrum and then go with the sequence of
> spectral patterns identified (perhaps tentatively). Patterson's ASI
> might be a better place to start, but it's computationally very expensive.
> Easy to do in a neural system, hard in a computer (as is correlation).

The power of computing is fast approaching the point where these things are feasible.....

Peter

Date: Mon, 16 Dec 1996 19:09:18 -0600
From: Bruce Abbott <abbott@CVAX.IPFW.INDIANA.EDU>
Subject: Re: Solving the "loss of input" problem

[From Bruce Abbott (961216.2010 EST)]

>Bill Powers (961211.1640 MST) --

>>Bruce Abbott (961211.1720 EST)

>>It seems to me that a PCT savvy control engineer facing the sort of problem
>>Hans is concerned about (with the controller working in an environment in
>>which the signal tends to vary in a way that can usually be predicted fairly
>>well in the short term) would design a system that would attempt to model
>>these systematic variations (as Han's system does) and use this model, not
>>to generate computed _outputs_, but to generate a computed substitute
>>_input_ to what otherwise would be an ordinary PCT-style controller.

>This won't work because you would also have to model the disturbances acting
>on the input. Normally you don't perceive the effects of disturbances
>because the output's opposing them. Also, there's a serious problem with the
>system's gain. With the feedback loop open, the output will be very
>sensitive to small changes in the input, so you'd have to simulate the input

>with great accurary to get the right output. Since all you want is the
>right output, it would be much more direct and appropriate to generate the
>output.

When steering my car I most certainly do feel the effects of disturbances; I can sense the pressure the steering wheel exerts against my hands as potholes and crosswinds push against the tires and car body and I can feel the lateral accelerations they produce (as well as those produced by my own control actions as I attempt to correct for these disturbances). To a fair degree I am able to separate out those effects and side-effects of my own control actions from those resulting from these less predictable effects.

As for loop gain, the feedback loop is NOT open; the system's normal input has been temporarily replaced by an artificial one; the system can't tell the difference, so it just goes on doing what it always does. What HAS changed is that the intended CV is no longer under control; instead, the controller acts to control the artificial substitute. If the substitute perceptual signal and the model of the EFF are perfect substitutes for the real thing, then by definition the control actions taken by the system will do to the actual CV what the system is doing to the artificial substitute, and the CV will appear to remain under control. Of course, no model will be a perfect substitute for the actual variable, and any differences between model and reality will fairly quickly build up (how fast depends on the fidelity of the model to what's really going on with respect to disturbances acting on the real CV).

But remember, when we say that we have a model, what we really have a model of is (a) the disturbance pattern and (b) the effect of the controller's actions on the CV. To the extent that the disturbance pattern is predictable, the model may be able to function reasonably well for some specific time-span (depending on the quality of the model). Unpredictable (i.e. unmodeled) disturbances cannot be controlled against unless (a) the state of actual CV can be sensed or (b) the disturbances can be sensed and their effects on the CV accurately estimated.

At any rate, my point in originally raising this issue was that Rick objected to Hans's model on the basis of its being a "computed output" system. I was simply attempting to show that a standard PCT-style controller (however you consider it to arrive at its output values) could be inserted as the primary system of an adaptive controller by using "computed inputs." The resulting system still works (in my estimation at least) the same way as PCT-style controllers always do, while continuing to take action against (imagined; modeled) disturbances when actual perceptual input is interrupted. Such a controller would continue to _appear_ to maintain control over the actual CV, while _actually_ maintaining control over the imagined CV, for so long as the modeled and real disturbances match reasonably well. Whether such a system is actually to be found in a human nervous system is an empirical question.

Regards,

Bruce

Date: Mon, 16 Dec 1996 20:05:56 -0500
From: Scott Stirling <scstirli@ANSELM.EDU>
Subject: In Defense of Popper & Harms

I would just add to this thread that I think Bruce Gregory is missing the point a bit, and T.B. Harms is being a bit coy. There is no need to posit an external reality toward which our understanding evolves--right? There is no way to ever prove that this world or my thought of it exists without stepping outside of the system. Once you've stepped out, you're in a new one that needs to be explained, ad infinitum, a la Godel and Hofstadter. Here we apply the serenity prayer...

All Harms and Popper need to say is that the perception we have of the world is remarkably consistent over time--even the new theories of scientific revolutions are consistent with what has come before (often revealing inconsistencies previously ignored, disregarded, or unknown)--and within this system of perceptions, the fit between the reasoning knower and the perceptible known (phenoumena and noumena are needless distinctions here) increases. This fit can be called knowledge, if we want, once we all agree on its meaning.

Predictability is not necessarily a criterion for anything since it is always possible that some fundamental unpredictability will be perceived as inherent in the system. Fit is the primary criterion of knowledge--predictability is not _necessarily_ a part of that.

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Date: Mon, 16 Dec 1996 22:15:04 -0500
From: Bruce Gregory <bgregory@CFA.HARVARD.EDU>
Subject: Re: In Defense of Popper & Harms

[From Bruce Gregory (961216.2215 EST)]

Scott Stirling December 16,1996 8:05 PM

> I would just add to this thread that I think Bruce Gregory is missing the
> point a bit, and T.B. Harms is being a bit coy. There is no need to
> posit an external reality toward which our understanding evolves--right?
> There is no way to ever prove that this world or my thought of it exists
> without stepping outside of the system.

I don't posit an external reality, I encounter it.

Bruce Gregory

Date: Mon, 16 Dec 1996 22:24:52 -0500
From: Scott Stirling <scstirli@ANSELM.EDU>
Subject: Re: In Defense of Popper & Harms

On Mon, 16 Dec 1996, Bruce Gregory wrote:

> I don't posit an external reality, I encounter it.

I do not intend to wrestle over matters of faith. Everyone is entitled to their beliefs.

Scott

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Date: Mon, 16 Dec 1996 21:07:20 -0700
From: Bill Powers <powers_w@FRONTIER.NET>
Subject: Re: Solving "loss of input" problem

[From Bill Powers (961216.2000 MST)]

Hans Blom, 961216e --

>>And of course none of these papers was concerned with how to apply
>>similar principles to the adaptation of other kinds of control
>>architectures like HPCT.
>
>HPCT, regrettably, neither has the same theoretical nor the same
>practical breadth, it seems. Why so much advance around Kalman's
>theory (since 1963) and so little around HPCT (since 1973)?

HPCT has been around, although not under that name, since 1960. The reason so many engineers have been interested in the Kalman filter is that it enables them to do lots of things (beside design control systems), and because it is taught to them in engineering courses. HPCT is not taught in any engineering courses that I know of. I woulod guess that all the control engineers who know about HPCT are on CSGnet. Your opinions about your model and its capabilities are among the kinds of reasons it is not taught. This is what I meant by the "advantages of incumbency."

>>Here's my point. If the same amount of labor had been put into
>>applying these adaptive methods to a PCT model, it would be much
>>easier to put aside the distraction of "adaptiveness" and simply
>>look at the basic organization being proposed.
>
>I'm sorry, Bill, but "adaptiveness" won't go away.

My goodness, you have a way of substituting your own words for mine. Read the above: "if the same amount of labor had been put into applying these adaptive methods to a PCT model ...". Does that sound as if I want "adaptiveness" to go away? My point is that if a similar amount of labor had been put into introducing adaptive properties to a PCT model, we could then compare the underlying achitectures without being distracted by the fact that PCT models, at present, don't have any general adaptive facilities.

>>The "modern control theory" approach is basically an attempt to
>>conform to a top-down, command-driven type of system in which the
>>point is to compute the right output.
>
>(H)PCT models _also_ need to compute the right output. You may object
>against the word "compute" because the underlying PCT mechanism is so
>simple, but what's the fundamental difference?

The fundamental difference is that the MCT model has to have a model that imitates the properties of the environmental link. The PCT model does not, nor does it need to compute the inverse of the environmental link. The world-model part is eliminated entirely, and the inverse is reduced to a function that is one order simpler than the differential equation of the environmental link. If the environmental link is an integator, its inverse would be a differentiator. But the PCT model (for this case) needs only a proportional gain in its output function, which is not the inverse of the environmental feedback function.

It's strange: there seems to be a little voice in my head telling me that your reaction to what I just said will be to say that there is still no fundamental difference between the PCT and MCT models. I feel as though I'm trying to talk in one of those acoustically dead rooms that Rick has mentioned.

>>That basic concept absolutely REQUIRES an internal world-model, and
>>also the ability to compute the INVERSE of the same world-model.

>Upon closer inspection, you will find the same in (H)PCT models. I'm
>sorry that you cannot see the correspondences between what is
>internal in "models" and the PCT notions of gain, slowing factor and
>such.

And I'm sorry that you can't see that there IS NO WORLD-MODEL in the PCT model that corresponds to the one in the MCT model.

>>Give us a world in which you will get 20,000 hits in looking up
>>"control of perception" and the status of the PCT model will be
>>greatly different.
>
>Another belief. When you get to that point, there may be something in
>it. Start to demonstrate a useful, real (not simulated) HPCT-based
>system and the number of articles will soar, I assure you.

How about my arm model? That's a simulation, to be sure, but a real model could easily, if not inexpensively, be built to test it. I think your comment smacks of "let them eat cake."

>>The well-developed approaches have all the advantages of incumbency,
>>which as we know have nothing to do with merit.
>
>Are you serious? Time-proven methods have no merit?

Time and numbers don't prove anything except how unimaginative people can be, and how easily they jump on bandwagons. I think you're forgetting one very important aspect of adhering to a widely accepted "time-proven" idea: you can get a job pursuing it, and you can get funds for doing research on it. This is a version of "the rich get richer and the poor get poorer." For that reason, a widely-accepted idea tends to suppress work on rival ideas, simply by hogging all the resources, and by allowing the proponents of the accepted idea to be the referees who decide what publications are likely to

advance the state of the art. THEIR art, of course.

Best,

Bill P.

Date: Mon, 16 Dec 1996 21:07:26 -0700
From: Bill Powers <powers_w@FRONTIER.NET>
Subject: Re: Early language learning

[From Bill Power (961216.2030 MST)]

Peter Cariani ((961216.1200 EST) --

>Just as the output of a filter is not only the magnitude of the output
>(like an rms of the signal coming out), but also the waveform of the the
>output signal, the output of a cross-correlator is not only a scalar
>"similarity" index, but the pattern of the output of the operation. The
>pattern also tells you what aspects of the two signals are the same.

The magnitude of the output of a filter represents the degree to which the input fits whatever the filter is designed to detect. But _variations_ in that output have no meaning until they reach another level of perception that is designed to detect those variations in terms of some other set of criteria. I can see how your statement fits in with your idea that a given perceptual signal contains all levels of information, which are passed on to higher levels. This is only a small step from the concept of a hierarchy of perception in PCT.

But it's a crucial step, because your assumption is that the information is in the signal, whereas I say it is created by the perceptual function that receives the signal. If you change the form of the perceptual function, you get a representation of a different aspect of the signal, a different pattern. You can't tell what a higher system will detect in a perceptual signal just by looking at the perceptual signal. You have to know the kind of function that receives it.

Your lovely citation from Ernst Mach is actually an illustration of what I mean. What matters is not what an external observer can detect in the sound. What matters is the tuning of the tuning forks inside the perceiving system. Aspects of the sound that do not excite the tuning forks, however salient they may seem to an observer, do not exist for the receiving system.

Best,

Bill P.

Date: Mon, 16 Dec 1996 21:07:29 -0700
From: Bill Powers <powers_w@FRONTIER.NET>
Subject: Re: Aristotle and Contingent Truths

[From Bill Powers (961216.2100 MST)]

Peter Cariani (961216) --

Your Declaration of Epistemology rings bells with me all the way through. A beautiful and eloquent statement.

I would change only one little thing to remove the last taint of naive realism. You say:

>Pragmatism begins with the limited observer and examines the
>ways (adaptive mechanisms, variation and selection) that
>limited beings are able to gain greater influence (dare I
>say control?) over their surrounds.

If you will just say "over their perceptions" the picture will be complete. If you want to speak purely, you will eliminate terms that make claims to be about that which we cannot know directly. All such claims beg the basic question of epistemology.

> Epistemology is not
>the study of how "true knowledge" is acquired; it is the
>study of how organisms and devices are able to acquire
>pragmatic knowledge, to better adapt to the world, to
>improve performance, to achieve goals. No gods. No
>orphic mystery cults. No "best seat in the house."
>Just good clean fun.

Again, there's that lurking claim to knowledge of "the world." The world that we come to know and learn to control is the world of perception. The "other" world is hypothetical, however attractive and useful that hypothesis may be. We can freely accept and use that hypothesis in our practical affairs, but when we are looking for a deeper understanding we have to identify it as what it is: a guess.

Best,

Bill P.

Date: Mon, 16 Dec 1996 21:07:32 -0700
From: Bill Powers <powers_w@FRONTIER.NET>
Subject: Re: In Defense of Popper & Harms

[From Bill Powers (961216.2010 MST)]

Scott Stirling (961216) --

>All Harms and Popper need to say is that the perception we have of the
>world is remarkably consistent over time--even the new theories of
>scientific revolutions are consistent with what has come before (often
>revealing inconsistencies previously ignored, disregarded, or
>unknown)--and within this system of perceptions, the fit between the
>reasoning knower and the perceptible known (phenoumena and noumena are
>needless distinctions here) increases. This fit can be called
>knowledge, if we want, once we all agree on its meaning.

I agree totally with this well-put proposition.

Best,

Bill P.

Date: Mon, 16 Dec 1996 21:46:32 +0000
From: Richard marken <marken@AERO.ORG>
Subject: Re: Decision

[From Rick Marken (961216.2040)]

Me:

> the only way to change the goals that are causing teh conflict
> (I think) is to see the conflict from a point of view where
> the conflict producing goals are seen as _options_ rather than
> _requirements_.

Tracy Harms (1996;12,16.13:40) --

> I don't suppose anybody has yet attempted to model the
> difference between seeing-as-optional and seeing-as-not-optional,
> have they?

When I talk about "seeing the conflict from a new point of view" I am talking about a consciousness phenomenon: something outside of the perceptual control hierarchy is "watching" the controlled inputs, reference inputs and, possibly, the reference outputs of control systems in the hierarchy. This something is, I think, consciousness.

Consciousness is not yet part of the formal HPCT model. At the moment, all we can do is experience it. But HPCT does suggest ways that consciousness might enter the behavioral picture. Imagine that consciousness can become aware of ("watch") one or another of the variables in a control loop. When consciousness is directed at the controlled perception we are aware of what we are "doing" (controlling); when it is directed at the reference signal we are aware of what we want; when it is directed at the reference output we are aware of what we are trying to do to get what we want.

It seems that consciousnes can be located _in_ only one (or a limited number of) control system(s) at a time. From the point of view the consciousness in one control system the controlled perception is just the way things are, the reference signal is a requirement regarding the way things _should_ be and the reference output is one of the options available for meeting the requirement set by the reference signal.

When I talk about viewing conflict producing goals as _options_ rather than _requirements_, I am talking about moving consciousness from the control system that gets the conflict producing goal sent to it as a reference signal to the control system that selects that conflict producing goal as one of its optional reference outputs.

For example, I am talking about something like moving one's consciousness from a control system that sees the practice of a particular religion as a requirement ("I just _am_ Catholic") to the control system that has selected the practice of this particular religion from among several options that could have been used to achieve a higher order goal (spiritual fulfillment, possibly).

Does this make any sense?

Best

Rick

Date: Tue, 17 Dec 1996 05:17:38 -0500
From: Bruce Gregory <bgregory@CFA.HARVARD.EDU>
Subject: Re: In Defense of Popper & Harms

[From Bruce Gregory (961217.0520 EST)]

Scott Stirling December 16,1996 10:24 PM
>
> On Mon, 16 Dec 1996, Bruce Gregory wrote:
>
> > I don't posit an external reality, I encounter it.
>
> I do not intend to wrestle over matters of faith. Everyone is entitled
> to their beliefs.

If I knew you better, I'd punch you in the nose and ask you to
posit that I hadn't ;-)

Bruce Gregory

Date: Tue, 17 Dec 1996 05:23:09 -0500
From: Bruce Gregory <bgregory@CFA.HARVARD.EDU>
Subject: Re: In Defense of Popper & Harms

[From Bruce Gregory(961217.0525 EST)]

Bill Powers (961216.2010 MST)

>
> Scott Stirling (961216) --
>
> >All Harms and Popper need to say is that the perception we have of the
> >world is remarkably consistent over time.
>
> I agree totally with this well-put proposition.

So you've joined Scott in positing a conistent world of perception.
Nothing more homey than a pair of solipcists. Don'y get me wrong,
some of my best friends are solipcists ;-)

Bruce Gregory

Date: Tue, 17 Dec 1996 11:27:52 +0100
From: "J.A. Blom" <J.A.Blom@ELE.TUE.NL>
Subject: resend: single top-level goal?

---------- ASCII.ASC follows ----------
[Hans Blom, 961216g]

On a recent visit of Martin here, he remarked that, to him, my most troublesome concept was the blanket statement that there is always "one uppermost goal" that is being pursued by a controller (or organism). My reply was, that is always mathematically possible to express a number of separate goals as one combined "supergoal". Martin's reply was that -- if I may paraphrase -- we might not be able to recognize such a "super-goal" as a _causal_ goal in the PCT sense. Martin may be right, and this post investigates the matter. The "supergoal" might be recognizable as a PCT-type of goal only if there are no conflicts, i.e. if all goals are independently realizable. In that case, the overall error (supergoal "reference" minus supergoal "perception") can go to zero. Not so if there are conflicts.

It appears that the groundwork for this post was already laid in my post [Hans Blom, 921022], which I now repeat in its entirety. Sorry for those who have this post fresh in mind :-). At the time, it prompted little feedback -- and little resistance. We only need to "invert" the argument to verify Martin's doubts. Martin, you are right! There _are_ problems with this concept.

My 921022 post: On conflict

Suppose that someone requests from you that you execute a task with two incompatible goals, and suppose that you go along with the request -- just for the fun of it -- to see how you might react. As an alternative, suppose that TWO people have incompatible requests, both of which you would like to satisfy as far as is possible. Or suppose that someone poses a request that is incompatible with YOUR OWN desires. Let's ana-lyze this situation.

Take a very simple example. You are driving on a straight road. One request is to drive one yard to the LEFT of the middle, the other request is to drive one yard to the RIGHT of the middle. Clearly, you cannot do both simultaneously. What is the best compromise?

We need some simple mathematics to specify the situation. We have two goals or reference levels, r1 and r2; in our example plus and minus one yard from the middle of the road. We also have to choose an actual course; let us call that x. We therefore have two errors or deviations, |x-r1| and |x-r2|, that ought to be minimized simultaneously in some way. There are several degrees of freedom left in this problem. The first is the relative importance of the goals: is one goal somehow more important than the other one? To simplify matters, let us take the goals as equally important. The second is the way in which we weigh the errors, i.e. how we choose the importance that we attach to the magnitude of the resulting errors once we select a course: is an error of one yard three times more severe than an error of one foot? More severe? Less? To simplify matters again, let us weigh both errors similarly.

The problem can be formulated as a minimization problem:

$$Q(x) = F(|x-r1|) + F(|x-r2|)$$

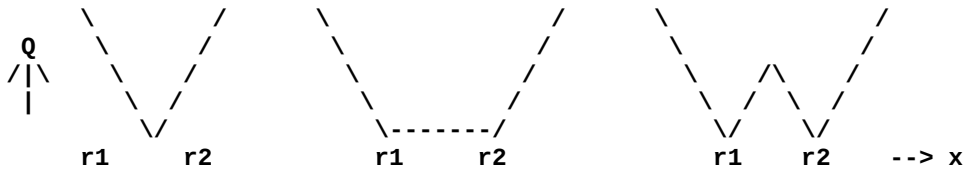
where Q must be minimized by a 'best' choice of x, and where F is a

function that provides the weights of the errors. Let us make a graph of Q (x) as a function of x for different choices of F. My ASCII character set does not allow nice graphs, only qualitative plots. In the plots below I have chosen F as a square, a unit function and a square root, respectively.

```
                          2         2
square:      Q (x) = (x-r1)  + (x-r2)

unit:        Q (x) = |x-r1| + |x-r2|

square root: Q (x) = sqrt |x-r1| + sqrt |x-r2|
```



The square function weights large errors more severely than small errors; it arrives at a unique minimum with a value of x halfway between r1 and r2. The unit function is indifferent to the relative sizes of the errors; it reaches the conclusion that any position between r1 and r2 is equally good. The square root function prefers small errors and takes larger errors less seriously; it tells you to pick ONE goal and to disregard the other.

If you want to, you can experiment with the general problem

```
     Q (x) = K * F1 (|x-r1|) + F2 (|x-r2|)
```

where the constant K specifies the relative importance of the first goal over the second, and where F1 and F2 allow you to weigh errors separately for each goal. You will then discover new possibilities; in particular a square-root-like plot with two UNEQUAL minima. In such a case you might, if you are not careful in solving your minimization problem, get stuck in a local minimum.

How do people reconcile incompatible goals? It seems that different solutions are chosen in different circumstances. Sometimes it is possible to reach a well-defined compromise. In other cases we observe that we have lots of freedom left for our actions. And sometimes we must reject one goal in favor of reaching another one exactly.

How does this problem generalize? Well, usually we have a great many different simultaneous, but conflicting, goals. Yet the TYPE of the solution space does not change; it is just of a higher dimension.

What is the lesson of this exercise? Well, it must be clear by now: conflict -- in the sense of having to choose between different minima of Q -- can arise only if you discount large errors for one goal by focussing on the other one only, or in preference. If you succeed in choosing a kind of behavior where you take large errors very seriously but small errors lightly, you will always arrive at a unique solution that saves you the trouble of having to decide which of several minima is the smallest. In fact, the more seriously you take large errors and the more you can disregard small ones, the more "robust" your solution becomes. This exercise is left for the reader: plot the graph of

$$Q(x) = |x-r1|^N + |x-r2|^N$$

for N = 4, 6, 8, ... What do you discover?

I leave all the philosophical, moral and ethical implications of this discussion to the reader.

This ended my previous post. Now let us consider some ramifications of this situation, which resembles many real life problems such as that of the ass between the two haystacks. Which strategy do people follow? That is open to test. A weird test, maybe, but it is easy to do. "I am going to present two white circles on the computer's display at the same vertical position. Your task is to use the mouse, which determines the horizontal position of the green circle, such that your green circle is as close as possible to _both_ white circles. Never mind that you cannot have zero error; you are only asked to do as well as you can." It might be, however, that what the subject does will be quite sensitive to the phrasing of this unusual request and of what he makes of it.

The earlier post indicates that there could be three strategies, besides non-participation. In all cases, the subject's behavior will be found at or around the minimum of a Q-plot, where the total error is smallest. In the first strategy, the green circle will be found at a mid-position between both white circles. In the second strategy, any position between both white circles will do. And in the third strategy one white circle will be tracked but not the other one, with maybe an occasional switch between both. An external observer, who knows the two conflicting goals, can of course easily discover the subject's strategy by an analysis of the position of the green circle with respect to the two white circles.

But how about an observer who does _not_ know about the two goals and the followed strategy? What if you can only observe the subject's behavior and want to _discover_ the subject's goal(s)? That is what The Test is meant to do.

The first problem is to decide how many simultaneous goals to test for. In the usual situation, we try to think of experimental conditions such that we can test for one goal only. If the subject uses strategy 1, the apparent discovery will be that the subject "controls for" an intermediate position; the two individual goals will not be discovered. In strategy 2, the subject will appear to "control for" any (random?) position in a varying range; quite confusing. And in strategy 3, chances are that only one of the two distinct goals may be discovered. The Test may thus yield somewhat confusing results as to the subgoals, whatever the strategy. Results do not become clearer if we assume a priori that the subject controls for two simultaneous goals. In fact, he/she does not. Control is for the one "supergoal" that is impossible to subdivide into individually and simultaneously realizable subgoals.

A (silly) example. Assume that the subject has two goals: cursor tracking and coffee drinking. Strategy 1: do your cursor tracking with the coffee mug at your lips; the performance of neither drinking nor tracking is optimal. Maybe stretching things, one could say that this resembles the logical AND operator. Strategy 3: do either cursor tracking OR drink your coffee, possibly alternately. Strategy 2 appears to be the most flexible and provides the most "freedom"; it allows any intermediate position. This does not exhaust the number of possible

strategies if other factors are dynamically taken into account as well; e.g. enjoy your coffee when tracking is easy, and put down the mug and concentrate on tracking when the latter becomes difficult.

My conclusion? Even if a subject does not dynamically switch between strategies: if the subject has multiple but conflicting/interacting (sub)goals, it may be hard or impossible to discover those (sub)goals from overall behavior. It appears that, although it is technically always possible to derive one mathematically equivalent "supergoal" given a number of distinct subgoals, the utility of that concept is limited: although some Q-formula (see above) will uniquely express the overall goal, naive inspection of the formula (rather than an extended analysis or a simulation) may well tell us little about the resulting behavior. And the inverse: given an external observer's perception of the possibly very complex "overall" behavior (with respect to the single "supergoal"), the analysis of which conflicting/interacting subgoals are contained in that single supergoal might be impossible, at least far from trivial.

Another conclusion concerns The Test. The Test may be difficult to apply or yield unintelligible results if multiple interacting (sub)goals are controlled for simultaneously. It appears that this will often (always?) be the case in real life. This may explain why PCT research has thus far failed to indentify "hard" (consistently and constantly present) goals or controlled variables. Even Maslow is more explicit ;-).

Strategy 3 (choice; either one or the other, with switching from one subgoal to the next over time) seems to play an important role in the behavior of complex control systems such as higher organisms. In my case, for instance, if I do cursor tracking, that (sub-sub-sub!)goal is easily discarded when someone shouts "fire!", or even "the coffee is ready!" And even something as urgent as breathing is temporarily stopped when you need to recover a watch from the depths of the swimming pool.

I hope this makes some sense to you, Martin. Your intuition is right. In particular strategy 2 (may we call it "freedom"?), but also strategy 3 ("choice") does not correspond at all with the PCT notion of the unique and deterministic behavior of a servo loop. So, in the PCT context we might as well do away with the "supergoal".

Alas, this is the opposite from a tendency in the modern (engineered) control system design process, where the ab initio specification of a single, all-encompassing design goal appears to become more and more important. No wonder I'm often confused about what control is ;-).

Greetings,

Hans

Date: Tue, 17 Dec 1996 12:10:43 +0100
From: "J.A. Blom" <J.A.Blom@ELE.TUE.NL>
Subject: Re: In Defense of Popper & Harms

[Hans Blom, 961217]

(Bill Powers (961216.2010 MST))

> Scott Stirling (961216) --

>>All Harms and Popper need to say is that the perception we have of
>>the world is remarkably consistent over time.

>I agree totally with this well-put proposition.

1. Isn't it remarkable how opposites can co-exist? On the one hand, our perception of the world is remarkably consistent, reproducible, modelable, "lawful", over time. On the other hand, we continuously encounter "disturbances", "noise", unexpected things happen, and surprises. Now, which is true? Which is _more_ true? Do we see the world primarily in terms of "laws" (regularities) or in terms of "noise" (irregularities)? We seem to have our personal biases...

2. If we believe we can detect "consistency over time", it seems to me that we have some inner mechanism that allows this detection. Since this detection is primarily some type of comparison between what occurs now and what occurred previously, it appears that we have some type of inner "store" where previous occurrences are somehow saved, possibly encoded and/or compacted. It also appears that, due to brain size limitations, it is impossible to store the raw data of _all_ previous occurrences in their entirety. So what is stored? Only occurrences that are recognized as "lawful"? But initially there are none. Are occurrences that are similar to a previous one somehow discarded? But how to establish what is similar right from the start? We seem to have a bootstrap problem, which indicates the need for an a priori existing recognition mechanism.

3. The term "inner world model" seems an appropriate description of such an internal store of those perceptions of the world that are remarkably consistent over time.

4. We know of mechanisms that (or: whose function it is to) extract consistencies and reject nonconsistencies. The simplest ones are probably auto/cross-correlators.

Greetings,

Hans

Date: Tue, 17 Dec 1996 13:27:18 +0100
From: "J.A. Blom" <J.A.Blom@ELE.TUE.NL>
Subject: Re: Solving "loss of input" problem

[Hans Blom, 961217b]

(Tracy Harms 1996;12,16.10:40)

>I propose that the appearance of gradualness is an illusion which
>comes from parallelism and redundancy.

Yes, there are these too. But I was focussing on another problem: how to decide (compute, control) when some small part of the information, that would -- if it were there -- allow an "optimal" decision, is missing.

>Foxes go a long way toward closing the gap with rabbits by smell,
>and perhaps sound. Sight is necessarily low among control systems
>because foxes cannot catch rabbits in a race; they must catch them
>by stealth. ...

Thanks for the info on foxes. This city boy doesn't really know the
least thing about them. I just thought I had concocted a nice
example. Teaches ya...

>I would agree that some of these controllings allow the hunt to
>continue with cunning effectiveness even when major perceptual
>channels are blinded, and even that it is precisely to cover for
>such sporatic and temporary blindings that these "models" exist.
>But I would guess that the nature of these internally-directed
>systems is such that they are not parallel to the world which is the
>subject of attention. Rather than being *models*, these are
>*strategies* or *tactics* which may increase the effectiveness for
>the higher-level perception.

To me, those are pretty much equivalent notions. Under conditions of
circular causality (in practice: effective control), the notion "if I
perceive X then I do Y" (a "forward" model) is pretty much the same
as "if I do Y then I perceive X" (a "backward" model). These models
"degrade" to being statistical (i.e. only "best" on average) if
"noise" or "disturbances" or unpredictabilities are present.

>The main problem I have with the proposed model-based control is
>that it is too expensive. The cost of good models vastly outstrips
>their possible benefit.

That was also said when model-based control was first applied in
industrial control systems. "It's far too complex and expensive, and
it only improves my yield by 2%." Yet, an extra yield of 2% in an oil
refinery may mean hundreds of millions of dollars per year advantage
over the competition. You don't need to do it, but as soon as the
competition does, you must follow if you are to survive.

Evolution theory says something similar about the survival of genes
in a population. If a single mutation has an evolutionary advantage
of 2%, the mutated gene will (accidents excepted) rapidly spread
throughout the whole population.

>>If the rabbit is behind a wide bush and invisible to the fox for
>>more than a moment, and if it picks that moment to reverse in it
>>tracks, the fox's "prediction"/control would be suboptimal. But
>>only gradually worse than when it reverses in plain sight.

>No, it is not gradually worse, it is *measurably* worse.

By gradual, I meant that there would be a smooth -- and not an abrupt
-- transition, e.g. between the two conditions "rabbit 99% hidden
behind bush for 0.3 seconds" and "rabbit 100% hidden behind bush for
0.3 seconds", all else being the same. In principle, the difference
ought to be measurable; in noisy practice, the difference might be
too small to be significant if the function is smooth.

>If the fox's response time is a function of the duration between the
>rabbit's direction change and the fox's observation of the rabbit,
>the fact that this magnitude is correctibly small does not defy the

>basic claim that a fox cannot respond to a visual observation of its
>prey without seeing it. (I'm not sure how to improve that statement
>over the tautological way I put it here.)

I have no intention to fight tautologies; your basic claim is, of course, quite logical ;-). My concern is different, however. In AI or philosophical terminology, I would express it as: how is the quality of decision making (in PCT: control) affected by missing information (in PCT: sensory perceptions), especially if the information loss is only slight (in space: rabbit still partly visible; in time: rabbit only briefly and occasionally not in view).

In other words: what does the hound (rather than fox; I stand corrected) follow? Is it the (current perception of the) rabbit itself, is it a mental picture of the rabbit, or is it some combination of both? Clearly not the second, except in daydreaming (do hounds?). If the first, what changes if there is (briefly) no perception to follow and what is then followed (or done) instead? If the hound has some kind of mental picture of the rabbit, how is that mental picture dependent on the hound's (history of) perceptions of the rabbit?

I'm not sure whether all these questions are empirical.

Greetings,

Hans

Date: Tue, 17 Dec 1996 13:44:23 +0100
From: Stefan Balke <sbalke@HRZ.UNI-BIELEFELD.DE>
Subject: Re: Decision

>From Stefan Balke (961217.1330 CET)

Rick Marken (961216.1010; 1300; 20.40), Bruce Gregory (961216.1340 EST) and Tracy Harms (961216.1340)

Thanks for this wonderful philosophical, theoretical and salutary ideas about decision making, problem solving and the nature of the consciousness. It makes my picture clearer.

Best, Stefan

End of CSGNET Digest - 16 Dec 1996 to 17 Dec 1996
****

Date: Wed, 18 Dec 1996 08:00:29 -0600
Subject: CSGNET Digest - 17 Dec 1996 to 18 Dec 1996

There are 41 messages totalling 2853 lines in this issue.

Topics of the day:

1. model-based tradeoff (2)
2. Aristotle and Contingent Truths (3)

Date: Tue, 17 Dec 1996 15:19:47 +0100
From: "J.A. Blom" <J.A.Blom@ELE.TUE.NL>
Subject: Re: model-based tradeoff

[Hans Blom, 961217c]

(Bill Powers (961216.1050 MST))

>>It is the other way around. If the model is told that the "laws of
>>the world" do not change, and they indeed do not, it can provide
>>accurate long-term predictions. If the model is told that the
>>"laws of the world" do not change, yet they do, it still will
>>provide long-term predictions, but these will of course be
>>completely incorrect. Finally, if the model is told that the "laws
>>of the world" change rapidly, yet they are constant, it will not
>>provide accurate long-term predictions -- although in principle it
>>could.

>That is exactly what I was talking about. There is a tradeoff.

I don't see a tradeoff; I see optimal behavior in the face of the uncertainties of the world. I think that I now understand that you talk about some simulations that you performed, where you wanted to see what the difference is between providing correct or incorrect "higher level" information to the model. And I assume that you have discovered -- although you may not have fully realized it -- that there is an optimal "tradeoff" when the high-level knowledge that the model is given about the world is approximately correct.

>Either you tell the model that the world changes rapidly or that it
>changes slowly.

Is what you tell the model correct? That is crucial. Are you giving correct information to the model or are you deceiving it? In the latter case, you won't get the model to behave correctly, of course, although you may see "interesting" (but pathological) results.

Solution: specify a matrix; one dimension is how rapidly the world changes (e.g. not at all, slowly, rapidly), the other dimension is how rapidly the model knows (is told; assumes) the world changes. I

made that distinction in both the text that accompanied my demo program and in the program itself, but you seem to discard it again.

If the world truly changes rapidly and the model knows it, for instance, the model will generate definite "best" long term predictions, but with the appropriately large margins of uncertainty. Long term predictions are simply impossible in such a world _and the model knows it_. If the world does _not_ change and the model knows _that_, the model will once again generate appropriate long term predictions, but this time with _small_ uncertainty margins. Both are optimally appropriate. Not, of course, if you provide incorrect information to the model.

>Disturbances come and go; they don't just vary in a nice Gaussian
>distribution. You can't predict what the crosswind will be as you
>emerge from a tunnel. You can't predict when the phone is going to
>ring. You can't predict that your keys will snag on a loop of thread
>as you pull them from your pocket. You can't predict ...

Sure, there are few, if any, things that can be fully predicted in a deterministic way. But that is not always necessary. Predictions may take the form of "it can happen that X" or "Y will be more or less present". You may know _that_ there may be a crosswind as you emergence from a tunnel, etc. You may also know that crosswinds vary in strength from zero to some upper limit Z. And even if you don't have this type of statistical information, you'd better have a control system in place that can handle such a type or _class_ of disturbances.

>A "best fit" is not what we need for control. The average crosswind
>is zero; you have to deal with the crosswind that exists THIS time,
>not its average characteristics.

Sure. You also need to have a mechanism in place that can deal with (a class of) crosswinds, generally.

>>It is impossible to predict individual noisy observations. It is,
>>however, frequently possible to accurately predict long-term
>>trends, whatever your belief in the contrary.

>I can't agree with that. The best predictions are for a medium term

"Medium term" is extremely context sensitive. But you are correct. We generally rely passively on the fact that most things hardly change within a nanosecond or so; we couldn't control such rapid disturbances away, anyway. We are also not interested in changes that take place over thousands of years; by that time our control system doesn't exist anymore. So, in a very general sense, all our models are for a medium term only.

The point I tried to get across was something different, however: that a control system is not designed to handle one specific disturbance or series of disturbances which occurs only once, but to handle a broad class of _possible_ disturbances. In this sense, a control system is _always_ based on some form of statistics.

>I think your logic is flawed, Hans. You have settled on a model that
>requires prediction. You realize that if this model is to work, the
>world must be mostly predictable.

That is not _my_ perception of my "logic"; it is what you perceive. My "logic" is quite different. Parts of it are, indeed, based on what I have come to know as useful mechanisms that implement learning -- and generalizations thereof, i.e. a useful theory of (a certain type of) learning. But what is much more important is that I perceive a world with "laws of nature", some of which I have gotten acquainted with consciously/cognitively, and others through the realization that I have "habits", customary patterns of behavior that are used rather indiscriminately, it seems when a certain class of perceptions appears to exist. All quite fuzzy, and more a personal research program than a set of conclusions.

Where I primarily see the habit/abstraction/generalization ("opening a door"), you primarily seem to see its variations ("never exactly the same"). We seem to classify and categorize differently, i.e. we have built different perceptual input functions. We also seem to have different goals ;-). But all that is to be expected...

>The world is quite lawful; I have no problem in agreeing with that.
>But it is also monstrously complex, and local conditions are always
>being affected by hidden or remote conditions of which we know and
>can know nothing.

And I have no problem in agreeing with that.

Greetings,

Hans

Date: Tue, 17 Dec 1996 10:11:57 EST
From: Bruce Gregory <gregory@CF4.HARVARD.EDU>
Subject: Re: Aristotle and Contingent Truths

[From Bruce Gregory (961217.1010 EST)]

Bill Powers (961216.2100 MST)

> Peter Cariani (961216) --

> > Epistemology is not
> >the study of how "true knowledge" is acquired; it is the
> >study of how organisms and devices are able to acquire
> >pragmatic knowledge, to better adapt to the world, to
> >improve performance, to achieve goals. No gods. No
> >orphic mystery cults. No "best seat in the house."
> >Just good clean fun.
>
> Again, there's that lurking claim to knowledge of "the world." The world
> that we come to know and learn to control is the world of perception. The
> "other" world is hypothetical, however attractive and useful that hypothesis
> may be. We can freely accept and use that hypothesis in our practical
> affairs, but when we are looking for a deeper understanding we have to
> identify it as what it is: a guess.

I hate to keep playing one note, but the "other" world is _not_ hypothetical. (I'll extend my offer to punch Scott Sterling in the nose to you, provided you don't hold me responsible for your perceptions ;-) .) My fist colliding with your nose is hardly hypothetical. What _is_ hypothetical is your interpretation of this experience ("Thanks I needed that!" "If you ever do that again I'll cancel your subscription to CSGNet!") Your model, in which all we know about the world comes from signals in our nervous system is an attractive and useful hypothesis. We can freely accept and use that hypothesis in our practical affairs (and exchanges on CSGNet), but when we are looking for a deeper understanding we have to identify it forwhat it is: a guess.

Nothing is more immediate than the world we experience. The question as to how we are able to experience this world leads us to hypothesis. In Quine's words, "What there is does not in general depend on our use of language, but what we _say_ there is does."

Pugnaciously,

Bruce Gregory

Date: Tue, 17 Dec 1996 10:17:00 EST
From: Bruce Gregory <gregory@CF4.HARVARD.EDU>
Subject: Re: Decision

[From Bruce Gregory (961217.1020 EST)]

Rick Marken (961216.2040)

> When I talk about "seeing the conflict from a new point of view"
> I am talking about a consciousness phenomenon: something outside
> of the perceptual control hierarchy is "watching" the controlled inputs,
> reference inputs and, possibly, the reference outputs of control systems
> in the hierarchy. This something is, I think, consciousness.
>
> Consciousness is not yet part of the formal HPCT model. At the
> moment, all we can do is experience it. But HPCT does suggest ways that
> consciousness might enter the behavioral picture. Imagine that
> consciousness can become aware of ("watch") one or another of the
> variables in a control loop. When consciousness is directed at the
> controlled perception we are aware of what we are "doing" (controlling);
> when it is directed at the reference signal we are aware of what we
> want; when it is directed at the reference output we are aware of what
> we are trying to do to get what we want.
>
> It seems that consciousnes can be located _in_ only one (or a limited
> number of) control system(s) at a time. From the point of view the
> consciousness in one control system the controlled perception is just
> the way things are, the reference signal is a requirement regarding the
> way things _should_ be and the reference output is one of the options
> available for meeting the requirement set by the reference signal.
>

> When I talk about viewing conflict producing goals as _options_ rather
> than _requirements_, I am talking about moving consciousness from the
> control system that gets the conflict producing goal sent to it as a
> reference signal to the control system that selects that conflict
> producing goal as one of its optional reference outputs.
>
> For example, I am talking about something like moving one's
> consciousness from a control system that sees the practice of a
> particular religion as a requirement ("I just _am_ Catholic") to the
> control system that has selected the practice of this particular
> religion from among several options that could have been used to achieve
> a higher order goal (spiritual fulfillment, possibly).
>
> Does this make any sense?

Boy, does it ever. Now you're cooking with gas. The Pope would
be proud of you ;-)

Bruce Gregory

Date: Tue, 17 Dec 1996 11:16:09 -0500
From: Martin Taylor <mmt@HERMES.DCIEM.DND.CA>
Subject: Re: model-based tradeoff

[Martin Taylor 961217 10:30]
> Hans Blom, 961217c
>
> If the world truly changes rapidly and the model knows it, for
> instance, the model will generate definite "best" long term
> predictions, but with the appropriately large margins of uncertainty.
> Long term predictions are simply impossible in such a world _and the
> model knows it_. If the world does _not_ change and the model knows
> _that_, the model will once again generate appropriate long term
> predictions, but this time with _small_ uncertainty margins. Both are
> optimally appropriate. Not, of course, if you provide incorrect
> information to the model.
>
I think a lot of the difficulty (conflict? misunderstanding?) between you and
Bill Powers comes from this matrix being inadequate. Bill is treating a
world in which the laws change slowly, if at all, but in which, in the
absence of control output, the data values change fast. You seem to
conflate those two speeds. You need either a third dimension to your matrix
(the third being law/environment) or dual values (slow-slow, slow-fast...)
on its world side. In Bill's world, an unchanging structure that
corresponds to the world's laws (i.e. an output of appropriate sign) is
part of the deal (I'd call it a minimal model, but I think Bill wouldn't).
But Bill's world needs a wide-band perceptual input to accommodate all the
different changes that might occur in the data values. What to do about
those changes is a matter that involves the slowly changing laws, and
can be specified by the slow process of reorganization.

It seems to me that if you use _only_ a model (which you have never claimed
to do, in my view), then you can accommodate only one speed of the changing
world, the speed of change of the laws being inseparable from the speed of
change of the data.

A reorganizing control hierarchy is in many ways (I'm tempted to say "all ways") equivalent to a model that adapts to the laws of the environment. The adaptation uses the data, or course, but its variation is nowhere near as fast as that of the data.

Hans Blom, 961216g

I don't remember seeing the original of this post.

>On a recent visit of Martin here, he remarked that, to him, my most
>troublesome concept was the blanket statement that there is always "one
>uppermost goal" that is being pursued by a controller (or organism). My
>reply was, that is always mathematically possible to express a number of
>separate goals as one combined "supergoal". Martin's reply was that --
>if I may paraphrase -- we might not be able to recognize such a "super-
>goal" as a _causal_ goal in the PCT sense.

Your message suggests that you quite misunderstood what I was getting at. I'm not at this point going to respond to your message, because I haven't thought about the issue you raise. But I'd like to try to illustrate what I intended when we met.

>My 921022 post: On conflict
>
>Suppose that someone requests from you that you execute a task with two
>incompatible goals,...

This isn't related to what I was trying to discuss with you. What I was addressing, instead, might be illustrated in a very simple form:

Suppose I am controlling for x=3, y=2, and z=1. You can mathematically say that I am also controlling for the top-level goal q (==x+y+z) = 6. [I use == to mean "is defined as"]. You can treat q=6 as a supergoal. Everything I do to stabilize x, or y, or z, will be in agreement with your mathematically equivalent the supergoal. But all I am doing is controlling three independent top-level goals, and I have no perception whatever of anything corresponding to q. The reference values for x, y, and z do not come from a control unit that has (x+y+z) as a perceptual input function.

The mathematician could equally assert that I have a supergoal corresponding to a relationship among x, y, and z, say r (==x=y+z) = "true"; or that my supergoal is s (== x-(y/z)^2) = -1. I may have none of these supergoals.

But then, you may say, the mathematician-experimenter could test for the existence of the supergoal by constraining z to be 2 rather than 1 (using "overwhelming force"). Then if I have a supergoal q=6, I will change the values of x and/or y. That is true. But suppose I _do_ have the supergoal q=6, and also the supergoals r = "true" and s = -1. Then what happens when you constrain z? There is no way that varying x and y can satisfy the references for r and s along with that for q. By constraining z, you have reduced my available degrees of freedom and induced conflict. (Conflict isn't a question, as has been implied in another thread, of two control systems having different reference values for _the same_ variable--it is a question of havign inadequate degrees of freedom to satisfy all reference values at the same time).

Now, if I am trying to satisfy reference values for q, r, and s simultaneously, and you constrain z, you will not discover that I am controlling for q = 6. My attempts to control for r = "true" and s = -1 will interfere. And even if you did discover the "supergoal" of controlling the value of q, I would still have no single "top-level" goal. I still have three _independent_ and normally non-conflicting top-level goals. And you, mathematician, could still discover w, a function of q, r, and s, that would allow you to say that my behaviour is "as if" I had the single top-level goal of satisfying a reference for w.

Conflict is a separate issue. It is true that ultimately there can be no conflict in a control hierarchy that has a single top-level goal (except when lower levels of the hierarchy get caught in local, non-global, minima). It is equally true that (with the same limitations) there can be no conflict in a hierarchy that has as many top-level goals as the narrowest point in the feedback loops through the hierarchy and the external environment.

The question of choosing between different "targets," when missing both is worse than hitting one and badly missing the other, is another separate issue. Both this and the conflict issue are interesting in their own right, but they are different from the issue I raised in Eindhoven.

Martin

Date: Tue, 17 Dec 1996 08:43:57 -0800
From: Richard Marken <marken@AEROSPACE.AERO.ORG>
Subject: Re: Solving the "loss of input" problem

[From Rick Marken (961217.0840)]

Bruce Abbott (961216.2010 EST) --

[Skipping past the expected misconceptions about control that result when one is defending a behaviorist system concept ;-) we come to...]

> my point in originally raising this issue was that Rick objected to Hans's
> model on the basis of its being a "computed output" system. I was simply
>attempting to show that a standard PCT-style controller (however you
>consider it to arrive at its output values)

[NB. There is only one way to "consider" how a control system "arrives at its output values: o = f(r-p). Output is proportional to error.]

>could be inserted as the primary system of an adaptive controller by using
>"computed inputs." The resulting system still works (in my estimation at
>least) the same way as PCT-style controllers always do, while continuing to
>take action against (imagined; modeled) disturbances when actual perceptual
>input is interrupted. Such a controller would continue to _appear_ to
>maintain control over the actual CV, while _actually_ maintaining control
>over the imagined CV, for so long as the modeled and real disturbances match
>reasonably well. Whether such a system is actually to be found in a human
>nervous system is an empirical question.

It seems to me that you are proposing the following: Under normal circumstances (input available) a control system controls a time varying perceptual variable, p(t), which (assuming a single time varying disturbance variable, d(t)) is proportional to:

p (t) = o (t) + d (t)

When the lights go out (no more input) the system begins controlling an imagined, time varying perception, p'(t), which is proportional to:

p'(t) = o (t) + d' (t)

where d'(t) is the modelled version of d(t) and o(t) is the real output (twhichis now added to both the real disturbance -- to determine the state of the now un-sensed controlled environmental variable-- and the imagined disturbance).

This approach to "model based control" (which differs from Hans' approach only in the fact that disturbances rather than outputs are computed from a model) is no better than the computed output approach; and it is probably worse for the reasons Bill Powers (961211.1640 MST) already gave.

The "computed disturbance" approach only works (gives the illusion that a variable is being kept under control, protected from disturbance, d(t)) if d'(t) is _precisely_ equal to d(t) over time - no phase errors, no amplitude errors, no frequency errors... nada. Any deviation of d'(t) from d(t) leads to outputs o(t) that _fail_ to compensate for the actual disturbance.

Very small deviations of d'(t) from d(t) will be magnified by system gain (and magnified even more if this is an integral controller). This means that the better the control system works when the input is intact (high gain controller) the poorer it will work when the input is lost -- unless the model's prediction of d(t) is perfect. And even this level of "pseudo control" can be achieved only if _both_ the feedback function (the function that determines the effect of o(t) on the real controlled variable) _and_ the disturbance function (the function that determines the effect of d(t) on the controlled variable) remain _exactly_ the same during the input loss period as they were during the input present period.

The PCT approach to dealing with loss of input is not the model-based predictions and computations of "modern" control theory. Rather, HPCT suggests that the best approach to dealing with possible loss of input is the development of many control systems that control different perceptual representations of the same environmental variable so that loss of one or more of these perceptual inputs will not necessarily mean loss of control of the enviromental variable.

Best

Rick

Date: Tue, 17 Dec 1996 12:22:19 -0600
From: Bruce Abbott <abbott@CVAX.IPFW.INDIANA.EDU>
Subject: Re: Solving the "loss of input" problem

[From Bruce Abbott (961217.1325 EST)]

>Rick Marken (961217.0840)]
>
>Bruce Abbott (961216.2010 EST) --
>

>[Skipping past the expected misconceptions about control that result when
>one is defending a behaviorist system concept ;-)

Such as?

>> my point in originally raising this issue was that Rick objected to Hans's
>> model on the basis of its being a "computed output" system. I was simply
>>attempting to show that a standard PCT-style controller (however you
>>consider it to arrive at its output values)

>[NB. There is only one way to "consider" how a control system "arrives at its
>output values: o = f(r-p). Output is proportional to error.]

Yes, if you are speaking about a proportional controller. What I was alluding to was whether you consider o = f(r-p) to be "computed output" or not. Hans wants to say that this formula is a formula for computed output, whereas you, I believe, have a different opinion, since you criticized his model-based controller because it was based on "computed output."

>It seems to me that you are proposing the following: Under normal
>circumstances (input available) a control system controls a time varying
>perceptual variable, p(t), which (assuming a single time varying disturbance
>variable, d(t)) is proportional to:
>
>p (t) = o (t) + d (t)
>
>When the lights go out (no more input) the system begins controlling an
>imagined, time varying perception, p'(t), which is proportional to:
>
>p'(t) = o (t) + d' (t)
>
>where d'(t) is the modelled version of d(t) and o(t) is the real output
>(twhichis now added to both the real disturbance -- to determine the state
>of the now un-sensed controlled environmental variable-- and the imagined
>disturbance).

Yep.

>This approach to "model based control" (which differs from Hans' approach
>only in the fact that disturbances rather than outputs are computed from a
>model) is no better than the computed output approach; and it is probably
>worse for the reasons Bill Powers (961211.1640 MST) already gave.

I noted in my post that at least one of the reasons Bill Powers "already gave" (that the system would go into open-loop mode) was wrong, and explained why. I didn't note your having countered that argument. Nor did Bill suggest that the system I devised would "probably be worse" than Han's system, nor did he give any reasons _why_ it would be worse than Han's system.

I do agree with Bill and Hans that any system that relies on estimation cannot handle unexpected disturbances. I agree with you and Bill that when we humans lose input, we tend to rely on substitute perceptions (e.g., the forces being exerted against our hands by the steering wheel) rather than estimating what the actual CV "should" be doing, and I agree that such estimation is very risky business for lower-level control systems that must deal with fast-changing and often unpredictable circumstances. So I am not advocating the system I described as a realistic alternative; my only purpose was to describe how such a system might work and (as your reply to my post failed to note) to indicate the circumstances under which it would

be expected to do the job intended of it.

Regards,

Bruce

Date: Tue, 17 Dec 1996 12:29:08 -0700
From: Bill Powers <powers_w@FRONTIER.NET>
Subject: Lights and buttons. Re: In Defense of Popper & Harms

[From Bill Powers (961217.0700 MST)]

Bruce Gregory(961217.0525 EST)]

>So you've joined Scott in positing a conistent world of perception.
>Nothing more homey than a pair of solipcists. Don'y get me wrong,
>some of my best friends are solipcists ;-)

Well, I'm glad they're not solipsists. I can't stand solipsists.

I'm just an ignorant engineer, not a philosopher. I know that solipsism is supposed to be bad, and that naive realism is supposed to be bad, and that pragmatism is supposed to be bad, and so on -- it all depends on whose book or paper you're reading. Which attitude you're supposed to believe in seems to depend mainly on how forcefully the person you're reading can present an argument, or how impressive his or her reputation is. When I read philosophy I feel like Nasrudin: everything I read seems to be right while I'm reading it. You say they can't ALL be right? By golly, you're right!

Here's how I look at perception. It's nothing fancy or deep; it's just the way an engineer deals with appearances, which includes what we seem to know about nervous systems, physics, and all that realist stuff.

The human brain acts on the world outside it by emitting neural signals; it detects the world outside it by receiving signals from sensory receptors. The evidence for this is pretty overwhelming. If a person loses the ability to send neural signals to muscles, paralysis is the result. If neural signals from sensory receptors are cut off, whatever used to be sensed that way is no longer sensed. That part of the experienced world just goes away, as you lose a lip when you get novocaine at the dentist's, or as the visual world goes away when you shut your eyes or go blind.

Along with this we have the role of the brain itself. Whatever we know, we need a brain to know it with, to think about what we know, to make associations, to have opinions, to understand, and so on. Damage to the brain from physical trauma or diseases like Alzheimer's severely affects all our mental facilities.

So it looks to me as though the brain is the knower and actor, or at least an indepensible part of the process of knowing and acting, and that what it knows comes to it in the form of neural signals from sensors, which are also indispensible.

If that's so, then the engineer would want to know what all those sensors are detecting: what gives rise to the neural signals? It seems that visual receptors generate signals in response to the absorption of light quanta of particular wavelengths by photosensitive nerve-endings; that olfactory

signals are generated by chemical reactions at the surfaces of other receptors; that tactile signals are generated by mechanical distortions of sensors; that auditory signals are generated by the bending of hair cells in the cochlea which distort auditory sensors -- and so on for all the sensors that there are. Each sensor is affected by local physical or chemical effects. Each sensory signal arises from physical effects in a tiny region immediately outside the sensory nerve-ending that generates the signal.

All this we get from a purely conventional naive-realist way of examining the world.

When we look at any one sensory signal, what kind of potential knowledge about the world do we find there? There seems to be a more or less simple relationship between the amount of physical or chemical effect on the sensory ending and the rate at which that ending generates impulses. Some sensors simply generate signals that depend monotonically on the intensity of the physical stimulation. Others respond in an exaggerated way to changes in intensity, but also show a steady-state response of a smaller magnitude. A few sensors seem to "adapt" in the sense that under steady stimulation their neural signals eventually die out altogether.

For each sensory signal, measured in terms of its frequency of firing, we can ask what a steady magnitude of that signal would represent in the world outside it. If the sensor adapts over time, a steady signal would have to be generated by a steadily-increasing amount of stimulation. For the static type of sensor, a constant magnitude of signal would correspond to a constant magnitude of stimulation. For a mixed type of receptor, a constant signal would go with a stimulation that increases exponentially at a certain rate to an asymptote.

So we can say that each neural signal emitted by a sensory receptor represents by its magnitude the state of some physical process going on in the immediate vicinity of, or even inside, the sensory nerve ending. That is the basic stuff of the initial signals. As those signals vary, they indicate variations in the local stimulation of the individual nerve-endings.

An engineer can understand this sort of arrangement, because there are counterparts of all the kinds of sensory receptors in artificial sensors like microphones, photocells, strain gauges, chemosensors, and so forth. Each device generates an electrical signal that depends on local effects of the environment: the absorption of light quanta by a photocathode, the displacement of a crystal or a coil in a magnetic field, the elongation of a wire that changes its resistance to current flow, or chemical reactions that liberate ions that can be detected electronically.

In building a device that uses lots of sensors, like a spacecraft, the engineer can see that there is a whole layer of signals, each coming from a particular sensor, that contain whatever any subsquent signal-processing can know about the world that's being sensed.

However, these signals by themselves don't represent anything very useful. To measure the velocity of the spacecraft, for example, the signals from strain gauges that are indicating a force due to acceleration must pass through a time-integrator to produce a new signal that represents velocity. And signals from several strain-gauges, each representing acceleration in a different dimension (linear or angular) must be passed through a computer which can yield a signal representing total magnitude of acceleration, and through another producing signals that indicate direction of acceleration. The signal from the photocell in a star tracker must be correlated with

signals indicating direction of pointing relative to the spacecraft, so a new signal indicating pointing error can be generated.

And so on. To the engineer, it's clear that what HE can see going on in the environment is of no use at all to the systems he's building. Whatever those systems do, they can do only on the basis of what the systems' own sensors are telling them, and what the systems' own analytical capabilities can derive from the multiple sensor readings. The systems have no other way to know what is going on outside them.

The engineer, who can see a lot more than the system he's building can see, can give the system a lot of help by arranging its sensors in the most useful way, and by combining signals in just the way that will correspond the most usefully to external phenomena. But every now and then, the engineer might pause to think about the analogy between his own internal systems and those of the system he's constructing. The first thing he will have to realize is that there is no engineer standing by outside to tell his internal systems what the signals from the sensors mean, or what the best way of combining them into new signals would be.

In fact, the engineer might find it somewhat confusing to imagine himself being the system he is constructing. The engineer, when not identifying with the system, knows that a certain sensory signals represents an effect of acceleration, but inside the system there is no label on the signal; the system can know only that there's a signal of a certain magnitude. And when that signal goes through a time integrator and turns into a velocity signal, again there's no label saying that it represents velocity. In fact, whatever the system "knows" about its environment must ALSO exist as a neural signal, an unlabeled signal.

Enough of this sort of cogitation might lead the engineer -- me -- to devise an analogy that goes like this:

Imagine that you find yourself in a control room in which the walls are covered with lights and buttons. Nothing is labeled. Your abilities consist of the ability to look at the lights and to push any buttons that strike your fancy. Having nothing else to do, you watch the lights and push the buttons.

The first thing you discover is that pushing the buttons always has an immediate effect on certain lights. Other lights may be affected, but they aren't always affected. One button always makes one set of lights turn on; another button makes a different, but partially overlapping, set of lights turn on. Figuring out which buttons always affect which lights takes a while, but eventually you have the connections memorized; you can make any of those lights turn on whenever you please because you know which buttons to push.

While you've been doing this, there have been certain effects that seem to come and go at random. Sometimes you feel good, sometimes you feel bad. After while, when you've pretty much got the buttons mastered, you realize that if you turn on one set of basic button-lights, sometimes another set of lights turns on and at the same time you feel bad -- or else good, depending on which lights turn on. So you try to figure out how to keep the bad-feeling lights from turning on, and how to keep the good-feeling lights from turning off. In doing this, you begin to recognize lights that you've seen before, and groupings of lights. They become familiar things. You learn how to turn them on and off by using combinations of the button-lights.

After long enough at doing this, you begin to use shorthand. Instead of turning on a set of 100 lights, you find that you can just attend to three or four lights in the set, and by turning them on you turn on the whole set. Turning on the three or four lights becomes equivalent to turning on all the lights in the set. You have constructed a name for the whole set, using a subset as a symbol for the whole set.

Now you can start reasoning in terms of the names: if set .:. is on, and set ::.. is off, or if ::.. is on and .:. is off, then :.:.. will be on. If :.:.. feels good, you will try to manipulate .:. in relation to ::.. so that the logical function (.:. XOR ::..) remains TRUE. You learn to like .:.ing, and also ::..ing, but not when they're going on at the same time.

In this way you can build up a world made of different patterns of lights, and learn to control them to avoid the bad and seek the good, and use subsets to name them and reason about them, and do everything that human beings do with perceptions -- all without the slightest idea of what is on the other side of the panels of lights and buttons.

That's my epistemology. We can poke the buttons and watch the lights, but the connections between the buttons and the lights, on the other side of the display panel, are invisible to us. We can be pretty confident that there ARE connections on the other side, but we can only guess at what they are on the basis of how the buttons, singly and in simultaneous and sequential combinations, affect what we can see.

```
*  *    *   *   *  *****        *        *   *  *   ***  *****
*  *   * *  *   *  *           * *       **  *  *  *     *
****  ***** *   *  ****       *****      * * *  *  *     ****  * * * * *
*  *  *   *  * *   *          *   *      *  **  *  *     *
*  *  *   *   *    *****      *   *      *   *  *   ***  *****
```

Best,

Bill P.

Date: Tue, 17 Dec 1996 12:29:17 -0700
From: Bill Powers <powers_w@FRONTIER.NET>
Subject: Re: resend: single top-level goal?

[From Bill Powers (961217.0945 MST)]

Hans Blom, 961216g --

I think that your analysis of the "top level goal" in cases of conflict is quite interesting. It might be more realistic, however, if you began with a more detailed analysis of a simple conflict; the relationships aren't completely straightforward.

Suppose you have two control systems at the same level, coupled together at a lower level so that both cannot simultaneously achieve zero error.

Each system will end up with a certain amount of error, the balance depending on the relative loop gains (assume no limits on output). If the feedback and input functions have a gain of unity, all the loop gain will be in the output functions: G1 and G2. Let e be the error. When system 1 alone

is acting, the value of x, the common lower-level variable, will be G1*e1. When system 2 alone is acting, x will be G2* e2. When both systems are acting, we introduce the constraint

G1*e1 = G2*e2 = x

Let e = r - x, so we have

G1*(r1 - x) = G2*(r2 - x). Solving for x, we obtain

x = (G2*r2 - G1*r1)/(G1 - G2)

Note that as the gains approach equality, x increases without limit when r1 is not equal to r2. The direction of the increase depends on which gain is the greater, and on whether G1*r1 is greater than or less than G2*r2. In real systems, the value of x will also depend on any physical limits to the output of the two systems.

As you can see, the result of conflict between two control systems is not just a simple balance of forces. A more complete treatment including the system dynamics would show that there can also be a point of instability, where the effective feedback between the systems becomes positive and the combined system either oscillates or runs away (until it encounters any physical limits that may exist).

This may alter your analysis of error minimization.

Best,

Bill P.

Date: Tue, 17 Dec 1996 12:29:20 -0700
From: Bill Powers <powers_w@FRONTIER.NET>
Subject: Re: In Defense of Popper & Harms

[From Bill Powers (961217.1100 MST)]

Hans Blom, 961217 --

>(Bill Powers (961216.2010 MST))
>
>> Scott Stirling (961216) --
>
>>>All Harms and Popper need to say is that the perception we have of
>>>the world is remarkably consistent over time.
>
>>I agree totally with this well-put proposition.
>
>1. Isn't it remarkable how opposites can co-exist? On the one hand,
>our perception of the world is remarkably consistent, reproducible,
>modelable, "lawful", over time. On the other hand, we continuously
>encounter "disturbances", "noise", unexpected things happen, and
>surprises. Now, which is true?

Both are true. Our perceptions (I presume) are stable representations of the state of the world, but there are influences in the world which can affect that state, thus also tending to cause (faithful) changes in the perceptions

representing it. In your own terms (approximately), measurement noise is small but process noise is large. You seem to assume, against all the evidence, that the reverse is true.

>Which is _more_ true? Do we see the
>world primarily in terms of "laws" (regularities) or in terms of
>"noise" (irregularities)? We seem to have our personal biases...

We _see_ the world in terms of variables; "laws" are derived at a higher level of perception, and they are even more regular than the observations. The world we observe is regular enough that we can detect small changes, and vary our actions so as to keep the changes small. The whole point of control is to keep the observed world matching our reference levels despite all the unpredictable changes that would occur if we didn't act. You're conflating disturbances that can cause variables to change with irregularities in the way perceptions represent those variables. Two different subjects.

>2. If we believe we can detect "consistency over time", it seems to
>me that we have some inner mechanism that allows this detection.

Yes, it's called a perceptual input function.

>Since this detection is primarily some type of comparison between
>what occurs now and what occurred previously, it appears that we >have some type of inner "store" where previous occurrences are >somehow saved, possibly encoded and/or compacted.

If you're going to propose a mechanism, why propose the most complicated one you can think of first? Perception of consistency over time doesn't have to be anything more complex than perceiving a first derivative of zero. You're talking in terms of very high-level conscious cognitive judgments. At lower levels such language is inappropriate.

>.. it is impossible to store the raw data of
>_all_ previous occurrences in their entirety. So what is stored?
>Only occurrences that are recognized as "lawful"? But initially >there are none. Are occurrences that are similar to a previous one >somehow discarded? But how to establish what is similar right from >the start? We seem to have a bootstrap problem, which indicates the >need for an a priori existing recognition mechanism.

You're setting up the problem in terms that make it extremely complex. There's no reason to think that we have to compare experiences with previous experiences in order to arrive at lawful perceptions, although we can certainly do that at a high enough cognitive level. Simply finding a perceptual function that produces a controllable signal is enough. The unsuccessful organizations don't have to be remembered. Past values of perceptions don't have to be remembered. "Similarity" isn't necessarily relevant.

>3. The term "inner world model" seems an appropriate description of
>such an internal store of those perceptions of the world that are
>remarkably consistent over time.

Only because you have set up the problem that way. Remember that your world-model model doesn't even have any perceptual functions. If you considered how much of the work you are describing could be done as information comes into the system, you'd see that it doesn't have to be done AGAIN in an internal world-model.

>
>4. We know of mechanisms that (or: whose function it is to) extract
>consistencies and reject nonconsistencies. The simplest ones are
>probably auto/cross-correlators.

Fine. When we make them work in a way that reproduces human perception, we will have made a lot of progress. But such functions can just as easily be part of perceptual input functions as world-models.

Best,

Bill P.

Date: Tue, 17 Dec 1996 12:29:24 -0700
From: Bill Powers <powers_w@FRONTIER.NET>
Subject: Re: Solving "loss of input" problem

[From Bill Powers (961217.1200 MST)]

Hans Blom, 961217b --

>To me, those are pretty much equivalent notions. Under conditions of
>circular causality (in practice: effective control), the notion "if I
>perceive X then I do Y" (a "forward" model) is pretty much the same
>as "if I do Y then I perceive X" (a "backward" model).

I think you're just out of practice in thinking about closed-loop systems. If you understood how ubiquitous disturbances are, you would not say "If I do X then I perceive Y." In the real world, if you "do X" ten times in a row, you will get nine different values of Y. On a given automobile trip from home to work, you will find that as you perceive X, the successive positions of the car, you will also see that you are doing Y, a series of movements of the steering wheel. But if you then produced those very same movements of the steering wheel on the next trip, do you think you would perceive the same trip to work Y as before?

The forward and backward models seem equivalent only if you're unfamiliar with how control systems work -- and the world they work in.

Best,

Bill P.

Date: Tue, 17 Dec 1996 18:13:36 +0100
From: "J.A. Blom" <J.A.Blom@ELE.TUE.NL>
Subject: Re: Solving "loss of input" problem

[Hans Blom, 961217f]

(Bill Powers (961216.2000 MST))

>The reason so many engineers have been interested in the Kalman
>filter is that it enables them to do lots of things (beside design
>control systems), and because it is taught to them in engineering
>courses.

One major reason was, I think, that the invention was fairly incremental; it was "only" a real-time version of already familiar least squares curve fitting procedures which had a proven utility but before that time existed only as batch programs. So, in one sense there was "nothing new"; in another sense, curve fitting techniques could suddenly be used in all types of real-time processes, including control.

>HPCT is not taught in any engineering courses that I know of.

There are a lot of different approaches to the design of control systems, some very familiar and others less so. Only practice -- actual designs -- can demonstrate their niche of applicability. Every approach has its weak and its strong points, and these are pretty well known of all the familiar methods. Control _engineering_ is extremely practice-oriented. In the design of a high performance control system, the use of "proven technology" is a key issue, not only for the choice of sensors and actuators, but also for the control algorithm. As long as it is unknown in which niche HPCT fits (if any), it will not be taught -- at least not in engineering classes.

But I thought that all that was no concern of yours. HPCT is not about designing practical control systems; it is about finding a match between human behavior and a proposed model. Or so I thought.

>I would guess that all the control engineers who know about HPCT are
>on CSGnet. Your opinions about your model and its capabilities are
>among the kinds of reasons it is not taught. This is what I meant by
>the "advantages of incumbency."

Demonstrate _that_ -- and the practical conditions when -- an HPCT design approach is superior to other methods. If you can do that, and if the niche is important enough, HPCT will soon be taught.

>>>Here's my point. If the same amount of labor had been put into
>>>applying these adaptive methods to a PCT model, it would be much
>>>easier to put aside the distraction of "adaptiveness" and simply
>>>look at the basic organization being proposed.

>>I'm sorry, Bill, but "adaptiveness" won't go away.

>My goodness, you have a way of substituting your own words for mine.
>Read the above: "if the same amount of labor had been put into
>applying these adaptive methods to a PCT model ...".

Thanks for your correction. I got the impression that you said that adaptiveness was distracting us from more important matters. As you have been known to say in the past. Is my internal model of you outdated? Is there now a new you that it must adjust to? ;-).

>>(H)PCT models _also_ need to compute the right output. You may
>>object against the word "compute" because the underlying PCT
>>mechanism is so simple, but what's the fundamental difference?

>The fundamental difference is that the MCT model has to have a model
>that imitates the properties of the environmental link. The PCT
>model does not, nor does it need to compute the inverse of the

>environmental link.

I once attended a control engineering course where we studied the family resemblances between the various control algorithms. That has helped me tremendously whenever a choice for a certain method was required, because I could see what a certain method concentrates on and what is relatively neglected. One discrimination between methods is along the line of adaptive/non-adaptive or variable/fixed control parameters. If parameters are fixed, they may be simply an invisible part of the structure, and the designer may be hardly or not at all aware that he has "chosen" certain fixed parameter values. Only when one compares different schemes may it become clear that where one method has e.g. a multiplier linking two elements, another scheme has "nothing" (just a passive link) in between those two elements. It is only on comparison that one can see that the "nothing" is a unity transfer function, where the transfer function in the other scheme was a different, maybe adaptive, function. That may be why I "see" more (or other things) in a PCT diagram than you do.

The advantages of "nothing" links are clear: simplicity and the ensuing ease of understanding; and a robustness regarding component failure. Disadvantages may be a smaller operating range when the parameters of the "system to be controlled" vary and less optimized performance. In applications where the disadvantages don't matter, the method is fine. If one can somehow do away with or decrease the disadvantages while keeping the advantages, one may have a superior method for more difficult applications as well.

>It's strange: there seems to be a little voice in my head telling me
>that your reaction to what I just said will be to say that there is
>still no fundamental difference between the PCT and MCT models.

What is "fundamental"? I see resemblances with other methods as well as differences.

>And I'm sorry that you can't see that there IS NO WORLD-MODEL in the
>PCT model that corresponds to the one in the MCT model.

How about an "invisible world model"? ;-).

>>Start to demonstrate a useful, real (not simulated) HPCT-based
>>system and the number of articles will soar, I assure you.

>How about my arm model? That's a simulation, to be sure, but a real
>model could easily, if not inexpensively, be built to test it.

A physical arm model would be a fine start, but it would need to do something _useful_ if it is not going to be considered a toy.

>Time and numbers don't prove anything except how unimaginative
>people can be, and how easily they jump on bandwagons.

Regrettably, not all imaginations bear fruits. They can be enjoyable in themselves, of course, but that kind of phantasy may bear little relationship to real life problem solving.

> ... a widely-accepted idea tends to suppress work on rival ideas,
>simply by hogging all the resources, and by allowing the proponents
>of the accepted idea to be the referees who decide what publications

>are likely to advance the state of the art. THEIR art, of course.

Yes. Very similar to how we now interpret "survival of the fittest".
The laws of the universe can be pretty annoying...

Greetings,

Hans

Date: Tue, 17 Dec 1996 13:34:30 -0800
From: Richard Marken <marken@AEROSPACE.AERO.ORG>
Subject: Re: Solving the "loss of input" problem

[From Rick Marken (961217.1330)]

Me:

>[Skipping past the expected misconceptions about control that result when
>one is defending a behaviorist system concept ;-)

Bruce Abbott (961217.1325 EST) --

>Such as?

I accidentally (;-)) trashed the post. Could you send me a copy. I'd be happy
to point out the misconceptions (there were only a couple, as I recall) if
you'd like.

>What I was alluding to was whether you consider o = f(r-p) to be "computed
>output" or not.

We had a discussion about this some time ago, didn't we? Yes, o is
continuously "computed" in the control equation above. But this kind of
computation differs substantially from the kind that is presumed to go on
in model- based control. I can think of two important ways in which
the control "computations" differ from the model based computations of
output: 1) The control computation is _much_ simpler. The control equation
can be implemented in a simple neural circuit that multiplies error (r-p)
to produce output. The model based control computations, on the other hand,
are typically much more complex, requiring the computation of trigonometric,
logarithmic and other functions. 2) The control computation is part of a
closed loop. The output of this "computation" is a function of an effect of
the output (p). The model based control computation, on the other hand, is
open loop. The output of this computation is a function of a "command signal"
(similar to the PCT reference signal) that specifies the intended result of
an output. A set of calculations (representing the inverse of the model-
determined feedback function) must be done to convert the command signal into
an output, o, that will have the intended result (which it will _only_have if
the model feedback function is, indeed, an accurate representation of the
real one).

>I noted in my post that at least one of the reasons Bill Powers "already
>gave" (that the system would go into open-loop mode) was wrong

Then you noted wrong. The system is in "open loop mode" as soon as you take
away the perceptual input. Since you agree that your system is controlling

p'(t) = o(t) + d'(t)

then noting that the system is not in open loop mode is notably wrong. p' is an _imagined_ perception; the control loop is closed via the imagination connection but it is certainly is not _closed_ in the normal sense of that term -- which is that the loop runs from output to controlled variable back to perception (p) and output again. When your model has no input it is running open loop.

> I didn't note your having countered that argument.

I guess I didn't see anything to counter. The above can count as my counter.

>Nor did Bill suggest that the system I devised would "probably be worse"
>than Han's system, nor did he give any reasons _why_ it would be worse than
>Han's system.

Read Bill's post more carefully (I trashed that one too; see, nothing personal; just thought the discussion was over;-)). I think he said something at the end about it being better to just go ahead and compute the output directly from the world model rather than try to model the disturbance.

Best

Rick

Date: Tue, 17 Dec 1996 16:52:34 -0500
From: Scott Stirling <scstirli@ANSELM.EDU>
Subject: Defense of Stirling

On Tue, 17 Dec 1996, Bruce Gregory wrote:

> [From Bruce Gregory (961217.1010 EST)]
[snip]
> I hate to keep playing one note, but the "other" world is _not_
> hypothetical.
[snip]
> Nothing is more immediate than the world we experience. The
> question as to how we are able to experience this world leads us
> to hypothesis.

Allow me to rephrase "Nothing is more immediate than the world we experience," as "Nothing is more immediate than the experience we experience." As you all can see, the former sentence posits a subject (_the world_) which is claimed to have existential import. We can say, equivalently, "There exists at least one world such that nothing is more immediate than our experience of it." Now, keeping these points in mind, note that the paraphrase of that first sentence above is a tautology. The tautology and the original sentence are equivalant.

let p= experience
let q= a world
p>q= if there is experience, then there is a world.
p, therefore q, i.e., there is experience, therefore there is a world.

As you can see, the argument above is valid in form. But is it true? Is there experience in the traditional sense of perception of a

world indeopendent of the experiencer? Or is there merely perception, which we call experience and assume corresponds to another reality. There is no way to prove that there is a world that exists independent of experience, just like there is no way to prove that the sun will rise tomorrow. Inductively we can make a sound argument for either case, but we cannot prove it in the same way that a deductive argument can be proved, i.e., absolutely true or false.

One can be punched in the face and, by sheer force of that sort of rhetoric, _believe_ that one has been punched into reality, but there is no way to prove deductively that experience of the world is not just perception-- period. The nature of inductive logic is such that no amount of cases or evidence can prove an argument or theory--it can only make a them sounder.

Solipsism is a theory that may be swiftly dispatched. Due to consistency of my perceptions and attempts to test the theory that I am the only entity that exists, I have concluded, satisfactorily, that even if I am the only being in existence, it behooves my emotional state and other senses, rational and physical, to act as if the other people I perceive do actually exist. Since this is so, solipsism does not seem to increase the fit between my perceptions and my reason, so there is no need for it.

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Date: Tue, 17 Dec 1996 10:39:22 -0700
From: "T. B. Harms" <tbh@TESSER.COM>
Subject: Re: Popper's Preposterous Propositions

*From Tracy Harms 1996;12,15.21:30

Bill Powers (961215.1800 MST)

>I think that a good deal of our difficulty with Popper arises from the fact
>that he was most closely associated with the _life_ sciences, where
>knowledge has been largely statistical in nature (for a number of reasons,
>including wrong theories).

This is just not so. Popper's main attention was to *physics*; the circles of scientists he frequented were mostly composed of physicists, and he made significant explanatory efforts regarding interpretation of quantum mechanics. One of the ways in which Popper is a bit frustrating to me is the way his frame of mind was shaped so much more by physics than life sciences.

>[Consider] an engineer doing strength-of-materials tests of a
>design for a bridge [...] The engineer has to seek out
>that knowledge, for some purpose of his own. But he can't get that
>knowledge without interacting with the frame member; the knowledge
>is there, waiting to be got if he wants it, but it will stay there

>forever if nobody looks for it.

The *stuff* is there, and with it the potential for knowledge. But the knowledge is not there; it is not extracted from, but instead constructed in relation to, that stuff.

>[...]
>Naturally, unpredicted failures do occur, but they don't occur by chance.
>They occur because of either lack of knowledge, or deliberately ignoring
>knowledge (often to save money).

One point of difference I think I detect beteen us is that you seem to be unaware of what Peter Munz calls false knowledge. This is a fairly new concept, not much percolated through the literature, and it certainly is counterintuitive to most people's presuppositions about knowledge. But the necessity of clarifying truth as distinct from (though related to) knowledge requires this odd combination. This is especially so for intellectual knowledge. That's just a teaser; I'll suspend the topic now that I've raised a bit of curiosity...

>Sorry for getting out the fire hose when all you wanted was a drink of water.

Anybody got a towel?

Tracy Bruce Harms tbh@tesser.com
Boulder, Colorado

Date: Tue, 17 Dec 1996 16:58:39 EST
From: Bruce Gregory <gregory@CF4.HARVARD.EDU>
Subject: Re: Lights and buttons. Re: In Defense of Popper & Harms

[From Bruce Gregory (961217.1700 EST)]

Bill Powers (961217.0700 MST)
>
> Bruce Gregory(961217.0525 EST)]
>
> >So you've joined Scott in positing a conistent world of perception.
> >Nothing more homey than a pair of solipcists. Don'y get me wrong,
> >some of my best friends are solipcists ;-)
>
> Well, I'm glad they're not solipsists. I can't stand solipsists.

Don't I get any credit for even thinking of the word solipsist at 5:25 A.M.? O.K. O.K. I will not misspell solipsist. I will not misspell solipsist. I will not misspell... (How about conistent?)

> I'm just an ignorant engineer, not a philosopher.

That "aw, shucks" approach is charming, but will get you nowhere with this audience.

Don't get me wrong. Sophisticated as I am, I like your model very much. In fact, it look a great deal like my model. I can understand why you have fallen in love with your model. We all

fall in love with our models. All I was saying is that _it is a model_. Its function is to make sense out of your experience. (I use it to make sense out of mine, too.) But if neither of us had any experience to begin with, neither of us would need a model to explain it. The experience is prior to (in a logical as well as chronological sense) the models used to explain it. I don't "infer" the existence of an external world, I encounter it whether I want to or not. My dogs don't infer that I am putting food in their bowls, they see me putting food in their bowls. My model (but not theirs) allows me to explain _how_ they are able to perceive me doing this, but the food is not a logical construct or an inference as far as they are concerned. Solipsism (see I got it right that time) reifies a philosophical model. Realism reifies a philosophical model. I'm trying to avoid doing the same by drawing a distinction between my models and what they are designed to represent. What they are designed to represent, as James argued forcefully, is our experience. There is a phrase from a long poem by the Third Patriarch which says something to the effect, "It is always before you. Begin to reason about it and at once you fall into error." We fall into error, from my point of view when we reify our models and think that the world is something we infer. You may well not agree. Since this is clearly philosophy rather than PCT I won't argue with you. (I don't want to take advantage of an ignorant engineer. ;-))

Bruce Gregory

Date: Tue, 17 Dec 1996 15:17:18 -0700
From: "T. B. Harms" <tbh@TESSER.COM>
Subject: Re: Lights and buttons. Re: In Defense of Popper & Harms

*From Tracy Harms 1996;12,17.15

Bill Powers (961217.0700 MST)

>[...]
>So it looks to me as though the brain is the knower and actor, or at least
>an indepensible part of the process of knowing and acting, and that what it
>knows comes to it in the form of neural signals from sensors, which are also
>indispensible.

You have reiterated the common-sense view of knowledge which, while plausible on the face of it, results in intractible explanatory difficulties.

The idea that what the knower knows comes to the brain in the form of neural signals from sensors is what Gary Cziko has called instructionist, what Perkinson calls transmission theory, and what Popper calls the bucket theory. I have no problem with the assertion that brain receives neural signals from sensors, but these signals do not comprise a reception of knowledge from outside. Oxygen comes to the brain from outside, as do nutrients. But *knowledge* is not parallel to these. Accepting neural signals as knowledge, or as mediators of knowledge, leads to more and worse problems than it solves.

Rick Marken (961215.2150) asks: ''But once people have found PCT, why shouldn't they drop off the "wise men" at the college center while they attend classes on their own?'' It is not the mentors who tag along, it is their respective contributions, and either these are incompatible with PCT (in which case we do well to identify which one looks worse) or they have compatabilities which are worth discovering. (Sorry, Rick, but your call sounds all too much like a wish that people would check their minds at the door...)

While it must be very convenient to rely on "a purely conventional naive-realist way of examining the world", that only holds so long as the problems which attend it are not more inconvenient. While I'll readily admit that philosophy has produced little, by and large, it has been relatively good at spelling out the problems of this approach. The impression I get, Bill, is that you hope if you ignore these problems they will go away.

>When we look at any one sensory signal, what kind of potential knowledge
>about the world do we find there?

Where you see potential knowledge, I see present, organic knowledge.

>[...] In fact, whatever
>the system "knows" about its environment must ALSO exist as a neural signal,
>an unlabeled signal.

To my dismay I have not located the famous statement by Konrad Lorenz, that there is knowledge in the hoof of the horse reflecting the reality of the steppes. That concept applies here to counter the idea that knowledge about the environment is primarily something channelled by nerves from sensors.

[Thought-experiment re. room with buttons and lights here snipped.]
>That's my epistemology. We can poke the buttons and watch the lights, but
>the connections between the buttons and the lights, on the other side of the
>display panel, are invisible to us. We can be pretty confident that there
>ARE connections on the other side, but we can only guess at what they are on
>the basis of how the buttons, singly and in simultaneous and sequential
>combinations, affect what we can see.

Having made a real effort to convince us that knowledge is a matter of signals which come to an observer through channels from sensors, you end up abandoning that altogether for your climactic description. In that sketch there is no reliance whatsoever on signals from an outside.

Tracy Bruce Harms tbh@tesser.com
Boulder, Colorado

"Human beings invented the word 'consciousness' to describe their own particular sentience, but nature's awareness is not exactly intelligence nor sentience, nor consciousness, nor awareness. It falls between all the words."

Robert Bly

Date: Tue, 17 Dec 1996 12:35:14 EST

From: Bruce Gregory <gregory@CF4.HARVARD.EDU>
Subject: Re: In Defense of Popper & Harms

[From Bruce Gregory (961217.1235 EST)

Scott Sterling 16 Dec 1996 22:24:52 -0500

> On Mon, 16 Dec 1996, Bruce Gregory wrote:
>
> > I don't posit an external reality, I encounter it.
>
> I do not intend to wrestle over matters of faith. Everyone is entitled
> to their beliefs.

Do my dogs and I posit a common world, or do we share one?
(Insofar as I know, they are not given to extensive positing.)

In a message that seems to have gone astray, I said that if I
knew you better, I would punch you in the nose, and ask you to
posit that I had not...

Bruce Gregory

Date: Tue, 17 Dec 1996 15:30:02 -0700
From: "T. B. Harms" <tbh@TESSER.COM>
Subject: Re: Defense of Stirling

*from Tracy Harms 1996;12,17.15:30

Scott M. Stirling,

>The nature of inductive logic is such that no
>amount of cases or evidence can prove an argument or theory--it can only
>make a them sounder.

Just a quick technical point: additional cases, by themselves, do not even
make a theory stronger. Induction has no strength whatsoever.

Tracy Bruce Harms tbh@tesser.com
Boulder, Colorado

Date: Tue, 17 Dec 1996 17:15:02 -0500
From: Martin Taylor <mmt@HERMES.DCIEM.DND.CA>
Subject: Re: Auditory temporal structure

[Martin Taylor 961217 15:30]
> Peter Cariani 961216 20:40
>
>
> The kind of correlational analysis that I'm interested in is very much in line
> with that of Roy Patterson's, although I think that he is thinking of this more
> in terms of peripheral representations, whereas I think that these kinds of

> representations could be analyzed more centrally. Patterson, as far as I know,
> doesn't take the time window way out to cover tens or hundreds of milliseconds
> and the running time patterns of whole phonetic elements and syllables.

I just checked his Web site <http://www.mrc-apu.cam.ac.uk> from which you can get to the site for the SAI (not ASI as I wrote before). From there you can download either a paper on the SAI (from the Journal of the Acoustical Society) or the software to run it on a Unix workstation. In the paper there are a couple of pictures of what the SAI looks like, and those pictures run to 32 msec. I've sent Roy e-mail asking him how far out he thinks it works in people. But yes, he's interested in peripheral representations that are stable when our perceptions are stable, and not otherwise.

> > Most computerized recognizers start
> > with some analysis of the spectrum and then go with the sequence of
> > spectral patterns identified (perhaps tentatively). Patterson's ASI
> > might be a better place to start, but it's computationally very expensive.
> > Easy to do in a neural system, hard in a computer (as is correlation).
>
> The power of computing is fast approaching the point where these things are
> feasible........
>
That doesn't affect what I intended to imply, that it takes a lot of operations in a computer to do what is a basic operation in a neural system...just as the reverse is also true. Neural systems find it hard and slow to do abstract arithmetic, which is as basic to computers as correlation, template matching/filtering (they are essentially the same thing), or real-valued transformation is to neural systems.

Bill Powers frequently comments that he can't imagine a neural system doing something as complex as X (fill in your own value for X), because he thinks of X as an operation in symbolic mathematics, very hard for a computer to emulate, and even harder for a human brain to do through the medium of emulating a logical machine...but very easy or even perhaps basic for a neural system to do. Whether something is feasible on a fast enough computer is quite a different issue.

Martin

Date: Tue, 17 Dec 1996 17:50:07 +0000
From: Peter Cariani <peter@EPL.MEEI.HARVARD.EDU>
Subject: Re: Aristotle and Contingent Truths

[From Peter Cariani (961217.2000 EST)]
>
> [From Bill Powers (961216.2100 MST)]
>
> Peter Cariani (961216) --
>
> Your Declaration of Epistemology rings bells with me all the way through. A
> beautiful and eloquent statement.
>
> I would change only one little thing to remove the last taint of naive
> realism. You say:
>
> >Pragmatism begins with the limited observer and examines the
> >ways (adaptive mechanisms, variation and selection) that

> >limited beings are able to gain greater influence (dare I
> >say control?) over their surrounds.
>
> If you will just say "over their perceptions" the picture will be complete.
> If you want to speak purely, you will eliminate terms that make claims to be
> about that which we cannot know directly. All such claims beg the basic
> question of epistemology.

Fine. You're more fastidious about this language than I (and I understand the reasons for it). To the extent that limited beings can influence their surrounds (neither we nor they understand <<exactly>> how they do this, since neither we nor they have complete access to all of the aspects of the world that come into play),they can control their perceptions. I could have said, more poetically, "control of their destinies", but at the risk of being misunderstood.

> > Epistemology is not
> >the study of how "true knowledge" is acquired; it is the
> >study of how organisms and devices are able to acquire
> >pragmatic knowledge, to better adapt to the world, to
> >improve performance, to achieve goals. No gods. No
> >orphic mystery cults. No "best seat in the house."
> >Just good clean fun.
>
> Again, there's that lurking claim to knowledge of "the world." The world
> that we come to know and learn to control is the world of perception. The
> "other" world is hypothetical, however attractive and useful that hypothesis
> may be. We can freely accept and use that hypothesis in our practical
> affairs, but when we are looking for a deeper understanding we have to
> identify it as what it is: a guess.

I think it is possible to talk about an external "world" without claiming to know the true form of that world, the specifics of its structure. By "world" I mean that undifferentiated realm beyond my senses. Otherwise one sounds too much like a solipsist.

Although the means by which we adapt is to control for perceptions on that world, we really are adapting to the world, not to the perceptions of the world. Unsensed aspects of the world can and do kill us, and it is the world at large, not our perceptions of it, that determine how effective our adaptive strategies will be. Even though our perceptions give us the error signals that we use to modify our behavior to (hopefully) further reduce their magnitude, all of the sensed and unsensed aspects of the world interact with all of the sensed and unsensed aspects of our bodies to determine those perceptual signals (and hence the error signals).

I think we're really on the same wavelength here; it's really a matter of rhetoric.

Peter

Date: Tue, 17 Dec 1996 17:46:53 -0500
From: Scott Stirling <scstirli@ANSELM.EDU>
Subject: Re: In Defense of Popper & Harms

Bruce,

There is no need to think about the philosophical problem of

epistemology, according to you yourself. Do you want to deal with it rationally? or ignore it?

Epistemology does not matter to the average guy, but to one who thinks, there is no denying its relevance to anyone doing work in science, philosophy, education, psychology, etc.

> Do my dogs and I posit a common world, or do we share one?
> (Insofar as I know, they are not given to extensive positing.)

So far, Bruce, your argument/proof for the proposition that there is a reality to which your experience corresponds is that you experience it, therefore it exists. How do you know that your experience corrseponds to reality? I, and thousands of thinkers over thousands of years, want to know!

> In a message that seems to have gone astray, I said that if I
> knew you better, I would punch you in the nose, and ask you to
> posit that I had not...
>
>
> Bruce Gregory
>

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Date: Tue, 17 Dec 1996 17:23:22 EST
From: Bruce Gregory <gregory@CF4.HARVARD.EDU>
Subject: Re: Defense of Stirling

[From Bruce Gregory (961217.1725 EST)]

17 Dec 1996 16:52:34 Scott Stirling

> One can be punched in the face and, by sheer force of that sort of
> rhetoric, _believe_ that one has been punched into reality, but there is
> no way to prove deductively that experience of the world is not just
> perception -- period. The nature of inductive logic is such that no
> amount of cases or evidence can prove an argument or theory--it can only
> make a them sounder.

If you feel that need your experience of the world to be proved either deductively or inductively, take two aspirins, lie down, and call me in the morning. I take that back. Clearly Scott Stirling is simply something I posit. _I_ will take two aspirins and lie down...

> Solipsism is a theory that may be swiftly dispatched. Due to
> consistency of my perceptions and attempts to test the theory that I am

> the only entity that exists, I have concluded, satisfactorily, that even
> if I am the only being in existence, it behooves my emotional state and
> other senses, rational and physical, to act as if the other people I
> perceive do actually exist. Since this is so, solipsism does not seem to
> increase the fit between my perceptions and my reason, so there is no
> need for it.

We solipsists have got to stick together ;-)

Bruce Gregory

Date: Tue, 17 Dec 1996 17:50:02 EST
From: Bruce Gregory <gregory@CF4.HARVARD.EDU>
Subject: Re: Defense of Stirling

[From Bruce Gregory (961217.1750 EST)]

Tracy Harms 1996;12,17.15:30

> Just a quick technical point: additional cases, by themselves, do not even
> make a theory stronger. Induction has no strength whatsoever.

You've been reading too much Popper. Most of what we believe we
believe on the basis of induction. Induction cannot lead to
certainty, but then what can?

Bruce Gregory

"Solipsists of the world unite. You have nothing to lose!"

Date: Tue, 17 Dec 1996 15:43:55 -0800
From: Ryan Cassidy <rmc10@AXE.HUMBOLDT.EDU>
Subject: Re: Aristotle and Contingent Truths

        DELPHINIUM BLUE IS COOL

Date: Tue, 17 Dec 1996 19:09:36 +0000
From: Peter Cariani <peter@EPL.MEEI.HARVARD.EDU>
Subject: Re: Lights, buttons, Christmas decorations

[From Peter Cariani (961217.1700 MST)]

> [From Bill Powers (961217.0700 MST)]

> I'm just an ignorant engineer, not a philosopher. ......
>
> Here's how I look at perception. It's nothing fancy or deep; it's just the
> way an engineer deals with appearances, which includes what we seem to know
> about nervous systems, physics, and all that realist stuff.

C'mon Bill, we're not fooled by this "Oh, I'm just a lil' simple engineer just puttin' the nuts and bolts where they need to go" stuff. You've thought about the metaphysics as much as anyone.

> So it looks to me as though the brain is the knower and actor, or at least
> an indepensible part of the process of knowing and acting, and that what it
> knows comes to it in the form of neural signals from sensors, which are also
> indispensible.
>
> If that's so, then the engineer would want to know what all those sensors
> are detecting: what gives rise to the neural signals? It seems that visual
> receptors generate signals in response to the absorption of light quanta of
> particular wavelengths by photosensitive nerve-endings; that olfactory
> signals are generated by chemical reactions at the surfaces of other
> receptors; that tactile signals are generated by mechanical distortions of
> sensors; that auditory signals are generated by the bending of hair cells in
> the cochlea which distort auditory sensors -- and so on for all the sensors
> that there are. Each sensor is affected by local physical or chemical
> effects. Each sensory signal arises from physical effects in a tiny region
> immediately outside the sensory nerve-ending that generates the signal.

A good engineer also knows that all sorts of factors contribute to the sensor reading (temperature, humidity, magnetic fields, etc. etc. etc.), factors that may not be what the sensor is thought to be sensing. We NEVER know all of the subtle factors that can affect the operation of a real world sensor. All we can do is calibrate to achieve rough congruence with other sensors.

> When we look at any one sensory signal, what kind of potential knowledge
> about the world do we find there? There seems to be a more or less simple
> relationship between the amount of physical or chemical effect on the
> sensory ending and the rate at which that ending generates impulses. Some
> sensors simply generate signals that depend monotonically on the intensity
> of the physical stimulation. Others respond in an exaggerated way to changes
> in intensity, but also show a steady-state response of a smaller magnitude.
> A few sensors seem to "adapt" in the sense that under steady stimulation
> their neural signals eventually die out altogether.

It's my turn to be persnickety. Go look at the primary literature (throw out your textbooks) -- look at any sensory system, what the neural spike trains really look like, and you'll find that it's much, much messier than that. In most systems the sensors are broadband at moderate stimulus levels, which means that they respond to lots of things with comparable discharge rates. In olfaction the situation is truly nightmarish -- each primary sensory neuron responds to an idiosyncratic gamut of different odorants and odorant types, and the response is highly history-dependent. [Compared to making sense of this mess, making a cross-correlation model of speech recognition is a snap! Not to say it's not hard, only to give a sense of how much more difficult it is to make a coherent picture of olfaction based on averate spike rates.]

> For each sensory signal, measured in terms of its frequency of firing, we
> can ask what a steady magnitude of that signal would represent in the world
> outside it. If the sensor adapts over time, a steady signal would have to be
> generated by a steadily-increasing amount of stimulation. For the static
> type of sensor, a constant magnitude of signal would correspond to a
> constant magnitude of stimulation. For a mixed type of receptor, a constant
> signal would go with a stimulation that increases exponentially at a certain
> rate to an asymptote.
>
> So we can say that each neural signal emitted by a sensory receptor

> represents by its magnitude the state of some physical process going on in
> the immediate vicinity of, or even inside, the sensory nerve ending. That is
> the basic stuff of the initial signals. As those signals vary, they indicate
> variations in the local stimulation of the individual nerve-endings.
>
> An engineer can understand this sort of arrangement, because there are
> counterparts of all the kinds of sensory receptors in artificial sensors
> like microphones, photocells, strain gauges, chemosensors, and so forth.
> Each device generates an electrical signal that depends on local effects of
> the environment: the absorption of light quanta by a photocathode, the
> displacement of a crystal or a coil in a magnetic field, the elongation of a
> wire that changes its resistance to current flow, or chemical reactions that
> liberate ions that can be detected electronically.

If only the nervous system were built this way, with discrete narrow-band sensors that behave in a monotonic manner with nice separation of the lines for different kinds of stimulus properties. On the very coarse scale of sensory modality it is organized this way, but within each modality, many of the sensors look much more broadband and much of the information looks like it is being mixed together (multiplexed) in some way. If the nervous system were anything like the picture that you paint, I guarantee to you that we would understand how it all works by now. We build things this way because we understand how to build things this way, and we analyze the workings of the brain in these terms because these are the kinds of systems we understand. But it doesn't follow that the nervous system has to be organized this way. It may very well be, mark my words, that we need substantially different ways of thinking about how sensory information is handled in the brain.

> In building a device that uses lots of sensors, like a spacecraft, the
> engineer can see that there is a whole layer of signals, each coming from a
> particular sensor, that contain whatever any subsquent signal-processing can
> know about the world that's being sensed.
>
> However, these signals by themselves don't represent anything very useful.
> To measure the velocity of the spacecraft, for example, the signals from
> strain gauges that are indicating a force due to acceleration must pass
> through a time-integrator to produce a new signal that represents velocity.
> And signals from several strain-gauges, each representing acceleration in a
> different dimension (linear or angular) must be passed through a computer
> which can yield a signal representing total magnitude of acceleration, and
> through another producing signals that indicate direction of acceleration.
> The signal from the photocell in a star tracker must be correlated with
> signals indicating direction of pointing relative to the spacecraft, so a
> new signal indicating pointing error can be generated.
>
> And so on. To the engineer, it's clear that what HE can see going on in the
> environment is of no use at all to the systems he's building. Whatever those
> systems do, they can do only on the basis of what the systems' own sensors
> are telling them, and what the systems' own analytical capabilities can
> derive from the multiple sensor readings. The systems have no other way to
> know what is going on outside them.
>
> The engineer, who can see a lot more than the system he's building can see,
> can give the system a lot of help by arranging its sensors in the most
> useful way, and by combining signals in just the way that will correspond
> the most usefully to external phenomena. But every now and then, the
> engineer might pause to think about the analogy between his own internal
> systems and those of the system he's constructing. The first thing he will
> have to realize is that there is no engineer standing by outside to tell his

> internal systems what the signals from the sensors mean, or what the best
> way of combining them into new signals would be.
>
> In fact, the engineer might find it somewhat confusing to imagine himself
> being the system he is constructing. The engineer, when not identifying with
> the system, knows that a certain sensory signals represents an effect of
> acceleration, but inside the system there is no label on the signal; the
> system can know only that there's a signal of a certain magnitude. And when
> that signal goes through a time integrator and turns into a velocity signal,
> again there's no label saying that it represents velocity. In fact, whatever
> the system "knows" about its environment must ALSO exist as a neural signal,
> an unlabeled signal.

Engineers (and software engineers particularly) do think from the inside out -- they think about what kinds of information a given module "knows" about, what it needs to "know" to do its job, etc. etc. I think it's a useful way of thinking....to take the perspective of a device more limited than oneself. This is exactly opposite from the tendency of some physicists to ascend to godhead.

The system may not have a label that says "effect of acceleration" on it, but the wiring of the system (if its a device) or the manner in which the information is encoded (if its a program) needs to keep track of the type of information that is associated with the magnitude (just as we keep the units for variables alongside
their magnitudes in physics and stoichiometry problems). Somehow different types of information need to be kept separate (by labelled lines or time-codes, however) if the system is to have a prayer of being coherently organized. Keeping the lines separate, though, doesn't require any knowledge of what they "mean" in terms of the
outside world (although maintaining the separation of the lines maintains a causal linkage with the sensors and their interactions with "the world"), only that
the different lines represent different types of information.

> Enough of this sort of cogitation might lead the engineer -- me -- to devise
> an analogy that goes like this:
>
> Imagine that you find yourself in a control room in which the walls are
> covered with lights and buttons. Nothing is labeled. Your abilities consist
> of the ability to look at the lights and to push any buttons that strike
> your fancy. Having nothing else to do, you watch the lights and push the
> buttons.
>
> The first thing you discover is that pushing the buttons always has an
> immediate effect on certain lights. Other lights may be affected, but they
> aren't always affected. One button always makes one set of lights turn on;
> another button makes a different, but partially overlapping, set of lights
> turn on. Figuring out which buttons always affect which lights takes a
> while, but eventually you have the connections memorized; you can make any
> of those lights turn on whenever you please because you know which buttons
> to push.
>
> While you've been doing this, there have been certain effects that seem to
> come and go at random. Sometimes you feel good, sometimes you feel bad.
> After while, when you've pretty much got the buttons mastered, you realize
> that if you turn on one set of basic button-lights, sometimes another set of
> lights turns on and at the same time you feel bad -- or else good, depending
> on which lights turn on. So you try to figure out how to keep the

> bad-feeling lights from turning on, and how to keep the good-feeling lights
> from turning off. In doing this, you begin to recognize lights that you've
> seen before, and groupings of lights. They become familiar things. You learn
> how to turn them on and off by using combinations of the button-lights.

Here the different kinds of information are being kept separate by the respective positions of the lights (a "place" code). But it's just as plausible that the lights are blinking in different time patterns and you find that certain time patterns make you feel good or bad, but it doesn't really matter which particular lights those time patterns come in on. But this is orthogonal to your point.

> After long enough at doing this, you begin to use shorthand. Instead of
> turning on a set of 100 lights, you find that you can just attend to three
> or four lights in the set, and by turning them on you turn on the whole set.
> Turning on the three or four lights becomes equivalent to turning on all the
> lights in the set. You have constructed a name for the whole set, using a
> subset as a symbol for the whole set.
>
> Now you can start reasoning in terms of the names: if set .:. is on, and set
> ::.. is off, or if ::.. is on and .:. is off, then :.:.. will be on. If
> :.:.. feels good, you will try to manipulate .:. in relation to ::.. so that
> the logical function (.:. XOR ::..) remains TRUE. You learn to like .:.ing,
> and also ::..ing, but not when they're going on at the same time.
>
> In this way you can build up a world made of different patterns of lights,
> and learn to control them to avoid the bad and seek the good, and use
> subsets to name them and reason about them, and do everything that human
> beings do with perceptions -- all without the slightest idea of what is on
> the other side of the panels of lights and buttons.
>
> That's my epistemology. We can poke the buttons and watch the lights, but
> the connections between the buttons and the lights, on the other side of the
> display panel, are invisible to us. We can be pretty confident that there
> ARE connections on the other side, but we can only guess at what they are on
> the basis of how the buttons, singly and in simultaneous and sequential
> combinations, affect what we can see.

Yes, of course. Nice example. Not bad for a plain ol' engineer like yourself!

Best wishes,
Peter

Date: Tue, 17 Dec 1996 18:54:58 -0700
From: Bill Powers <powers_w@FRONTIER.NET>
Subject: Re: Popper's Preposterous Propositions

[From Bill Powers (961217.1800 MST)]

Tracy Harms 1996;12,15.21:30 --

>>I think that a good deal of our difficulty with Popper arises from the
>>fact that he was most closely associated with the _life_ sciences, where
>>knowledge has been largely statistical in nature (for a number of reasons,
>>including wrong theories).
>
>This is just not so. Popper's main attention was to *physics*; the circles
>of scientists he frequented were mostly composed of physicists, and he made

>significant explanatory efforts regarding interpretation of quantum
>mechanics. One of the ways in which Popper is a bit frustrating to me is
>the way his frame of mind was shaped so much more by physics than life
>sciences.

Don't know where I got that idea, but I guess it's wrong. Actually, I was
going to say that his ideas would also apply in fields like quantum physics,
but didn't because I had heard of his work only through psychologists.

>>[Consider] an engineer doing strength-of-materials tests of a
>>design for a bridge [...] The engineer has to seek out
>>that knowledge, for some purpose of his own. But he can't get that
>>knowledge without interacting with the frame member; the knowledge
>>is there, waiting to be got if he wants it, but it will stay there
>>forever if nobody looks for it.
>
>The *stuff* is there, and with it the potential for knowledge. But the
>knowledge is not there; it is not extracted from, but instead constructed
>in relation to, that stuff.

Really, I agree with this, that knowledge is constructed. But it's also
constrained by observation. That is, you can't just "know" any old thing
that you'd like to know.

>>[...]
>>Naturally, unpredicted failures do occur, but they don't occur by chance.
>>They occur because of either lack of knowledge, or deliberately ignoring
>>knowledge (often to save money).
>
>One point of difference I think I detect beteen us is that you seem to be
>unaware of what Peter Munz calls false knowledge. This is a fairly new
>concept, not much percolated through the literature, and it certainly is
>counterintuitive to most people's presuppositions about knowledge. But the
>necessity of clarifying truth as distinct from (though related to)
>knowledge requires this odd combination. This is especially so for
>intellectual knowledge. That's just a teaser; I'll suspend the topic now
>that I've raised a bit of curiosity...

Better expand on that. Sounds like a round square to me.

Best,

Bill P.

Date: Tue, 17 Dec 1996 18:55:03 -0700
From: Bill Powers <powers_w@FRONTIER.NET>
Subject: Re: Lights and buttons. Re: In Defense of Popper & Harms

[From Bill Powers (961217.1815 MST)]
Bruce Gregory (961217.1700 EST) --

>> I'm just an ignorant engineer, not a philosopher.
>
>That "aw, shucks" approach is charming, but will get you nowhere
>with this audience.

Oh, good, you mean I AM a philosopher?

> ... if neither of us
>had any experience to begin with, neither of us would need a
>model to explain it. The experience is prior to (in a logical
>as well as chronological sense) the models used to explain it.

Right. Experience is directly perceived.

>I don't "infer" the existence of an external world, I encounter
>it whether I want to or not.

Just leave out "external" and we'll get along fine. "External" is an interpretation.

Best,

Bill P.

Date: Tue, 17 Dec 1996 18:55:06 -0700
From: Bill Powers <powers_w@FRONTIER.NET>
Subject: Re: Lights and buttons. Re: In Defense of Popper & Harms

[From Bill Powers (961217.1820 MST)]

Tracy Harms 1996;12,17.15 --

>You have reiterated the common-sense view of knowledge which, while
>plausible on the face of it, results in intractible explanatory
>difficulties.

I was trying to develop those intractible explanatory difficulties, but wandered off the track before I could get to the boffo finish. I was distracted by personal matters.

The idea was that this simple engineer who just takes the world as it appears to be ends up realizing that if he is also constructed in the way his artificial system is constructed, then all perceptions of and ideas about the world, including the apparent laws of physics, chemistry, and neurology, must reside in the signal-processing systems in the brain, and in fact that the brain itself exists in the world of experience as an idea, a model. Even the division between "inside" and "outside" is a product of how we think, how we classify experiences. There are many yogas, but they are all the same yoga. I've heard similar rumors about the Tao. Meaning you can start from anywhere, even naive realism, and still end up with the same understanding _if you carry it far enough_.

See "The Goldbug Variations" by Richard Powers (no relation).

The only reality is experience. All else is contained within that. The "all else" comes up when we start trying to _explain_ experience instead of just experiencing it: that is, when we start trying to create knowledge.

Physics is an attempt to explain experience. The odd thing is that physics presents a picture of a world that we can't experience at all. It tells us that tabletops are mostly empty space, that something called "light" travels from the objects we can see into our eyes, that waves of compression carry sounds from other people and other events into our ears, that the universe

is made of quarks, and that when we strain to lift something too heavy for us, we're not doing any work. Physics purports to be about a world that underlies our perceptions, that lies outside direct experience. And the oddest thing about this is that physics explains a lot of experiences very, very well, with incredible accuracy, while direct experience offers no explanations at all. Direct experience allows us only to extrapolate from the past to the future. It can't predict anything that hasn't been experienced before.

When Dr. Johnson kicked the rock and said "I refute you _thus_," he was looking only at the world of direct experience. He didn't notice that the rock acquired an upward and sideward velocity, and for some inexplicable reason returned to earth. He didn't notice that the rock compressed slightly and rebounded from his foot, and was, in fact, elastic (although less so than his foot). He didn't notice that when the rock landed, it spun for a while on one point before beginning to wobble and finally falling over. What Dr. Johnson did notice were only the major things with which he was familiar; he never saw that behind these events and relationships something completely unexplained was going on. He didn't see that there are invisible constraints and connections that make one event depend on another by means that are completely hidden from human perception. Dr. Johnson was not a physicist.

I can't prove it, but I stake my life a dozen times a day, even just walking down a flight of steps, on the assumption that there is a world beyond the world of direct experience. When I look at a stick angling out of a pond, I "know" that it isn't "really" bent, even though direct experience tells me that it is. When I flip a light switch, I "know" that there are wires in the wall and a power plant somewhere, which explain why the light goes on -- even though those things are not and may never be part of my direct experience. Sometimes we get to pry the lid off the black box by just a crack, and sure enough we find mechanisms inside it that may or may not conform to what we guessed was in there -- but there is always SOMETHING in there, not a void. Even the quantum physicists find that this is so; every time they peer deeper into the box, they find something, not nothing. Every time they find a "fundamental particle" they find, sooner or later, that it is made of still more "fundamental" particles. They think that even nothing is something, briefly, every now and then. I suspect that this process is endless.

I would never go so far as to say that the world of physics is the "real" world, while the world of experience is an illusion. The world of physics is an idea that exists in the world of experience -- I almost said "in our heads." Its only purpose is to explain experience, and the world it has created is imaginary in the technical sense of the word. Yet, if we want to talk about inferences and evidence, the existence of another world, outside experience, seems almost a certainty.

Best,

Bill P.

Date: Tue, 17 Dec 1996 20:46:34 -0500
From: Bruce Gregory <gregory@CF4.HARVARD.EDU>
Subject: Re: In Defense of Popper & Harms

[From Bruce Gregory (961217.850 EST)]

Scott Stirling December 17,1996 5:46 PM

> So far, Bruce, your argument/proof for the proposition that there
> is a reality to which your experience corresponds is that you experience
> it, therefore it exists. How do you know that your experience
> corrseponds to reality? I, and thousands of thinkers over thousands
> of years, want to know!

O.K. O.K. Thousands of years is long enough to wait. But you don't have to hold your breath. Peter has already answered your question:

Peter Cariani (961217.2000 EST)]

"I think it is possible to talk about an external "world" without claiming to know the true form of that world, the specifics of its structure. By "world" I mean that undifferentiated realm beyond my senses. Otherwise one sounds too much like a solipsist.

Although the means by which we adapt is to control for perceptions on that world, we really are adapting to the world, not to the perceptions of the world. Unsensed aspects of the world can and do kill us, and it is the world at large, not our perceptions of it, that determine how effective our adaptive strategies will be. Even though our perceptions give us the error signals that we use to modify our behavior to (hopefully) further reduce their magnitude, all of the sensed and unsensed aspects of the world interact with all of the sensed and unsensed aspects of our bodies to determine those perceptual signals (and hence the error signals)."

Thanks Peter. I (obviously) could not have said it better myself.

Bruce Gregory

Date: Tue, 17 Dec 1996 17:00:00 GMT
From: Inman Harvey <inmanh%cogs.sussex.ac.uk@UKACRL.BITNET>
Subject: ECAL97 Final CFP

Full details on our website http://www.cogs.susx.ac.uk/ecal97

FINAL CALL FOR PAPERS
4th. EUROPEAN CONFERENCE ON ARTIFICIAL LIFE
ECAL97
Brighton, UK, July 28-31 1997

This interdisciplinary conference aims to provoke new understandings of the relationships between the natural and the artificial. Artificial Life is often described as the endeavour to synthesize life-like phenomena in artificial media in an attempt to establish a formal and general understanding of life. In practice it is something much broader. At its core are exchanges of ideas and blurring of boundaries between disciplines traditionally constrained to just the natural or just the artificial.

ECAL97 will foster further cross-fertilisation and hopes to extend the Artificial Life community by encouraging contributions from people involved in the Arts and Humanities. The conference will involve oral presentations, both

invited and contributed, poster sessions, debates, exhibitions, demonstrations, installations and related activities.

Scientific topics to be covered will include, but not be limited to, the list below. Contributions from biologists are particularly welcome.

Self-organisation. Origins of Life. Prebiotic evolution. RNA Systems. Fitness Landscapes. Natural Selection. Sexual Selection. Ecosystem evolution. Evolutionary Optimisation. Evolutionary Computation. Immune Networks. Neural Networks. Multicellular Development. Natural and Artificial Morphogenesis. Learning and Development. Communication. Artificial Worlds. Simulations of Ecological and Evolving Systems. Mobile Agents. Autonomous Robots. Evolutionary Robotics. Software Agents. Collective Behaviour. Swarm Intelligence. Cooperation. Evolution of Social Behaviour. Philosophical Issues in Alife. Ethical problems.

NEW --- Publisher will now be: MIT Press/Bradford Books. Papers should not be longer than 10 pages (including figures) in MIT Press format. Format instructions and LaTeX template are available on our web-page. We encourage paper submissions via the Internet (see web-page) though traditional paper is also acceptable (4 hard-copies). Demonstrations, Videos, and proposals for associated workshops are also welcomed.

IMPORTANT DATES
Feb 28, 1997 -- Submission deadline
Apr 12 -- Notification of acceptance
May 1 -- Camera-ready due
May 31 -- Early registration deadline
July 28-31, 1997 -- Conference dates

ECAL97 will be held in Brighton on the South Coast of England. There are good travel connections; it is just one hour by train from London and conveniently close to London Gatwick airport. The conference will be inside the Metropole Hotel on Brighton seafront. Special accomodation rates will be available for those staying at the hotel, with other cheaper accommodation available elsewhere.

Please PRE-REGISTER your interest by filling out a form via our WWW site. All new announcements will be emailed to those who have pre-registered, and will also be available on this site:
http://www.cogs.susx.ac.uk/ecal97/

Conference organizers: Phil Husbands and Inman Harvey
Local organization: Medeni Fordham and Joseph Faith
Conference Secretariat: Medeni Fordham
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PROGRAM COMMITTEE

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NEW -- European Workshop on Learning Robots

EWLR-6, the 1997 European Workshop on Learning Robots, will be held in association with ECAL97 on the day after, Friday August 1st 1997, in the same location, the Metropole Hotel Brighton. The Call For Papers for EWLR-6 will be issued some time in January 1997; for further information contact Andreas Birk cyrano@arti14.vub.ac.be.

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Date: Tue, 17 Dec 1996 19:15:48 -0700
From: "T. B. Harms" <tbh@TESSER.COM>
Subject: too much Popper

*From Tracy Harms 1996;12,17.19:00

Bruce Gregory (961217.1750 EST)

>> Just a quick technical point: additional cases, by themselves, do not even
>> make a theory stronger. Induction has no strength whatsoever.
>
>You've been reading too much Popper. Most of what we believe we
>believe on the basis of induction. Induction cannot lead to
>certainty, but then what can?

The topic at hand is *knowledge*, not belief. The casual confusion of the two is much of what interferes with problem solving in this field of study.

Induction simply does not occur as a logical structure, and use of the word 'induction' to describe psychological tendencies to generalize is ill-advised because it creates an impression to the contrary.

Tracy Bruce Harms tbh@tesser.com
Boulder, Colorado

Date: Tue, 17 Dec 1996 21:18:55 -0500
From: Scott Stirling <scstirli@ANSELM.EDU>
Subject: Re: In Defense of Popper & Harms

Peter Cariani:

"I think it is possible to talk about an external "world" without claiming to
know the true form of that world, the specifics of its structure. By "world" I mean that undifferentiated realm beyond my senses. Otherwise one sounds too much like a solipsist"

Bruce, Bill, Peter, T.B. et al.,

I do not know what else to add at this point. Bill has said it all, and I thought I had too. T.B. has said it all, plus some wacky fringe stuff (we are on the edge of our virtual seats waiting for that false knowledge thing!). I think Bruce has restated his claim several times, and now Peter C. has done the same. Am I missing something here? I understand that one can _think_ that it is possible to know a reality beyond perception--I think that myself. But to know, indubitably, in the face of arguments raised by people like Hume, Berkeley, Kant, and Pyrrho (to name a few)? I am exasperated.

I understand someone telling me, again and again, that there is an external world, and that we all know it, but I haven't heard any unpacking of that assertion. I am not upset, I just hoped more people would take a hard look at Bill Powers message (961217.1820 MST), and if they had problems with it, present their own arguments against specific points, while adding more to the discussion than just restating their original point louder than last time. Sounds whiny, huh?

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Date: Tue, 17 Dec 1996 21:20:41 -0500
From: Bruce Gregory <gregory@CF4.HARVARD.EDU>
Subject: Re: too much Popper

[From Bruce Gregory (961217.2125 EST)]

Tracy Harms 1996;12,17.19:00

> The topic at hand is *knowledge*, not belief. The casual confusion of the
> two is much of what interferes with problem solving in this field of study.
>
> Induction simply does not occur as a logical structure, and use of the word
> 'induction' to describe psychological tendencies to generalize is
> ill-advised because it creates an impression to the contrary.

Point taken.

Bruce Gregory

Date: Tue, 17 Dec 1996 21:25:27 -0500
From: Bruce Gregory <gregory@CF4.HARVARD.EDU>
Subject: Re: Lights and buttons. Re: In Defense of Popper & Harms

[From Bruce Gregory (961217.2130 EST)]

Bill Powers (961217.1815 MST)

> Bruce Gregory (961217.1700 EST) --

> >I don't "infer" the existence of an external world, I encounter
> >it whether I want to or not.
>
> Just leave out "external" and we'll get along fine. "External" is an
> interpretation.

You've got yourself a deal.

Bruce Gregory

Date: Tue, 17 Dec 1996 22:54:10 -0700
From: "T. B. Harms" <tbh@TESSER.COM>
Subject: RANT: mostly empty space?

*From Tracy Harms 1996;12,17.22:40

Bill Powers (961217.1820 MST)

>The odd thing is that physics presents a picture of a world
>that we can't experience at all. It tells us that tabletops
>are mostly empty space [...]

I don't think physics tells us that, although far too many *physicists* seem to. Physics tells me that the space in a tabletop is mostly electromagnetic fields which are quite rigidly interrelated as various interlocking solids. It tells me that it is statistically only slightly less of an impediment than empty space *if I happen to be a neutrino*. It also tells me that if I happen to be a complex colloidal system (which, at least by contrast to neutrinos or x-rays, I am) that field-filled space is no less dense than my touch indicates.

It may be electromagnetic, but it aint empty, and I'm pretty much sick and tired of the "cleverness" by which it has become habitual to say that it is. That's not meant to hit you in particular, but rather is a mini-rant against the whole trend of elevating the dynamics at the ultra-micro scale as somehow more genuine, more physical, than what transpires in and around the scale of our bodily experience, where meters-per-minute is a handy measure.

Tracy Bruce Harms tbh@tesser.com
Boulder, Colorado

Date: Tue, 17 Dec 1996 23:20:22 +0000
From: Richard marken <marken@AERO.ORG>
Subject: A grain of salt

[From Rick Marken (961217.2210)]

Tracy Harms (1996;12,17.15) --

> Rick Marken (961215.2150) asks: ''But once people have found PCT,
> why shouldn't they drop off the "wise men" at the college center
> while they attend classes on their own?'' It is not the mentors
> who tag along, it is their respective contributions, and either
> these are incompatible with PCT (in which case we do well to
> identify which one looks worse) or they have compatabilities which
> are worth discovering. (Sorry, Rick, but your call sounds all
> too much like a wish that people would check their minds at the
> door...)

!!!!! Actually, I meant it as a wish that people would check their preconceptions at the door and enter the halls of PCT with just their skeptical little minds. I know that many people like to determine the degree to which the contributions of their various mentors are "compatable" with PCT. But I have never found this to be a particularly useful exercise. The main problem is that people _want_ to find compatabilities, whether they are there or not, when they like both the mentor _and_ PCT. We have seen some rather remarkable attempts to see PCT compatabilies in the contributions of mentors who were about as far from PCT as they could be.

There is nothing _wrong_ with finding compatibilities between statements made by some mentor and and the ideas of PCT. I've used such compatibilities myself to give a scholarly garnish to a description of PCT (see my paper called "A science of purpose" in the _American Behavioral Scientist_ (Sept/Oct 1990). But I think that attempts to learn PCT by looking for compatabilities with the contributions of mentors is the wrong way to go. I think (based on my own experience) that the only way to learn PCT is to forget about everything you _think_ you know about how behavior (and mind) works and start from scratch -- building and _testing_ models of purposeful behavior. I think that one of the best place to go to start learning PCT is the series of articles Bill Powers published in BYTE in 1979 (June through September, I think). Bill shows how to build a computer model of a basic control system and how to do experiments to test the model.

The revolution that is PCT exists in the most basic facts about how the simplest control systems work. All control systems act to control a perceptual representation of some physical state of affairs; behavior is the control of perception. No mentor (other than William T. Powers) has understood, let alone tried to build a science and an epistemology based on, that basic fact about behavior. I have never seen any evidence that any mentor had any notion that behavior was anything other than responses caused (or "controlled" or "selected") by internal or external events.

Some mentors (like William James) have said things that are remarkably compatable with PCT. But these mentors did not understand how control of percpetion worked. Thus they were not able to build a systematic science of behavior around control of perception. So we have some phrases here and there that sound like PCT. But there phrases are found in books most of the ideas have little to do with or are flat out contradicted by PCT.

I think reading "mentors" can be interesting and even informative. But I think the contributions of these "behavioral/philosophical" mentors can now be taken in the same spirit as we take the contributions of "alchemist" mentors; with a grain of salt.

Best

Rick

Date: Tue, 17 Dec 1996 23:41:28 -0800
From: David Wolsk <dow@PINC.COM>
Subject: words,words,words

22:55 PST 17/12/96 David Wolsk wrote:

>Given the penchant of conventional psychologists to use the word
>"environment" to mean "perception", perhaps Bill's "answer" to the
>conventional claim that "behavior is controlled by the environment"
>should have been "behavior: the control _of_ the enviroment".

In this always recurring type of discussion I've always been left with a sense of unease. I suspect that the underlying problem may derive from mixing words and concepts from the life sciences with those from engineering. I came to PCT after some years of neurophysiology and psychophysics research. The terminology for PCT which says it all for me is as follows:

The task of the organism is coordinating neural and muscular input and output relationships utilizing negative feeback and CNS hierarchies for maximizing predictability and control.

I doubt if this set of words work for anyone else but they do it for me.

To switch topics, I've been fascinated by the way I used to simultaneity of switching a ligt switch and hearing a noise to illustrate a point. Ever since, aside from the first response long ago, that light switch keeps being turned on and its original illustrative purpose is left in the dark.

On Popper, his book, "The Open Society and its Enemies" was quite important in my early education. his views on Israel certainly seem predictive of today's intransigencies.

David Wolsk
Victoria, BC Canada

Date: Wed, 18 Dec 1996 11:22:44 +0100
From: "J.A. Blom" <J.A.Blom@ELE.TUE.NL>

Subject: reply to Martin: combining goals

[Hans Blom, 961218]

(Martin Taylor 961217 10:30)

>Suppose I am controlling for x=3, y=2, and z=1. You can
>mathematically say that I am also controlling for the top-level goal
>q (==x+y+z) = 6. [I use == to mean "is defined as"]. You can treat
>q=6 as a supergoal.

No, you cannot. There is no mathematical equivalence. For one thing, the dimension isn't right. Controlling for x=3, y=2, and z=1 simultaneously (!) sets as the goal the point (x,y,z) = (3,2,1) in three-dimensional space. Controlling for x+y+z=6 (in that same three-dimensional space) is something very different; the goal is now a large subspace rather than a single point. You can _not_ simply replace "control for x=3 and for y=2 and for z=1" by "control for x+y+z=3+2+1".

When you want to combine goals, you must proceed differently, for instance as I did in my post. Start by replacing "control for x=3" by "minimize (x-3)^2".

[Note: Actually, expected values or time integrals ought to be inserted as the things to be minimized, but let's skip those mathematical niceties; other functions than a square are possible as well, and they make a difference, as my previous post suggests.]

You can now replace "control for x=3 and for y=2 and for z=1" by "minimize (x-3)^2 + (y-2)^2 + (z-1)^2". Which you can translate back into "control for the minimum of (x-3)^2 + (y-2)^2 + (z-1)^2", if you want. This is how you find the "top" goal.

[Note: Actually, different weights can be attached to the individual terms, as in "minimize (x-3)^2 + 2 * (y-2)^2 + 5 * (z-1)^2". Doing so does not change the solution, at least not if there are no constraints in the form of forbidden areas in xyz-space, but it does change the path toward the solution if the solution is to be found by an iterative procedure (such as a control system).]

Constraints are easy to add. Say that z cannot be smaller than 2. The top goal then becomes "minimize (x-3)^2 + (y-2)^2 + (z-1)^2 | z >=2", where the symbol | means "given that" or "under the constraint that".

[Note: Mathematicians see control as a minimization process. They intuitively translate "control for x=3" by "minimize (x-3)^2". They also recognize that the minimization is actually done in one specific way: by the control system. Thus, the way in which the controller operates acts as one constraint on the minimization process. Further constraints may be due to the properties of the space in which the minimization takes place, e.g. inaccessible areas in xyz-space. Thus, for a mathematician, control by some specific control system is seen as a process of function minimization under specific constraints. Some people see a "fundamental" difference between control processes and equilibrium processes. Mathematicians don't; they see different constraints. Am I still understandable? :-)]

Now that this confusion is cleared, reread your post and check

whether my post answered what your question ought to have been ;-).

Greetings,

Hans

Date: Wed, 18 Dec 1996 06:49:02 -0500
From: Bruce Gregory <gregory@CF4.HARVARD.EDU>
Subject: Re: In Defense of Popper & Harms

[From Bruce Gregory (961218.0650 EST)]

Scott Stirling December 17,1996 9:18 PM
>
> Bruce, Bill, Peter, T.B. et al.,
>
> I do not know what else to add at this point. Bill has
> said it all, and I thought I had too. T.B. has said it all, plus some
> wacky fringe stuff (we are on the edge of our virtual seats waiting for
> that false knowledge thing!). I think Bruce has restated his claim
> several times, and now Peter C. has done the same. Am I missing
> something here? I understand that one can _think_ that it is possible to
> know a reality beyond perception--I think that myself. But to know,
> indubitably, in the face of arguments raised by people like Hume, Berkeley,
> Kant, and Pyrrho (to name a few)? I am exasperated.

Excellent. You are making great progress! Exasperation is a _much_
more compelling argument for the existence of a real world than
either induction or deduction! The key point is that I am not talking
about _knowing_ a reality beyond perception. I am taking about having
no doubt that there _is_ a real world. My "knowledge" is in the form
of conjectures or models that I test against my perceptions. I keep
the models that pass this test and (hopefully) discard the ones that
do not. The fact that most models fail convinces me that there is a real
world. (As does the fact that my cat always sleeps on the chair in
front of my computer.)

> I understand someone telling me, again and again, that there is
> an external world, and that we all know it, but I haven't heard any
> unpacking of that assertion. I am not upset...

How can you be exasperated and not upset?

Bruce Gregory

Date: Wed, 18 Dec 1996 05:07:21 -0700
From: Bill Powers <powers_w@FRONTIER.NET>
Subject: Re: RANT: mostly empty space?

[From Bill Powes (961218.0500 MST)]

Tracy Harms 1996;12,17.22:40 --

>>The odd thing is that physics presents a picture of a world

>>that we can't experience at all. It tells us that tabletops
>>are mostly empty space [...]
>
>I don't think physics tells us that, although far too many *physicists*
>seem to. Physics tells me that the space in a tabletop is mostly
>electromagnetic fields which are quite rigidly interrelated as various
>interlocking solids.

Oh, quibble, quibble. Actually electromagnetic fields are soft and squishy when you look at matter on that scale. I once built an interferometer-based control system for a guy who rules diffraction gratings. He talked about his 7-ton steel engine as if it were made of soft butter. And it was, when you looked at it with an interferometer that could resolve 0.01 wavelengths of neon light (he's now down to 0.002 wavelengths and is complaining because the control system contains visible error signals).

Ain't nothin' in this world that's "rigid."

But it's always nice to get a pet rave off your chest.

Best,

Bill P.

Date: Wed, 18 Dec 1996 05:07:24 -0700
From: Bill Powers <powers_w@FRONTIER.NET>
Subject: Re: Lights, buttons, Christmas decorations

[From Bill Powers (961218.0600 MST)]

Peter Cariani (961217.1700 MST)--

>C'mon Bill, we're not fooled by this "Oh, I'm just a lil' simple engineer
>just puttin' the nuts and bolts where they need to go" stuff. You've
>thought about the metaphysics as much as anyone.

Sure, but my handicap is my poor scholarly education in this field. I don't know what who else said about what else. It's best to play the ignorant engineer because that's basically what I am.

>A good engineer also knows that all sorts of factors contribute to the
>sensor reading (temperature, humidity, magnetic fields, etc. etc. etc.),
>factors that may not be what the sensor is thought to be sensing. We NEVER
>know all of the subtle factors that can affect the operation of a real
>world sensor. All we can do is calibrate to achieve rough congruence with
>other sensors.

You betcha. Any sensory neuron can be stimulated in a lot of ways that our standard classifications ignore, and they all look alike to the neuron. Ambiguity in perception starts at the first level.

>It's my turn to be persnickety. Go look at the primary literature (throw
>out your textbooks) -- look at any sensory system, what the neural spike
>trains really
>look like, and you'll find that it's much, much messier than that. In most
>systems the sensors are broadband at moderate stimulus levels, which means
>that they respond to lots of things with comparable discharge rates. In

>olfaction the situation is truly nightmarish -- each primary sensory neuron
>responds to an idiosyncratic gamut of different odorants and odorant types,
>and the response is highly history-dependent. [Compared to making sense of
>this mess, making a cross-correlation model of speech recognition is a
>snap! Not to say it's not hard, only to give a sense of how much more
>difficult it is to make a coherent picture of olfaction based on averate
>spike rates.

Re the "broad-band" assertion, see the comment about 4 inches below.

Some of the problem here, from the HPCT perspective, is failure to recognize levels of perception. For any higher-level system there are many combinations of lower-level input signal magnitudes that will yield the same higher-level perception. That's what "invariant" means. When you poke an electrode into a mass of higher-level systems (not knowing whether you're looking at perceptual signals, reference signals, error signals, or output signals), and try to correlate measurements at that level with first-order stimulations, you're bound to find a confused mess (whether you calibrate your instruments in impulses per second or milliseconds per impulse interval).

>If only the nervous system were built this way, with discrete narrow-band
>sensors that behave in a monotonic manner with nice separation of the lines
>for different kinds of stimulus properties.

Two problems with this. First, I don't posit narrow-band signals, but wide-band signals. Frequency can vary at rates right up to the limit set by the discreteness of the impulses and their minimum refractory period. The world is _dynamic_ and perceptual signals, therefore, are also dynamic. Martin Taylor would say that frequency can vary up to the Nyquist limit set by (twice) the refractory period.

Second, different stimulus properties, as mentioned above, are detected at different levels (I would rather say "invented" or "created"). If you look for the correlate of a stimulus property at too low a level in the hierarchy, you'll find ambiguous or only statistical relationships. You have to find the level where that property is finally extracted before you can find a signal that varies directly with it (if you've defined it correctly).

>On the very coarse scale
>of sensory modality it is organized this way, but within each
>modality, many of the sensors look much more broadband and much of the
>information looks like it is being mixed together (multiplexed) in some >way.

If that's what you find, I would seriously suggest that you're looking at the wrong correlates of the signals, or looking at signals at too low a level in the hierarchy to go with the correlates you're interested in. It isn't that the information is "mixed together." It's still all spread out over different channels, and won't make sense until some input function receives those scattered signals and converts them into a higher-level signal.

I really differ with you over the use of this image of a "multiplexer". The fact that you have signals with rapidly-varying frequency (or interval, as you please) doesn't indicate that those variations are significant in any one channel, one nerve-fiber. It's the _ensemble_ of nerve-fibers that is carrying the information from which one or more higher systems will later extract higher-level invariants. If there are relationships among various signals at a given level, such as synchronizations or correlations, this fact means nothing perceptually until some higher system, as it were, _notices one of the relationships_ and makes something of it -- namely, a

higher-order perceptual signal representing the relationship. In a given ensemble of perceptual signals at one level, there is an infinity of invariants that could be extracted by a higher-level input function, or at least a very large number. We extract only some of them, the most useful ones. And we extract many of them at the same time, in parallel.

The problem here may just be one of terminology. You've said you're not an engineer. When you say "multiplexed signal" the image that pops into the mind of any electronics engineer -- anyone who actually works with multiplexed signals -- contains a _demultiplexer_ that peels off each of the mixed-up channels of information. To make a demultiplexer work, you need some kind of master synchronization between the receiver and the transmitter, or else special marker signals embedded at regular intervals or using special unique codes in the stream to act as delimiters that the receiver can pick up, or else frequency channels to which the receiver can tune in while ignoring the other channels. Without some kind of planned method for demultiplexing that corresponds exactly to the method by which the signals were originally combined into a single channel, the mixed-up information is just going to stay mixed up. Unless you can specify how the multiplexing and demultiplexing are coordinated, your talk of multiplexed signals is going to strike any communications engineer as having an element of magic in it. If I'm wrong about that, any communications or electronics engineers on CSGnet ought to set me straight before I give Peter apoplexy.

I really think the HPCT concept comes closer to handling the situation you want (whether you like frequency or interval measurements) without your employing what is basically a misuse of a technical term.

>If the nervous
>system were anything like the picture that you paint, I guarantee to you
>that we would understand how it all works by now.

If I were sure you understood the picture I paint -- contrary to some of the things you say above -- I might agree. No, come to think of it, I wouldn't, because the picture I paint is quite complex when you get into the details, and checking it out against the real nervous system would be no picnic.

>Engineers (and software engineers particularly) do think from the inside
>out -- they think about what kinds of information a given module "knows"
>about, what it needs to "know" to do its job, etc. etc. I think it's a
>useful way of thinking....to take the perspective of a device more limited
>than oneself. This is exactly opposite from the tendency of some physicists
>to ascend to godhead.

I'm glad we're back to agreeing again.

>The system may not have a label that says "effect of acceleration" on it,
>but the wiring of the system (if its a device) or the manner in which the
>information is encoded (if its a program) needs to keep track of the type
>of information that is associated with the magnitude (just as we keep the
>units for variables alongside their magnitudes in physics and stoichiometry
>problems). Somehow different types of information need to be kept separate
>(by labelled lines or time-codes, however) if the system is to have a
>prayer of being coherently organized. Keeping the lines
>separate, though, doesn't require any knowledge of what they "mean" in
>terms of the outside world (although maintaining the separation of the
>lines maintains a causal linkage with the sensors and their interactions
>with "the world"), only that the different lines represent different types
>of information.

Back at the beginning of PCT I worried a lot about this problem. I finally decided, as you did, that the lines have to be kept separate once they are established (that determines the structural organization of the system). But I gradually came to see that that's ALL that's needed. The lines don't need to be labeled. That's because as far as the brain is concerned, ALL signals are initially meaningless (barring what for human beings is just a few inherited systems). Paul Churchland's "network theory of knowledge" is the answer to the problem, I think: perceptions take on meaning only in terms of other perceptions. And this is accomplished through the acquisition of perceptual functions and their associated control systems. Covariances are converted into invariances by perceptual input functions at various levels, as the levels develop. But nothing has to _know_ that this is happening. We don't perceive our nervous systems, and don't need to.

>> Enough of this sort of cogitation might lead the engineer -- me -- to
>>devise an analogy that goes like this:

>Here the different kinds of information are being kept separate by the
>respective positions of the lights (a "place" code). But it's just as
>plausible that the lights are blinking in different time patterns and you
>find that certain time patterns make you feel good or bad, but it doesn't
>really matter which particular lights those time patterns come in on. But
>this is orthogonal to your point.

It is orthogonal, but there's no reason that the dynamics can't also be extracted as invariants, along with the positional information. When this group blinks in a certain repetitive pattern, it may signify the same thing as when that group blinks in the same pattern. You just funnel the sensations from the two sets of lights into the same pattern-recognizer. You see, I agree that we perceive temporal patterns -- when the correlate is temporally patterned. And that level of perception is already in the HPCT model, as the event level.

>Yes, of course. Nice example. Not bad for a plain ol' engineer like yourself!

Thank you, thank you. For my next number I will do my imitation of the Lesser Grebe (flap, flap, flap).

Best,

Bill P.

End of CSGNET Digest - 17 Dec 1996 to 18 Dec 1996
****

Date: Thu, 19 Dec 1996 08:00:21 -0600
Subject: CSGNET Digest - 18 Dec 1996 to 19 Dec 1996

There are 23 messages totalling 1501 lines in this issue.

Topics of the day:

1. careless button pressing considered harmful
2. A grain of salt
3. Lights, buttons, Christmas decorations (2)
4. words,words,words (2)

Date: Wed, 18 Dec 1996 16:58:31 +0100
From: "J.A. Blom" <J.A.Blom@ELE.TUE.NL>
Subject: careless button pressing considered harmful

[Hans Blom, 961218]

(Bill Powers (961217.0700 MST))

>Imagine that you find yourself in a control room in which the walls
>are covered with lights and buttons. Nothing is labeled. Your
>abilities consist of the ability to look at the lights and to push
>any buttons that strike your fancy. Having nothing else to do, you
>watch the lights and push the buttons

... and get shot on sight. When the shaken guard-on-duty reports to the general in charge, the latter congratulates the guard and sighs: "Well, he knew he'd get shot. He was told, and told, and told."

The incident, like everything else, was filmed. Several years later, after thousands of scientists from all over the world had finished their investigation of the alien spacecraft, someone remembered the accident and the film. And discovered that the careless having-nothing-else-to-doer had had his finger just a fraction of an inch away from the craft's self-destruct. That fraction of an inch was all that had saved the inner planets and all the space colonies up to and even beyond Jupiter from utter destruction.

Be more careful next time, Bill Powers!

Greetings,

Hans Blom

PS: Funny where our phantasy can take us...

Date: Wed, 18 Dec 1996 17:23:16 +0100
From: Stefan Balke <sbalke@HRZ.UNI-BIELEFELD.DE>
Subject: Re: A grain of salt

>From Stefan Balke (961218.1720 CET)

Rick Marken (961217.2210) --
>I think that one of the best place to go to start learning PCT
>is the series of articles Bill Powers published in BYTE in 1979 (June

>through September, I think). Bill shows how to build a
>computer model of a basic control system and how to do experiments
>to test the model.

I would like to read this BYTE-articles, but unfortunately they are not available at our library. Does anybody have an idea how I could get this articles or a copy of them. Or another idea, couldn't they be added to the csg-www-documents?

Curiously, Stefan

Date: Wed, 18 Dec 1996 11:31:44 +0000
From: Peter Cariani <peter@EPL.MEEI.HARVARD.EDU>
Subject: Re: Lights, buttons, Christmas decorations

[From Peter Cariani (961218.1100 EST)]
>
> [From Bill Powers (961218.0600 MST)]
>
> Peter Cariani (961217.1700 MST)--
>
> >C'mon Bill, we're not fooled by this "Oh, I'm just a lil' simple engineer
> >just puttin' the nuts and bolts where they need to go" stuff. You've
> >thought about the metaphysics as much as anyone.
>
> Sure, but my handicap is my poor scholarly education in this field. I don't
> know what who else said about what else. It's best to play the ignorant
> engineer because that's basically what I am.

Not being indoctrinated into the false dichotomies and mushy conflations of mid and late-20th century philosophy is an asset.

> >On the very coarse scale
> >of sensory modality it is organized this way, but within each
> >modality, many of the sensors look much more broadband and much of the
> >information looks like it is being mixed together (multiplexed) in some >way.
>
> If that's what you find, I would seriously suggest that you're looking at
> the wrong correlates of the signals, or looking at signals at too low a
> level in the hierarchy to go with the correlates you're interested in. It
> isn't that the information is "mixed together." It's still all spread out
> over different channels, and won't make sense until some input function
> receives those scattered signals and converts them into a higher-level signal.

Very, very few examples of what might be clean higher-level signals have ever been found (although this is what every cortical physiologist and his brother is looking for). At some point, after looking and looking and looking and trying to force-fit what you see into what you expect you should see, one has to sit in a quiet dark room for a while and re-examine the basic assumptions. Procrustean science is easier than finding an unexpected kind of solution, but at some point the epicycles just don't do it any more.
I'm not accusing you of this, since you're fairly far removed from the nitty-gritty
of how the models are supposed to mesh with the physiology. All of us need to have the ability and the readiness to question our assumptions.

> I really differ with you over the use of this image of a "multiplexer". The

> fact that you have signals with rapidly-varying frequency (or interval, as
> you please) doesn't indicate that those variations are significant in any
> one channel, one nerve-fiber. It's the _ensemble_ of nerve-fibers that is
> carrying the information from which one or more higher systems will later
> extract higher-level invariants. If there are relationships among various
> signals at a given level, such as synchronizations or correlations, this
> fact means nothing perceptually until some higher system, as it were,
> _notices one of the relationships_ and makes something of it -- namely, a
> higher-order perceptual signal representing the relationship. In a given
> ensemble of perceptual signals at one level, there is an infinity of
> invariants that could be extracted by a higher-level input function, or at
> least a very large number. We extract only some of them, the most useful
> ones. And we extract many of them at the same time, in parallel.

> The problem here may just be one of terminology. You've said you're not an
> engineer. When you say "multiplexed signal" the image that pops into the
> mind of any electronics engineer -- anyone who actually works with
> multiplexed signals -- contains a _demultiplexer_ that peels off each of the
> mixed-up channels of information. To make a demultiplexer work, you need
> some kind of master synchronization between the receiver and the
> transmitter, or else special marker signals embedded at regular intervals or
> using special unique codes in the stream to act as delimiters that the
> receiver can pick up, or else frequency channels to which the receiver can
> tune in while ignoring the other channels. Without some kind of planned
> method for demultiplexing that corresponds exactly to the method by which
> the signals were originally combined into a single channel, the mixed-up
> information is just going to stay mixed up. Unless you can specify how the
> multiplexing and demultiplexing are coordinated, your talk of multiplexed
> signals is going to strike any communications engineer as having an element
> of magic in it. If I'm wrong about that, any communications or electronics
> engineers on CSGnet ought to set me straight before I give Peter apoplexy.

The multiplexing you are thinking about is time-division multiplexing of the kind used in telephony. Time-division multiplexing is the concept that underlies virtually all of the current discussions about segmentation and binding in the neurosciences (to explain the GRay and Singer findings of correlated responses in different parts of the visual cortex and how these might be used to group responses that make up the perception of a unified object). The notion of time-division multiplexing preserves the coding of pattern by which particular neurons are firing (a "place" code) -- it just says that in the time slot (t0, t0 + Delta-t) the place pattern goes with object A whereas in the next time slot it may go with object B.

There is also frequency-division multiplexing where signals are carried in different frequency bands (like radio), and this is the general kind of multiplexing that I am making reference to. I suppose we could call it "temporal pattern-division multiplexing", but maybe it's just easier not to use the M-word here.

> >The system may not have a label that says "effect of acceleration" on it,
> >but the wiring of the system (if its a device) or the manner in which the
> >information is encoded (if its a program) needs to keep track of the type
> >of information that is associated with the magnitude (just as we keep the
> >units for variables alongside their magnitudes in physics and stoichiometry
> >problems). Somehow different types of information need to be kept separate
> >(by labelled lines or time-codes, however) if the system is to have a
> >prayer of being coherently organized. Keeping the lines
> >separate, though, doesn't require any knowledge of what they "mean" in
> >terms of the outside world (although maintaining the separation of the

> >lines maintains a causal linkage with the sensors and their interactions
> >with "the world"), only that the different lines represent different types
> >of information.
>
> Back at the beginning of PCT I worried a lot about this problem. I finally
> decided, as you did, that the lines have to be kept separate once they are
> established (that determines the structural organization of the system). But
> I gradually came to see that that's ALL that's needed. The lines don't need
> to be labeled. That's because as far as the brain is concerned, ALL signals
> are initially meaningless (barring what for human beings is just a few
> inherited systems). Paul Churchland's "network theory of knowledge" is the
> answer to the problem, I think: perceptions take on meaning only in terms of
> other perceptions. And this is accomplished through the acquisition of
> perceptual functions and their associated control systems. Covariances are
> converted into invariances by perceptual input functions at various levels,
> as the levels develop. But nothing has to _know_ that this is happening. We
> don't perceive our nervous systems, and don't need to.

I went through a similar exercise of a person seated in a control room with
alot of lights that flash (maybe we imagine this as the control room of a
nuclear power plant or submarine). Even if the signals are "meaningless" in
the sense that you have no idea how they are connected to things going on
in "the external world", you need to keep signal A separate from signal B
over time. Let's say that you do build up associations, correlations and
anticorrelations between the lights going on and off (and we'll assume for the
sake of avoiding digressions that the lights are place-coded, light A goes
with signal A and light B goes with signal B, i.e. light A is wired to the
sensor that provides signal A and light B is wired to sensor/signal B).
To determine whether the wiring matters consider what happens to such a
system if someone rearranges the wires of the control panel:
the system will be incoherent for some time
until it has time to restablish the (new) correlations between the lights.
This is a good way to decide what aspects of a system are essential for
a given kind of code or representation to work -- rearrange the wires,
or temporal patterns, or arrival times and see what happens.

Just because we have an adaptive system that compensates for screwing up the
code (assuming that the wires aren't rearranged again) doesn't mean that the
particular wiring isn't important (it is absolutely critical for this kind of
code).
Note that if one had a temporal code (time pattern A is correlated with one
consequence, time pattern B with another), the system would be insensitive to
the rewiring. [As an aside, in olfaction the receptors and their connections
are constantly turning over (rewiring) every few weeks, so a long-standing
puzzle has been to explain how we maintain stable perceptions of odors if it's
all encoded in rates and connectivity patterns.]

Paul Churchland a decade ago was arguing that observables (and perceptions) are
not needed to do science (or for knowledge). It was an absurd position then and
it's an absurd position now. As far as I know, Carnap was the one who logicized
semantics and made everything into an internal logical relation in a formal
system,
and he was under the platonic spell of Tarski and Godel. This was a reversal
of his sensible inclusion of observation-linked terms in his early work
(the Aufbau). Logicization of everything into hermeneutically-sealed systems
 was the rage in the late 40's and 50's, it went along with symbolic AI,
the idea that perception (connection to an "external world" or "environment")
isn't important; that all you need is logic. If this is the case, all you need
is a computer to do science (No messy experiments! No muss no fuss, just simulate

it all! Contain the universe on your desktop!)

In order for anything to have "meaning" (and I hate these word-games about what "meaning" means -- I try to avoid calling people "philosophers" in polite company, it invites the proverbial punch in the nose reality check, although I do think that philosophical issues are extremely important. Philosophy is too important to be left to the "philosophers".)...

In order to have pragmatic meaning, to have some impact on the realization of one's
goals vis-a-vis the rest of the world,

SOME CONNECTION TO THE WORLD OUTSIDE OF THE ORGANISM OR DEVICE IS ESSENTIAL.

OBSERVATION ("MEASUREMENT") IS ESSENTIAL FOR DOING SCIENCE.

Churchland and others denied that such connections were necessary. How they could do this tells you reams about the sorry state that philosophy has fallen into in the
late 20th century. Maybe they now a decade later see the error of their ways, I don't know. I certainly hope so. We are still only slowly recovering from these profound conceptual errors.

Peter Cariani

Date: Wed, 18 Dec 1996 09:15:19 -0800
From: Richard Marken <marken@AEROSPACE.AERO.ORG>
Subject: Re: words,words,words

[From Rick Marken (961218.0915)]

Me:

>Given the penchant of conventional psychologists to use the word
>"environment" to mean "perception", perhaps Bill's "answer" to the
>conventional claim that "behavior is controlled by the environment"
>should have been "behavior: the control _of_ the enviroment".

David Wolsk (22:55 PST 17/12/96) --

>In this always recurring type of discussion I've always been left with a
>sense of unease.

Since both you _and_ Bill Powers have now been made uneasy by my suggestion, I wish I had never made it -- facetious or not;-)

>The terminology for PCT which says it all for me is as follows:

>The task of the organism is coordinating neural and muscular input and
>output relationships utilizing negative feeback and CNS hierarchies for
>maximizing predictability and control.

I don't see what is particularly "PCTish" about this statement. It doesn't seem to differ in any significant way from what a non- PCT oriented behavioral scientist might say about behavior. For example, I think you could find statements like this in the "motor control" literature made by people like Kugler, Turvey, Kelso, Bizzi etc -- people who have no time at all for

PCT.

I think what is missing from this terminology is a clear statement of just _what is controlled_ when the organism is done "...coordinating neural and muscular input and output relationships utilizing negative feeback and CNS hierarchies..."

What says it all for me is:

Behavior: The control of perception.

Best

Rick

Date: Wed, 18 Dec 1996 10:16:50 -0700
From: "T. B. Harms" <tbh@TESSER.COM>
Subject: false knowledge

*From Tracy Harms 1996;12,18.10:00

Bowing to the noisy insistence of the eager mob, I here will post a bit more on that perverse creature, false knowledge. For the moment I do so by transcribing part of a paragraph written by Peter Munz in _Our Knowledge of the Growth of Knowledge_ (Routledge, 1985) p282:

> With the emergence of consciousness, we get a further change in the nature of change. Conscious organisms can create falsehoods: they can lie and delude and deceive both themselves and others. Using deceits and falsehoods, they can construct artificial species which we call human societies and thus surround themselves with a wall of protection against the ravages of the environment. Hence, natural selection can be made to cease. In this way, cultures are created. The most elementary strategy used in the development of cultures is the artificial protection of knowledge from criticism. Certain pieces of knowledge, though obviously not all knowledge, are set aside and protected from critical appraisal. The thunder is identified with a god, the shadow of a man with his soul, and twins with cucumbers. Rational doubts are nipped in the bud by the mere absence of competing alternative proposals. Such protected knowledge can be used as a social bond. People who subscribe to it are members of a society; people who don't are outside that society. In this way, a lot of knowledge is syphoned off and used for non-cognitive purposes--that is, as a catechism. But such syphoning-off, though initially obviously counter-adaptive, is an oblique advantage. A society so constituted is larger than a group of people bonded by nothing but the web of kinship and is therefore capable of effective division of labour and co-operation. Thus, we get the astonishing spectacle of societies which cherish a mountain of false knowledge and which thrive for a long time not in spite of that false knowledge, but because of it.

Date: Wed, 18 Dec 1996 12:50:15 EST
From: Bruce Gregory <gregory@CF4.HARVARD.EDU>
Subject: Re: false knowledge

[From Bruce Gregory (961218.1250 EST)]

Tracy Harms 1996;12,18.10:00

> Bowing to the noisy insistence of the eager mob, I here will post a bit
> more on that perverse creature, false knowledge. For the moment I do so by
> transcribing part of a paragraph written by Peter Munz in _Our Knowledge of
> the Growth of Knowledge_ (Routledge, 1985) p282:

The most perverse aspect of Peter's writing would appear to be
is his use of the word "knowledge" where all right thinking
people would say "belief".

Bruce Gregory

Date: Wed, 18 Dec 1996 11:01:21 -0700
From: "T. B. Harms" <tbh@TESSER.COM>
Subject: Re: words,words,words

*From Tracy Harms 1996;12,18.11:00

Rick Marken (961218.0915)

>Since both you [David Wolsk] _and_ Bill Powers have now been made uneasy
>by my suggestion, I wish I had never made it -- facetious or not;-)

Rick, Rick, Rick. You must overcome these urges to control for other
people's ease of mind, and drive without remorse toward optimizing
understanding. A suggestion made which falls before the pressures of
critical examination is far preferable to silence. Nothing ventured,
nothing gained.

:-)

Tracy Bruce Harms tbh@tesser.com
Boulder, Colorado

Date: Wed, 18 Dec 1996 10:05:32 -0800
From: David Wolsk <dow@PINC.COM>
Subject: auditory processing

18/12/96 09:25
Marftin Taylor Mon, 16 Dec 1996 11:09:14 -0500
[Martin Taylor 961216 10:40]
>Bill Powers (961214.0600 MST)
> Peter Cariani ((961213.1700 EST)--
>
> >When we hear running speech
> >there are time patterns corresponding to the acoustic transients
> >in the speech waveform and their neural correlates can be found

> >at all levels of the auditory system (i.e. large numbers of neurons
> >are following the slower modulations in the waveform).
snip

> This is an interesting post, in that it reminds me strongly of the days when
> I first became aware that my own perceptions were involved in everything I
> experienced (instead of just seeing the world as given as I always did
> before then). When you hear speech, you're obviously noticing
> "characteristic temporal structures," but it's not so obvious that when you
> look at the neural correlates of speech, you're ALSO noticing
> "characteristic temporal structures." It's a particular way of perceiving
> that is involved in BOTH cases.
> ...
> If you listen to speech with a trained ear, so you can hear all the rising
> and falling pitches, the phonemes, the transitions between phonemes, and so
> on, you're clearly not listening to "temporal structures;" you're listening
> at one or more lower levels where you can discern the _elements that are
> structured_.

I'd say "higher levels", not "lower levels." Once you get to pitch, and
even more so when you get to phonemes and phoneme transitions, you are
well beyond the level about which Peter is talking. He's talking about
a phenomenon that has been extensively studied by Roy Patterson and
his colleagues at the MRC Applied Psychology Unit in Cambridge.

I've listened to Roy's acoustic material a little. The waveforms are made
as white noise (Gaussian independent samples), repeating at a period T.
If T is very long, you just hear white noise. If it's shorter than some
threshold value, you hear patterns, such as "bump squeak whine bump squeak
whine bump squeak whine...". snip ...
> When you look at neural correlates of acoustic patterns, if you can trace
> the correlations between the sounds and the neural signals you aren't
> looking at the outputs of the pattern recognizers, but only at the inputs
> which have not yet reached these recognizers. You won't have found the
> temporal pattern recognizer until you find signals that are present when the
> pattern is present, absent when it is absent, and remain _constant_ as long
> as the same temporal pattern is going on.

These are the characteristics of Patterson's "Auditory Stabilized Image",
snip. He claims that all variations that a human can detect in an
auditory pattern can be seen as variations in the screen display, and
all variations that can be seen in the screen display can be heard as
variations in the auditory pattern.

It's been many many years since I called the Kresge Hearing Research
Institute at the Univ Mich home. But I have the feeling that many of the
same differences in approaching sensory/perceptual systems are still
present. To overgeneralise, the perception types get inpressed with the
phenomena which indicate an extremely complex early learning process while
the mathematical modellers get more and more sophisticated in analyzing
input patterns.

There is the"cocktail party" phenomena: at a noisy cocktail party, lots of
people talking at once, if the sexy women you are interested in gives her
telephone number to her girlfriend while standing 12 feet from you, you will
be able to process it.

There is Toscanni whose musical processing competencies scared all orchestra
players since they knew that the slightest error, in the midst of 85 other

sound sources, would be detected.

There is the speech "shadowing" phenomena. With very little training/practice, you can follow along with someone reading text unknown to you. For the audience, it is almost impossible to detect any delay between the two voices.

Perhaps there is a clue to the extent and duration of the learning process in the phenomena of learning to speak a new language. If it happens before one is about twelve, the outcome will be without a "foreign accent".

There is just a hell of a lot of neural tissue in the auditory cortex. My own prediction, that the new implants that stimulate the auditory nerve directly as a "cure" for deafness would fail, seems to be proving false.

All this is not to say that detailed analytical approaches to the first stage acoustic input is useless, just that the researchers may be overextending the application of their patterns when it gets used as data for speech perception.

David Wolsk
Victoria, BC Canada (where the silence of the night is awesome)
P.S. Is the greatly increased volume of our pre-Xmas contributions some sign that we are feeling out of holiday control?

Date: Wed, 18 Dec 1996 13:24:45 -0500
From: Martin Taylor <mmt@HERMES.DCIEM.DND.CA>
Subject: Re: reply to Martin: combining goals

[Martin Taylor 961218 13:15]
> Hans Blom, 961218]

Hans, either you don't understand me or I don't understand you, or both. I'll grant that the "supergoal" you assert is not the one I thought you were asserting, so the degree of misunderstanding may be reducing. But your new formulation could be substituted for mine without in any way altering my point about the "supergoal" being a property of the analyst and not of the organism.
>
> (Martin Taylor 961217 10:30)
>
> >Suppose I am controlling for x=3, y=2, and z=1. You can
> >mathematically say that I am also controlling for the top-level goal
> >q (==x+y+z) = 6. [I use == to mean "is defined as"]. You can treat
> >q=6 as a supergoal.
>
> No, you cannot. There is no mathematical equivalence. For one thing,
> the dimension isn't right. Controlling for x=3, y=2, and z=1
> simultaneously (!) sets as the goal the point (x,y,z) = (3,2,1) in
> three-dimensional space. Controlling for x+y+z=6 (in that same
> three-dimensional space) is something very different; the goal is now
> a large subspace rather than a single point. You can _not_ simply
> replace "control for x=3 and for y=2 and for z=1" by "control for
> x+y+z=3+2+1".

I agree.
>

> You can now replace "control for x=3 and for y=2 and for z=1" by
> "minimize (x-3)^2 + (y-2)^2 + (z-1)^2". Which you can translate back
> into "control for the minimum of (x-3)^2 + (y-2)^2 + (z-1)^2", if you
> want. This is how you find the "top" goal.

So the analyst says. But the organism doesn't (by construction; the simulated organism has been _defined_to have no such "top" goal). The _analyst_ sees that the organism seems to behave _as if_ it did have such a top goal. But in fact (as we know, having constructed the organism to be so) the organism has three independent "top" goals, and the analyst has engaged in self-deception--or at least is wrong--in saying that it has one "top" goal.

There's a separate issue, too. You add the possibility that:

>Actually, different weights can be attached to the individual
>terms, as in "minimize (x-3)^2 + 2 * (y-2)^2 + 5 * (z-1)^2". Doing so
>does not change the solution, at least not if there are no
>constraints in the form of forbidden areas in xyz-space, but it does
>change the path toward the solution if the solution is to be found by
>an iterative procedure (such as a control system).]

Now your "top" goal has three extra degrees of freedom that the analyst can use to get around the problem that the gains on the x, y, and z "real" perceptual control systems may be different, leading to ellipsoidal rather than spherical iso-deviation contours. The analyst can still delude himself into seeing a non-existent top goal by using these extra degrees of freedom provided by the weights.

> Now that this confusion is cleared, reread your post and check
> whether my post answered what your question ought to have been ;-).
>
I pass the above back to you.

And to answer your question, no it didn't.

Martin

Date: Wed, 18 Dec 1996 11:43:12 -0700
From: Bill Powers <powers_w@FRONTIER.NET>
Subject: Re: Lights, buttons, Christmas decorations

[From Bill Powers (961218.1030 MST)]

Peter Cariani (961218.1100 EST)]

>> If that's what you find, I would seriously suggest that you're looking at
>> the wrong correlates of the signals, or looking at signals at too low a
>> level in the hierarchy to go with the correlates you're interested in. It
>> isn't that the information is "mixed together." It's still all spread out
>> over different channels, and won't make sense until some input function
>> receives those scattered signals and converts them into a higher-level
signal.
>
>Very, very few examples of what might be clean higher-level signals have
>ever been found (although this is what every cortical physiologist and his
>brother is looking for).

But what are they looking for? Isaac Kurtzer has been trying some PCT-style experiments with what people perceive as a phoneme, looking at many current theories about how the formants F1 and F2 are related. This is working strictly between the physical sound-wave and the subjective identification of an isolated phoneme, so the model of neural conduction is irrelevant. What he did was randomly vary F1 and have the subject vary F2 with a mouse (for each F1), looking for the boundaries between perception of the phoneme and failure to perceive it. He found very broad regions of both formants, with no indication that the ratio was being preserved. Of course this was just a preliminary study and everything is subject to revision and rethinking. But he found some odd things, for example that there are holes in the region where the phoneme is NOT heard.

In most neurological studies I have seen the neurology is pretty sophisticated but the selection of "correlates" to be perceived is essentially just a simple commonsense selection from ordinary experience -- the _experimenter's_ experience or however much of it he has paid any critical attention to. There might be a lot more success in finding "clean high-level signals" if one first did the test for the controlled variable to identify exactly what stimulus condition the subject was perceiving and controlling. If a subject, given the means, can't control what the experimenter decides is the interesting physical aspect of the stimulus, then the subject probably can't perceive it, either, no matter how obvious it is to the experimenter. In Demo1, I have been astonished at how many people are unable to control the pitch of a sound -- I haven't kept track but it seems to be 30 or 40 percent of the people who try it. Many of them comment afterward that they're "tone-deaf". I'm sure that most of them could learn to do it, since most of them improve their control with practice, but the point is that these are adults and for whatever reason they have never learned to perceive sound in the dimension of pitch. It can't be an output problem because they do fine in controlling other perceptions using the very same means.

Just imagine how much luck you'd have in finding neural correlates of pitch in one of these people's brains. You might find signals in the auditory pathway that correlate with pitch, but in that person there is no input function that discriminates the pitch information in these signals. It might as well not be there as far as higher-level perceptions are concerned.

>At some point, after looking and looking and looking and
>trying to force-fit what you see into what you expect you should see, one
>has to sit in a quiet dark room for a while and re-examine the basic
>assumptions.

I'd be happy to do so if I thought that what I expect has actually been tested and found not to be there. The way the brain's perceptual functions work is not critical to either PCT or HPCT. But I'm far from convinced of that. Do you know of any studies that have looked systematically for neural correlates of perceptions of intensities, sensations, configurations, transitions, events, relationships, categories, programs, principles, and system concepts? And that have verified, through use of the Test, that whatever is assumed to be perceived is actually being perceived?

>> I really differ with you over the use of this image of a "multiplexer".

>The multiplexing you are thinking about is time-division multiplexing of
>the kind used in telephony.

>There is also frequency-division multiplexing where
>signals are carried in different frequency bands (like radio), and this is
>the general kind of multiplexing that I am making reference to.

I mentioned that one, too.

>I suppose
>we could call it "temporal pattern-division multiplexing", but maybe it's
>just easier not to use the M-word here.

You can make up a term like that, but you still haven't shown that such a thing could be done without the usual requirement that the multiplexer and the demultiplexer must be accurately coordinated -- somehow. That's tough to do, especially in your case where there is no specific multiplexer. When you get to the point of positing a mechanism that you can't even come close to describing, except that it does what you want it to do, you've crossed the boundary into "dormitive principles" (a pill makes you sleepy because it contains a Dormitive Principle).

Enough. I'm still feeling bad about our old cat, who was assisted out of life yesterday. And anyway this discussion is beginning to get repetitive. What do you think about my proposal for verifying what people are perceiving by using the Test?

Best,

Bill P.

Date: Wed, 18 Dec 1996 14:04:39 -0500
From: Scott Stirling <scstirli@ANSELM.EDU>
Subject: Re: In Defense of Popper & Harms

[From Bruce Gregory (961218.0650 EST)]

>The key point is that I am not talking
>about _knowing_ a reality beyond perception. I am taking about having
>no doubt that there _is_ a real world.

Then I don't know what we've been arguing about. Having no doubt and knowing are different things, as I said about ten posts back when I said I did not intend to argue over matters of faith.

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Date: Wed, 18 Dec 1996 14:44:04 -0500
From: Martin Taylor <mmt@HERMES.DCIEM.DND.CA>
Subject: Re: Tone deaf?

[Martin Taylor 961218 14:30]
>Bill Powers (961218.1030 MST)

>In Demo1, I have been astonished at how many
>people are unable to control the pitch of a sound -- I haven't kept track
>but it seems to be 30 or 40 percent of the people who try it. Many of them
>comment afterward that they're "tone-deaf". I'm sure that most of them could
>learn to do it, since most of them improve their control with practice, but
>the point is that these are adults and for whatever reason they have never
>learned to perceive sound in the dimension of pitch. It can't be an output
>problem because they do fine in controlling other perceptions using the very
>same means.

I suspect this is an example of the problem that conscious perception is ordinarily only at one level. We do not consciously perceive the angle of the steering wheel when we are conscious of keeping the car in the middle of its lane, or of the position of the car in its lane when we are consciously following an intricate route.

Too many years ago, one of the early signal-detection researchers (W.P.Tanner) told me of an experiment he had done with pitch. I don't remember the details, but Tanner was convinced that people could learn to discriminate just about anything that was distinct in acoustic patterns, whether or not the sounds ever occurred in nature, provided that the subject's actions resulted in feedback that we might now say affected their control of their perception of being usually correct. (I think that he might have become a PCT-er, had he lived).

Anyway, he did this experiment with pitch, in which almost all subjects performed very well, with two exceptions. Those two exception were native speakers of a language in which fine pitch-transition discriminations are linguistically important (Thai?). They could not make sense of their language without being able to discriminate pitch pretty well. But when "pitch" was, so as to speak, abstracted and made an attribute of a non-linguistic sound, they performed as if they could not distinguish pitches at all.

I wish I could remember more, but this was almost 40 years ago, and what I've written is almost all I can remember. But it sounds like your experience. People may be unable to hear the pitch differences because they are accustomed to hearing them at a different perceptual control level than under the conditions of the experiment. And in the experiment, the differences they are used to hearing don't exist, which makes them "tone deaf" despite making heavy use of pitch in everyday life.

I wonder if these people could control the pitch if you did a couple of trivially simple manipulations: (1) frequency modulate the pitch with a random modulator of bandwidth perhaps 10 or 15 Hz and a maximum excursion of about 2% (These are top-of-the-head numbers probably of the right order of magnitude), and (2) add another tone that doesn't change pitch, so that one of the two tones is in F1 territory and the other is in F2 territory (I think this second manipulation is less important than the first).

The idea of these manipulations comes from a little demo I heard at a long-ago meeting of the Acoustical Society of America, in which a warbled whistle suddenly was heard as "whistled speech" when the relatively slow warble was frequency modulated by this fast random modulator. Something about the rapid random variation assists us to hear sound "as speech" whether it is or not. Perhaps your "tone deaf" subjects are hearing pitch as a meaningful

element of speech, and as such, could control it. (The second manipulation is also for the same reason, but has the problem that if you keep the second pitch steady what the subjects might be controlling is chord quality rather than pitch, and if you don't, they might have difficulty separating out the tone to be controlled from the tome to be ignored).

Come to think of it, I'm not sure whether the modulation of manipulation (1) should be FM or AM, or if it matters at low modulation levels. Again, it was a long time ago. Sorry to be vague.

A speculation.

Martin

Date: Wed, 18 Dec 1996 17:06:59 EST
From: Bruce Gregory <gregory@CF4.HARVARD.EDU>
Subject: Re: In Defense of Popper & Harms

[From Bruce Gregory (961218.1710 EST)]

18 Dec 1996 14:04 Scott Stirling

> Then I don't know what we've been arguing about. Having no doubt
> and knowing are different things, as I said about ten posts back when I
> said I did not intend to argue over matters of faith.

I'll try to follow your example -- especially when dealing
with people who question whether the world is real.

Bruce Gregory

Date: Wed, 18 Dec 1996 17:07:53 -0600
From: Bruce Abbott <abbott@CVAX.IPFW.INDIANA.EDU>
Subject: Solving the "loss of input" problem: RunBlind.pas

[From Bruce Abbott (961218.1810 EST)]

Enough of these verbal arguments!

Appended below is a Pascal program, RunBlind.pas, which you can compile and run if you have access to either Turbo Pascal 7.0 or Borland Pascal 7.0 or later. You will also need to compile the GRutils.pas unit, which you will find appended beneath the RunBlind code.

If you do not have these Pascal compilers but do have access to a PC, I can send the executable code via e-mail on request. (It will come MIME encoded; e-mail programs such as Eudora will automatically decode it. This is a DOS program, so if you are running Windows, you will have to run it within a DOS box.)

The program begins in "sighted" mode, with the controller keeping its controlled perceptual variable nicely under control against a variable disturbance waveform. Pressing the space bar switches the controller to "blind" mode, at which time it uses its internal model of the "modeled"

disturbance to determine its control actions. The actual disturbance acting on the CV is an additive combination of modeled and unmodeled disturbances. You will note that control over the actual CV deteriorates when the controller is switched to blind mode, but that the CV still varies less than it would if all control were lost.

Rick Marken, please note that when switched to the blind mode, the controller does not go open loop, but continues to control its perceptual variable (top line in display). Because the modeled portion of the disturbance is modeled accurately, some degree of control over the EV is retained as well.

Regards,

Bruce

RunBlind.Pas follows immediately.

```
-------------------------------------------------------------------------
program RunBlind;
{ Simulation to demonstrate a controller capable of continuing to control
  (to some degree) after its input has been lost.

  Compile in Turbo Pascal 7.0 or Borland Pascal 7.0 or above

  For simplicity, I have given the controller a perfect model of one of
  the disturbance waveforms, which it uses as a substitute for the real
  disturbance when running "blind." There is also an unmodeled disturbance
  waveform present, and the actual disturbance waveform is the sum of the
  modeled and unmodeled disturbance waveforms.

  The program begins with the controller running in "sighted" mode,
  controlling a perceptual variable that is identical to the environmental
  variable of which the perceptual variable is a function. By pressing
  the space bar, you can force the controller to run "blind," relying on
  its model of the disturbance to determine the state of the controlled
  perceptual variable. You will note that when the controller runs in the
  "blind" mode, it is no longer able to counter the unmodeled disturbance,
  but still does counter the modeled disturbance. Consequently, the
  environmental variable does not vary as much as it would in the absence
  of all controll.

  Written by Bruce Abbott
  Indiana - Purdue Fort Wayne
  (961218)
}

uses dos, crt, graph, grutils;

var
  ch: char;
  Stop, CanSee: boolean;
  p, r, e, g, o, s, CV, CVm, d, dMod, dUnmod, dt, t: real;
  MaxX, MaxY, MaxColor: integer;
  X, Y1, Y2, Y3, Y4, Y5: integer;

procedure InitScreen;
begin
  ClrScr;
  InitGraphics;
```

```
  MaxX := GetMaxX; MaxY := GetMaxY;
  MaxColor := GetMaxColor;
  OutTextXY(200, 10, 'RunBlind Controller Demo');
  SetColor(Yellow);
  OutTextXY(145, 30, 'Press Spacebar to toggle Sighted/Blind');
  SetColor(LightCyan);
  OutTextXY(10, 60, 'Controlled Perceptual Variable');
  SetColor(LightRed);
  OutTextXY(10, 75, 'Environmental Variable');
  SetColor(White);
  OutTextXY(10, MaxY - 90, 'Controller Output');
  SetColor(Yellow);
  OutTextXY(10, MaxY - 75, 'Disturbance to Perceptual Variable');
  SetColor(LightCyan);
  OutTextXY(10, MaxY - 60, 'Disturbance to Environmental Variable');
  SetColor(White);
end;

procedure InitModel;
begin
  CanSee := true;
  t := 0.0;
  dt := 0.1;
  s := 0.1;
  CV := 90.0;
  o := CV;
  r := 100.0;
  g := 100.0;
  dMod := 0.0;
  dUnmod := 0.0;
  d := dMod + dUnmod;
  CanSee := true;
end;

procedure StepModel;
begin
  p := CV;
  e := r - p;
  o := o + (g*e - o)*s*dt;
  dMod := 2.0*sin(t);
  dUnmod := 0.5*sin(3.0*t/2.0) + 0.5*sin(t/3.0) + 0.5*cos(t/5.0);
  if CanSee then d := dMod + dUnmod
    else d := dMod;
  CV := o + d;
  if CanSee then CVm := CV else CVm := o + dMod + dUnmod;
  t := t + dt;
end;

procedure GraphVars;
begin
  X := X + round(dt*10.0);
  if X > 600 then
    begin
      X := 1;
      SetViewPort(1, 100, MaxX, MaxY-100, ClipOff);
      ClearViewPort;
      SetViewPort(1, 1, MaxX, MaxY, ClipOff);
    end;
  Y1 := MaxY - round(o*10.0) + 725;
```

```
  Y2 := MaxY - round(d*10.0) - 200;
  Y3 := MaxY - round(CV*10) + 625;
  Y4 := MaxY - round(CVm*10) + 660;
  Y5 := MaxY - round((dMod+dUnmod)*10) - 150;
  putpixel(X, Y1, white);
  putpixel(X, Y2, yellow);
  putpixel(X, Y3, lightcyan);
  putpixel(X, Y4, lightred);
  putpixel(X, Y5, lightcyan);
end;

procedure ShowStatus;
begin
  SetViewport(250,MaxY-50, 350, MaxY, ClipOff);
  ClearViewPort;
  SetViewport(1, 1, MaxX, MaxY, ClipOff);
  if CanSee then OuttextXY(250, MaxY-25, 'Sighted')
    else OuttextXY(250, MaxY-25, 'Blind');
end;

begin
  TextMode(co80);
  ClrScr;
  InitScreen;
  Stop := false;
  InitModel;
  ShowStatus;
  repeat
    StepModel;
    GraphVars;
    delay(50);
    if keypressed then
      begin
        ch := readkey;
        Case ch of
          #27: Stop := true;
          ' ': If CanSee = true then
            begin
              CanSee := false;
              ShowStatus;
            end
            else
              begin
                CanSee := true;
                ShowStatus;
              end;
          end;
      end;
  until Stop;
end.
-------------------------------------------------------------------------
Separate the code below for GRutils.pas into its own text file and compile
separately.
-------------------------------------------------------------------------
unit GrUtils;
{ Graphics Utilities Unit }

interface
```

```
uses
  Graph, bgidriv;

var
  GraphDriver, GraphMode, Error: integer;

procedure InitGraphics;
procedure Retrace;

implementation

procedure retrace;
begin
 case Graphdriver of
  Ega,Vga,Ega64,EgaMono: begin
           while (port[$3da] and 8) = 8 do ;
           while (port[$3da] and 8) = 0 do ;
          end;
  HercMono: begin
           while (port[$3ba] and $80) = 0 do ;
           while (port[$3ba] and $80) = $80 do ;
           end;
  ELSE begin
          while (port[$3da] and 8) = 0 do ;
          while (port[$3da] and 8) = 8 do ;
       end;
 end;
end;

procedure Abort(Msg : string);
begin
  Writeln(Msg, ': ', GraphErrorMsg(GraphResult));
  Halt(1);
end;

procedure InitGraphics; {ADAPTS TO HARDWARE}
begin
  { Register all the drivers }
  if RegisterBGIdriver(@CGADriverProc) < 0 then
    Abort('CGA');
  if RegisterBGIdriver(@EGAVGADriverProc) < 0 then
    Abort('EGA/VGA');

  GraphDriver := Detect;                  { autodetect the hardware }
  InitGraph(GraphDriver, GraphMode, '');  { activate graphics }
  if GraphResult <> grOk then             { any errors? }
  begin
    Writeln('Graphics init error: ', GraphErrorMsg(GraphDriver));
    Halt(1);
  end;
end;
(*
procedure InitGraphics; {ADAPTS TO HARDWARE}
begin
 graphdriver := 0; graphmode := 0;
 detectgraph(graphdriver,graphmode);
 initgraph(graphdriver,graphmode, BGIDIR);
 graphmode := getmaxmode;
 setgraphmode(graphmode);
```

```
end;
*)

begin
end.
```

Date: Wed, 18 Dec 1996 18:09:51 -0500
From: Scott Stirling <scstirli@ANSELM.EDU>
Subject: Re: In Defense of Popper & Harms

On Wed, 18 Dec 1996, Bruce Gregory wrote:
> [From Bruce Gregory (961218.1710 EST)]
>
> 18 Dec 1996 14:04 Scott Stirling
>
> > Then I don't know what we've been arguing about. Having no doubt
> > and knowing are different things, as I said about ten posts back when I
> > said I did not intend to argue over matters of faith.
>
> I'll try to follow your example -- especially when dealing
> with people who question whether the world is real.
>
>
> Bruce Gregory
>

Do you think I should make it clear that I do not think it's possible to disprove the existence of an external reality? I mean it works both ways. I guess I've just been focusing on the assertion that it is possible to know that perception correlates to a "real world." If someone were arguing as strongly for the opposite, that it is possible to know that there is no "real world" to which our perceptions correspond, I would focus on that assertion, all the while trying to bring the realm of knowledge or "having no doubt" back to more middle territory.

Scott

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Date: Wed, 18 Dec 1996 17:19:53 -0700
From: Bill Powers <powers_w@FRONTIER.NET>
Subject: Re: Solving the "loss of input" problem: RunBlind.pas

[From Bill Powers (961218.1715 MST)]

Bruce Abbott (961218.1810 EST) --
>
>Enough of these verbal arguments!

>
>Appended below is a Pascal program, RunBlind.pas, which you can compile and
>run if you have access to either Turbo Pascal 7.0 or Borland Pascal 7.0 or
>later. You will also need to compile the GRutils.pas unit, which you will
>find appended beneath the RunBlind code.

Better look over this program again, Bruce. The input is not lost when the system is in the "blind" mode. The perceptual signal is still proportional to the CV. Loss of input means that the CV is no longer perceived by the system. I suggest working out a block diagram to see what your program does.

Best,

Bill P.

Date: Wed, 18 Dec 1996 16:22:28 -0800
From: Richard Marken <marken@AEROSPACE.AERO.ORG>
Subject: Lies, Damn Lies and Programs

[From Rick Marken (961218.1630)]

Bruce Abbott (961218.1810 EST) --

>Enough of these verbal arguments!

Oh, goody. Now you're going to "pull a Hans" and use a program to justify your cause-effect perspective on behavior. What a gas.

>Rick Marken, please note that when switched to the blind mode, the
>controller does not go open loop, but continues to control its perceptual
>variable (top line in display).

That's because you wrote the code that way (at least with respect to CV; see below). Your model controls a perception of the CV whether it is in "see" mode or "blind" mode. All that happens when the model goes into "blind" mode is that the disturbance changes, from dMod + dUnmod to dMod. Since p = CV in both "see" and "blind" modes, then CV (which I presume to be the actual environmental variable) will be kept under control just fine in both modes because the model can "see" the CV in both modes.

Perhaps you are thinking of CVm as the real environmental variable that is under control? If so, then you've still got problems. Look at the code:

```
procedure StepModel;
begin
  p := CV;
  e := r - p;
  o := o + (g*e - o)*s*dt;
  dMod := 2.0*sin(t);
  dUnmod := 0.5*sin(3.0*t/2.0) + 0.5*sin(t/3.0) + 0.5*cos(t/5.0);
  if CanSee then d := dMod + dUnmod
    else d := dMod;
  CV := o + d;
  if CanSee then CVm := CV else CVm := o + dMod + dUnmod;
  t := t + dt;
end;
```

When the model is in "see" mode, CVm = CV so control of both CV and CVm looks equally good. When the model is in "blind" mode, CV = o + dMod but CVm = o + dMod + dUnmod. It will look like control of CVm is worse than control of CV by the size of dUnmod. In fact, if CVm is the "real" CV then dMod should have no effect on it. The statement:

if CanSee then CVm := CV else CVm := o + dMod + dUnmod;

should be changed to

if CanSee then CVm := CV else CVm := o + dUnmod;

Now CVm is a measure of what the "real" CV is doing when the control system is "blind". The amount by which variations in CV (the variable controlled in imagination) differ from CVm (the real controlled variable) depends on the difference between dMod and dUnmod. If dMod = dUnmod then it will look like your controller can control in the "blind". To the extent that dMod <> dUnmod, then there will be little apparent control of CVm. In either case, however, your model is not really in control of CVm; it only controls CV. You model controller _is_, indeed, blind to variations in CVm and is thus _open loop_ with respect to that variable.

So, all in all, I'd say that your program is just a jot worse than your verbal arguments. Let me know what you learn when you fix it up;-)

Best

Rick

Date: Wed, 18 Dec 1996 21:57:44 -0800
From: David Wolsk <dow@PINC.COM>
Subject: categorization

Hans Blom 18/12/96
2. If we believe we can detect "consistency over time", it seems to me that we have some inner mechanism that allows this detection. Since this detection is primarily some type of comparison between what occurs now and what occurred previously, it appears that we have some type of inner "store" where previous occurrences are somehow saved, possibly encoded and/or compacted. It also appears that, due to brain size limitations, it is impossible to store the raw data of _all_ previous occurrences in their entirety. So what is stored? Only occurrences that are recognized as "lawful"? But initially there are none. Are occurrences that are similar to a previous one somehow discarded? But how to establish what is similar right from the start? We seem to have a bootstrap problem, which indicates the need for an

---

Date: Wed, 18 Dec 1996 23:39:32 +0000
From: Richard marken <marken@AERO.ORG>
Subject: Re: Solving the "loss of input" problem: RunBlind.pas

[From Rick Marken (961218.2230)]

Here's the corrected code for your model, Bruce:

```
procedure StepModel;
 begin
 p := CV;
 e := r - p;
 o := o + (g*e - o)*s*dt;
 dMod := 2.0*sin(t);
 dUnmod := 0.5*sin(3.0*t/2.0) + 0.5*sin(t/3.0) + 0.5*cos(t/5.0);
 if CanSee then d := dUnmod
   else d := dMod;
 CV := o + d;
 CVm := o + dUnmod;
 t := t + dt;
 end;
```

Note that CVm is the "real" environmental variable that is to be controlled; it is always equal to o+dUnmod, where Unmod is the real disturbance. When the model is "blind", CV is the imagined input to the perceptual function; so it is o+dMod, where dMod is the imagined disturbance.

This is a very cool program Bruce; the graphs are beautiful and it shows rather clearly just what happens when a control system goes "blind" and cannot imagine (model) _exactly_ what is happening to the environmental variable that had been under control before the loss of input.

Thanks for sharing;-)

Best

Rick

Date: Thu, 19 Dec 1996 12:33:40 +0100
From: "J.A. Blom" <J.A.Blom@ELE.TUE.NL>
Subject: Re: reply to Martin: combining goals

[Hans Blom, 961219]

(Martin Taylor 961218 13:15)

>Hans, either you don't understand me or I don't understand you, or
>both.

Both, I think. And, going up a level, you probably wonder how I can be so dumb as to not understand you -- and me you... Things like that happen all the time. That's why we humans talk so much :-).

>I'll grant that the "supergoal" you assert is not the one I thought
>you were asserting, so the degree of misunderstanding may be
>reducing.

Good.

>But your new formulation could be substituted for mine without in
>any way altering my point about the "supergoal" being a property of
>the analyst and not of the organism.

All right, the problem is about epistomology after all, not about mathematics.

>You can now replace "control for x=3 and for y=2 and for z=1" by
>"minimize (x-3)^2 + (y-2)^2 + (z-1)^2". Which you can translate back
>into "control for the minimum of (x-3)^2 + (y-2)^2 + (z-1)^2", if
>you want. This is how you find the "top" goal.

>So the analyst says. But the organism doesn't (by construction; the
>simulated organism has been _defined_to have no such "top" goal).

This is the core of the problem, I think:

1. You construct a control system with 3 independent, parallel goals.

2. I demonstrate to you that there is 1 mathematically equivalent "top-level" goal that subsumes your three goals.

3. You say "No, I constructed 3 goals". I say "Sure, but those 3 can be compacted into 1".

That is, your construction seems to have an implication ("unintended side effect"?) that you neither intended nor now wish to intend. And therefore you cannot -- and do not want to -- see that super-goal as something that _causes_ the behavior. And it doesn't, of course; the 3 goals that you specified for and designed into the controller cause its behavior. In that sense, the super-goal is not causal; it is only mathematically equivalent to your 3 causal parallel goals combined. Or does that make the super-goal causal by implication?

>The _analyst_ sees that the organism seems to behave _as if_ it did
>have such a top goal. But in fact (as we know, having constructed
>the organism to be so) the organism has three independent "top"
>goals, and the analyst has engaged in self-deception--or at least is
>wrong--in saying that it has one "top" goal.

Beautiful! Here we have the raw contrast between "as if" and "in fact" ;-). We see one and the same thing, the 3-goal controller that you designed. You see that it has properties P1, P2, ..., PN. I see an additional property PN+1. Is that property "in fact" there, or is it only "as if" it is there, but not really? What kind of test would you propose to make this question decidable?

AI has the notions of explicit and implicit knowledge. If an expert system's database contains the facts "A is true" and "B is true" and its rulebase contains the (always true) rule "A and B", does the expert system now "know" that "A and B is true"? The answer is: an expert system has an inferencing mechanism. Before inferencing, the truth value of "A and B" is not yet known; after inferencing it is. Thus, implicit knowledge becomes explicit. If you don't infer, you don't know what you know. Thus, inferring is highly recommended ;-).

I know a different test. We must both design a control system whose 3 goals (x=3, etc.) are provided to us. You build a controller which implements those 3 goals directly. I first combine the three goals into one super-goal, after which I design my controller to control for that super-goal. When testing the behaviors of both controllers, they turn out to be exactly identical. Can we now answer the following questions:

- do both controllers have one super-goal?
- do both controllers have 3 independent, parallel goals?
- does your controller have 3 goals whereas mine has one?
- all of the above.
- neither of the above.

What would you say?

>There's a separate issue, too. You add the possibility that:

>> ... different weights can be attached to the individual terms

>Now your "top" goal has three extra degrees of freedom that the
>analyst can use to get around the problem that the gains on the x,
>y, and z "real" perceptual control systems may be different, leading
>to ellipsoidal rather than spherical iso-deviation contours.

You're right about the contours, whose slopes determine along which path a 'steepest ascent hill-climber' will approach the top. It's not correct to say that the _top goal_ has extra degrees of freedom; in fact, its position does not change whatever your choice of the weights. A lot of different roads lead to Rome, as they say here. That doesn't mean that Rome's location has "degrees of freedom".

>The analyst can still delude himself into seeing a non-existent top
>goal by using these extra degrees of freedom provided by the
>weights.

This I do not understand. Can you explain?

I think we're getting at some very interesting issues!

Greetings,

Hans

Date: Thu, 19 Dec 1996 13:26:37 +0100
From: "J.A. Blom" <J.A.Blom@ELE.TUE.NL>
Subject: Re: resend: single top-level goal?

[Hans Blom, 961219]

(Bill Powers (961217.0945 MST))

>Suppose you have two control systems at the same level, coupled
>together at a lower level so that both cannot simultaneously achieve
>zero error.

I tried to discover a physical realization (block diagram, if you want) of your formulas. I can't. Although you use symbols, I cannot discover a meaning. It's a situation resembling statements like "it rains but it does not rain" or "x=3 and x=4". Logical contradictions, in other words. The problems are here:

>Let e be the error. When system 1 alone is acting, the value of x,
>the common lower-level variable, will be G1*e1. When system 2 alone

>is acting, x will be G2* e2. When both systems are acting

How are the outputs of both systems (physically) combined? You do not specify that. As an addition? No. As an average? No. You set them to be equal:

>, we introduce the constraint G1*e1 = G2*e2 = x

This is a situation that most resembles short-circuiting one output with another system's output: x = output1 = output2. If so, the model isn't as accurate/complete as it need be to describe this situation, because it won't now be true that output1 = G1*e1 _and_ output2 = G2*e2 if G1 and G2 are to have their normal (non-short- circuit) meanings.

Note also that e1 and e2 require a meaning in the combined system. They were defined for other conditions.

>Let e = r - x, so we have G1*(r1 - x) = G2*(r2 - x).

In a short-circuit situation, this won't generally be true if G1 and G2 are to have their normal (non-short- circuit) meanings.

>Solving for x, we obtain x = (G2*r2 - G1*r1)/(G1 - G2)
>Note that as the gains approach equality, x increases without limit
>when r1 is not equal to r2.

Infinities ought to warn you that your model cannot be a correct representation of some physical reality.

>This may alter your analysis of error minimization.

Not yet. Although reality (I will continue to use that word) can be modelled or expressed as words or formulas, not every model or set of words or formulas can be given a physical meaning. It seems to me that you play with words (formulas).

Greetings,

Hans

Date: Thu, 19 Dec 1996 13:44:08 +0100
From: "J.A. Blom" <J.A.Blom@ELE.TUE.NL>
Subject: Re: In Defense of Popper & Harms

[Hans Blom, 961219b]

(Bill Powers (961217.1100 MST))

>>2. If we believe we can detect "consistency over time", it seems to
>>me that we have some inner mechanism that allows this detection.

>Yes, it's called a perceptual input function.

You misunderstand me. With "consistency over time" I mean, for example, that "the sun rises again and again". A perceptual input function may tell me that "the sun rises", but not -- by itself --

that this occurrence is similar to other sunrises that I have observed in the past. To establish the similarity, it appears to me, there must be _two_ things to be compared. What I assume is that one is in actual perception (the sunrise that I observe now), and one is in memory (about sunrises that I've observed in days, months and years past). That seems pretty inescapable, even tautological...

If you believe that such "consistency over time" is established by a perceptual input function, it is one that encodes sequences, I guess. But a "sequence" still links two occurrences, at least one of them from the past, and needs a similar mechanism.

Greetings,

Hans

Date: Thu, 19 Dec 1996 13:49:17 +0100
From: "J.A. Blom" <J.A.Blom@ELE.TUE.NL>
Subject: Re: Solving "loss of input" problem

[Hans Blom, 961219c]

(Bill Powers (961217.1200 MST))

>I think you're just out of practice in thinking about closed-loop
>systems. If you understood how ubiquitous disturbances are, you
>would not say "If I do X then I perceive Y." In the real world, if
>you "do X" ten times in a row, you will get nine different values of
>Y.

I did The Test. The last ten times I turned my car's steering wheel to the right, the car was perceived to turn to the right. Not once to the left.

What kind of car do you have? :-).

Greetings,

Hans

End of CSGNET Digest - 18 Dec 1996 to 19 Dec 1996
****

Date: Fri, 20 Dec 1996 08:00:08 -0600
Subject: CSGNET Digest - 19 Dec 1996 to 20 Dec 1996

There are 40 messages totalling 2386 lines in this issue.

Topics of the day:

1. Lies, damned lies, and programs (9)
2. Solving the "loss of input" problem: RunBlind.pas (3)
3. Rick' "corrected" code
4. In Defense of Popper & Harms (7)
5. RunBlind.pas (3)

Date: Thu, 19 Dec 1996 08:10:58 -0600
From: Bruce Abbott <abbott@CVAX.IPFW.INDIANA.EDU>
Subject: Re: Lies, damned lies, and programs

[From Bruce Abbott (961219.0910 EST)]

>Rick Marken (961218.1630) --

>Bruce Abbott (961218.1810 EST)

>>Enough of these verbal arguments!

>Oh, goody. Now you're going to "pull a Hans" and use a program to justify
>your cause-effect perspective on behavior. What a gas.

Thanks for the sarcasm. It's always appropriate in scientific debate, isn't it?

Let's see if I've got this straight, Rick. If we engage in verbal argument then we have trouble because words can mean different things to different people; to settle such issues we need to have an objective method of demonstrating that what we assert will happen actually does happen. The way you and Bill do this is through computer simulation.

On the other hand, if I use computer simulation to demonstrate the validity of my analysis, then I am "pulling a Hans." May I take it that in the future you will stop making the claim that PCT is superior to other approaches because it is supported by working computer simulations? Obviously you don't believe they have any value when used to support analyses with which you do not agree, but the value of this approach cannot depend on who is using it to support which point of view.

>>Rick Marken, please note that when switched to the blind mode, the
>>controller does not go open loop, but continues to control its perceptual
>>variable (top line in display).

>That's because you wrote the code that way (at least with respect to CV; see
>below). Your model controls a perception of the CV whether it is in "see"
>mode or "blind" mode.

Damned right it's because I wrote the code that way. I wrote the code that way so that it would implement the model I described in my previous post. Here is your description of my proposal, which I agreed was an accurate account:

>It seems to me that you are proposing the following: Under normal
>circumstances (input available) a control system controls a time varying
>perceptual variable, p(t), which (assuming a single time varying disturbance
>variable, d(t)) is proportional to:
>
>p (t) = o (t) + d (t)
>
>When the lights go out (no more input) the system begins controlling an
>imagined, time varying perception, p'(t), which is proportional to:
>
>p'(t) = o (t) + d' (t)
>
>where d'(t) is the modelled version of d(t) and o(t) is the real output
>(twhichis now added to both the real disturbance -- to determine the state
>of the now un-sensed controlled environmental variable-- and the imagined
>disturbance).

This is exactly what the program implements.

>All that happens when the model goes into "blind" mode
>is that the disturbance changes, from dMod + dUnmod to dMod. Since p = CV in
>both "see" and "blind" modes, then CV (which I presume to be the actual
>environmental variable) will be kept under control just fine in both modes
>because the model can "see" the CV in both modes.

That's right, Rick. Exactly as I described verbally in my earlier post. No loss of input, no running in open loop mode. Let's take a somewhat different look at the model than the one you have extracted from the code.

The "real" CV:

$$CV = x + d,$$

where x = the effect of the controller's output on the CV and d = the disturbance acting on the CV. The environmental feedback function is assumed to be:

$$x = 1 * o,$$

where o is the controller's output.

The "model" CV:

$$CV = x' + d',$$

where x' is the controller's model of the effect of the controller's output on the CV and d' is the controller's model of the predictable portion of the disturbance waveform. We can consider the actual disturbance as comprising both this component and an unmodeled component, d*:

$$d = d' + d^*$$

I am assuming that the controller's model of the EFF is

$$x' = 1 * o$$

When running is "sighted mode," the controller is "seeing" the CV as:

CV = x + d

When running in the "blind mode," the controller is "seeing" the CV as:

CV = x' + d'

But in the sighted mode,

CV = x + d = (1 * o) + (d' + d*) = o + d' + d*,

and in the blind mode,

CV = x' + d' = (1 * o) + d' = o + d'.

Thus, in my computer model, the only thing I need to do "switch" to blind mode is drop out d*.

In setting up the problem this way, I have implicitly assumed the best of all possible worlds: that the modeled portion of the disturbance waveform is identical to the predictable protion of the disturbance waveform in shape, phase, and amplitude, and that the modeled EFF is identical to the actual EFF. I did this in order to demonstrate the principle involved under the most favorable conditions and to keep the demonstration simple, to foster understanding of its basic principles. A more complex model could derive its modeled EFF and disturbances from experience, as in Hans's Kalman filter system. Here I have just assumed that such a mechanism exists and has done its job.

>So, all in all, I'd say that your program is just a jot worse than your
>verbal arguments. Let me know what you learn when you fix it up;-)

What needs repair is your understanding of the program, not the program, which works exactly as intended, and demonstrates exactly what I said it does.

Regards,

Bruce

Date: Thu, 19 Dec 1996 15:58:28 +0100
From: "J.A. Blom" <J.A.Blom@ELE.TUE.NL>
Subject: Solving the "loss of input" problem: RunBlind.pas

[Hans Blom, 961219c]

(Bruce Abbott (961218.1810 EST))

>Enough of these verbal arguments! Appended below is a Pascal
>program, RunBlind.pas ... Pressing the space bar switches the
>controller to "blind" mode, at which time it uses its internal model
>of the "modeled" disturbance to determine its control actions. The
>actual disturbance acting on the CV is an additive combination of
>modeled and unmodeled disturbances.

Brilliantly simple idea, Bruce: separate the feedback perception into a fully predictable and a fully unpredictable part. Great!

Greetings,

Hans

Date: Thu, 19 Dec 1996 08:59:59 -0600
From: Bruce Abbott <abbott@CVAX.IPFW.INDIANA.EDU>
Subject: Re: Rick' "corrected" code

[From Bruce Abbott (961219.1000 EST)]

>Rick Marken (961218.2230)]

>Here's the corrected code for your model, Bruce:

>[botched "correction" that turns correct model into incorrect one]

>Note that CVm is the "real" environmental variable that is to be
>controlled; it is always equal to o+dUnmod, where Unmod is the real
>disturbance. When the model is "blind", CV is the imagined input to the
>perceptual function; so it is o+dMod, where dMod is the imagined
>disturbance.

No, Rick, if CVm is the "real" environmental variable, then it _should_ always be equal to o + dMod + dUnmod, as I have it in the original code. dMod + dUnmod together are the "real" disturbance.

In the program, CV is the perceptual variable controlled by the system, and it is _always_ controlled. CVm is the environmental variable, the perception of which is lost under the blind condition. When I began to write the program, I had indended CVm to be the modeled CV, but then I realized that I could use the same CV variable within the controller whether CV represented the "real" or "modeled" CV. However, I still needed to compute the "real" CV during times when the controller was running blind (for display purposes), so I reused CVm to represent that variable. I which I had changed that to "EV," or something like that, because it seems to be causing a lot of confusion.

>This is a very cool program Bruce; the graphs are beautiful and
>it shows rather clearly just what happens when a control system
>goes "blind" and cannot imagine (model) _exactly_ what is happening
>to the environmental variable that had been under control before the
>loss of input.

Thanks. Your conclusion is absolutely correct, although it applies to the program as I wrote it and not to your altered version.

>Thanks for sharing;-)

Hmm, more sarcasm.

You know, I'm having a strong feeling of "deja vu all over again." Remember the battle we had over the final version of my "reinforcement-based" e-coli demo? After "explaining" to me how my program just couldn't work the way I claimed it did (and even offering "proof" in the form of your own computer program that botched the computations), you and Bill finally had to admit that I was right. I was right then, and I'm right now. So there. (;->

Regards,

Bruce

Date: Thu, 19 Dec 1996 09:55:53 EST
From: Bruce Gregory <gregory@CF4.HARVARD.EDU>
Subject: Re: In Defense of Popper & Harms

[From Bruce Gregory (961219.1000 EST)]

Scott Stirling 18 Dec 1996 18:09:51

> Do you think I should make it clear that I do not think it's
> possible to disprove the existence of an external reality? I mean it
> works both ways. I guess I've just been focusing on the assertion that it
> is possible to know that perception correlates to a "real world." If
> someone were arguing as strongly for the opposite, that it is possible to
> know that there is no "real world" to which our perceptions correspond, I
> would focus on that assertion, all the while trying to bring the
> realm of knowledge or "having no doubt" back to more middle territory.

Perhaps it would help if you gave an example of what you _do_
know. What _can_ you prove? How _do_ you prove it? What is more
certain to you than the existence of a "real world"?

Bruce Gregory

Date: Thu, 19 Dec 1996 08:49:00 -0700
From: "T. B. Harms" <tbh@TESSER.COM>
Subject: Re: In Defense of Popper & Harms

*from Tracy Harms (961219.08)

>Scott Stirling 18 Dec 1996 18:09:51
>
[...]
>> I would focus on that assertion, all the while trying to bring the
>> realm of knowledge or "having no doubt" back to more middle territory.
>
>Perhaps it would help if you gave an example of what you _do_
>know. What _can_ you prove? How _do_ you prove it? What is more
>certain to you than the existence of a "real world"?
>
>
>Bruce Gregory (961219.1000 EST)

Rather than ask these questions, I desire to explore this Faustian union
between "the realm of knowledge" and "having no doubt". For starters let
me propose that these two matters are very separate: improvement of
knowledge is independent of increases in confidence.

Tracy Bruce Harms tbh@tesser.com

Boulder, Colorado

Date: Thu, 19 Dec 1996 09:59:00 -0600
From: Bruce Abbott <abbott@CVAX.IPFW.INDIANA.EDU>
Subject: Re: RunBlind.pas

[From Bruce Abbott (961219.1100 EST)]

The code in the StepModel procedure of RunBlind.pas can be improved slightly by making the following change:

Old:

```
  if CanSee then CVm := CV else CVm := o + dMod + dUnmod;
```

New:

```
  CVm := o + dMod + dUnmod;
```

This has no effect on program operation, because when CanSee is true, then CV equals o + dMod + dUnmod; thus CV equals o + dMod + dUnmod whether CanSee is true or false.

I suggest replacing all instances of CVm in the program with EV, in order to make it clear that CVm is the environmental variable to be controlled. However, again, this will not affect program operation, so this change is completely optional.

>[Hans Blom, 961219c] --

>Brilliantly simple idea, Bruce: separate the feedback perception into
>a fully predictable and a fully unpredictable part. Great!

Thanks, Hans!

By the way, the "fully unpredictable part" is actually predictable, because of laziness on my part. (I constructed it from sine and cosines a la Fourier.) However, a smoothly changing random disturbance such as is found in Bill Powers's simulations would work as well as the unpredictable portion of the disturbance here.

Regards,

Bruce

Date: Thu, 19 Dec 1996 08:00:23 -0800
From: Richard Marken <marken@AEROSPACE.AERO.ORG>
Subject: Re: Lies, damned lies, and programs

[From Rick Marken (961219.0800)]

Bruce Abbott (961219.1000 EST)--

>No, Rick, if CVm is the "real" environmental variable, then it _should_
>always be equal to o + dMod + dUnmod, as I have it in the original code.

>dMod + dUnmod together are the "real" disturbance.

I thought dMod was the modeled disturbance. How can a modeled (imagined) disturbance have an effect on a variable in the environment?

If you check my code carefully I think you'll find that it correctly implements the model you described. The model will, indeed, _appear_ to keep the environmental variable (CVm) under control if dMod = dUnmod. To the extent that dMod differs by even a teensy, weensy bit from dUnmod, control of the environmental variable breaks down (although control of the perceived CV is maintained, of course). Gee, now that I think of it, your model is a perfect description of your behavior on the net. You have your understanding of PCT (CV in your model) completely under control because you are controlling against an imaginary representation of the disturbance; from out here it's easy to see that you haven't got PCT (CVm) under control at all;-)

>You know, I'm having a strong feeling of "deja vu all over again."

I was asked to give you two years because that's how long it typically takes people to understand PCT. Your two years are up;-)

>Remember the battle we had over the final version of my "reinforcement-
>based" e-coli demo? After "explaining" to me how my program just couldn't
>work the way I claimed it did (and even offering "proof" in the form of your
>own computer program that botched the computations), you and Bill finally
>had to admit that I was right. I was right then, and I'm right now. So
>there. (;->

Not quite. There were two results of your e-coli demonstration. First, you showed that you could make the results of tumbles non-random by defining a result as the _change_ from the current to the present input concentration gradient. You still had to build a control model (one which delayed a tumble in proportion to the size of the post-tumble difference between input and reference) to get e-coli to navigate to the goal. Second, you _were_ able to build a reinforcement model that was a learning model; this model changed the loop gain of the control system organization of e-coli (which started with delay lengths being equally probable for all consequences of tumbles) on the basis of the _change_ in concentration after a tumble. This model could _only_ learns to move to the goal which as _defined into_ the way the measure of this _change_ was computed. And it could not learn at all when the change in concentration was itself made random.

No one disagreed with the fact that your programs worked. We just disagreed with what you claimed that they showed (that a reinformcent model could account for the behavior of e-coli).

Hans Blom (961219c)--

>Brilliantly simple idea, Bruce: separate the feedback perception into
>a fully predictable and a fully unpredictable part. Great!

See the kind of friends you make when you develop cause-effect models of control;-)

Best

Rick

Date: Thu, 19 Dec 1996 09:08:19 -0700
From: "T. B. Harms" <tbh@TESSER.COM>
Subject: Re: Solving the "loss of input" problem: RunBlind.pas

*From Tracy Harms 1996;12,19.09:00

Bruce Abbott (961218.1810 EST)

Interesting study. For awhile I had a problem making sense of

```
  if CanSee then d := dMod + dUnmod
    else d := dMod;
  CV := o + d;
```

But then I saw that this simply represents the aspect of your model-based structure which relies on an internal model if primary input is not available.

The problem I have now is, what can effect this switching between sensor and model? In your program you have a signal (change of space-bar status) which indicates whether or not to use one or the other. But as Bill Powers has recently (and I presume repeatedly) noted, signal-channels are not labelled. That is, there is no change to input-sensors which signals blindness. Another structure must evaluate whether or not to trust the sensors, or the model. What now exists as a combination of space-bar and "if CanSee" need to be represented *as a control structure* before I'll find your program persuasive.

Tracy Bruce Harms tbh@tesser.com
Boulder, Colorado

Date: Thu, 19 Dec 1996 09:23:06 -0700
From: Bill Powers <powers_w@FRONTIER.NET>
Subject: Re: resend: single top-level goal?

[From Bill Powers (961219.0745 MST)]

Hans Blom, 961219 --

>>Suppose you have two control systems at the same level, coupled
>>together at a lower level so that both cannot simultaneously achieve
>>zero error.
>
>I tried to discover a physical realization (block diagram, if you
>want) of your formulas. I can't. Although you use symbols, I cannot
>discover a meaning. It's a situation resembling statements like "it
>rains but it does not rain" or "x=3 and x=4". Logical contradictions,
>in other words. The problems are here:

Imagine two people trying to push a swinging door open at the same time, from opposite sides. Each is trying to achieve a reference condition for the angle of the door relative to the frame. The reference conditions are different. The formulas describe the combined systems at the moment the conflict becomes evident. Of course the real people will resolve the

conflict quickly, but that's not the point. The formulas describe (very roughly) the conflict while it exists.

Keep trying, Hans; you'll get it. It's just a simple analysis of a physical situation.

Best,

Bill P.

>
>>Let e be the error. When system 1 alone is acting, the value of x,
>>the common lower-level variable, will be G1*e1. When system 2 alone
>>is acting, x will be G2* e2. When both systems are acting
>
>How are the outputs of both systems (physically) combined? You do not
>specify that. As an addition? No. As an average? No. You set them to
>be equal:
>
>>, we introduce the constraint G1*e1 = G2*e2 = x
>
>This is a situation that most resembles short-circuiting one output
>with another system's output: x = output1 = output2. If so, the model
>isn't as accurate/complete as it need be to describe this situation,
>because it won't now be true that output1 = G1*e1 _and_ output2 =
>G2*e2 if G1 and G2 are to have their normal (non-short- circuit)
>meanings.
>
>Note also that e1 and e2 require a meaning in the combined system.
>They were defined for other conditions.
>
>>Let e = r - x, so we have G1*(r1 - x) = G2*(r2 - x).
>
>In a short-circuit situation, this won't generally be true if G1 and
>G2 are to have their normal (non-short- circuit) meanings.
>
>>Solving for x, we obtain x = (G2*r2 - G1*r1)/(G1 - G2)
>>Note that as the gains approach equality, x increases without limit
>>when r1 is not equal to r2.
>
>Infinities ought to warn you that your model cannot be a correct
>representation of some physical reality.
>
>>This may alter your analysis of error minimization.
>
>Not yet. Although reality (I will continue to use that word) can be
>modelled or expressed as words or formulas, not every model or set of
>words or formulas can be given a physical meaning. It seems to me
>that you play with words (formulas).
>
>Greetings,

>
>Hans
>
>

Date: Thu, 19 Dec 1996 09:23:10 -0700
From: Bill Powers <powers_w@FRONTIER.NET>
Subject: Re: In Defense of Popper & Harms

[From Bill Powers (961218.0745 MST)]

Hans Blom, 961219b --

>(Bill Powers (961217.1100 MST))
>
>>>2. If we believe we can detect "consistency over time", it seems to
>>>me that we have some inner mechanism that allows this detection.
>
>>Yes, it's called a perceptual input function.
>
>You misunderstand me. With "consistency over time" I mean, for
>example, that "the sun rises again and again". A perceptual input
>function may tell me that "the sun rises", but not -- by itself --
>that this occurrence is similar to other sunrises that I have
>observed in the past.

If you know that it is similar to sunrises you have observed in the past, then you're _perceiving_ a similarity between a remembered sunrise and a presently-seen sunrise. If you can recognize "again and again", then you are _perceiving_ "again and again."

I'm beginning to understand the logic of the Pascal compiler. When a compilation error is encountered, a great deal of wasted communication is avoided if you just stop on the first error. The C compiler give you a long list of errors, most of which are meaningless once the first error is corrected.

Best,

Bill P.

To establish the similarity, it appears to me,
>there must be _two_ things to be compared. What I assume is that one
>is in actual perception (the sunrise that I observe now), and one is
>in memory (about sunrises that I've observed in days, months and
>years past). That seems pretty inescapable, even tautological...
>
>If you believe that such "consistency over time" is established by a
>perceptual input function, it is one that encodes sequences, I guess.
>But a "sequence" still links two occurrences, at least one of them
>from the past, and needs a similar mechanism.
>
>Greetings,
>
>Hans
>
>

Date: Thu, 19 Dec 1996 09:23:14 -0700
From: Bill Powers <powers_w@FRONTIER.NET>
Subject: Re: Solving "loss of input" problem

[From Bill Powers (961218.0800 MST)]

Hans Blom, 961219c --

>> In the real world, if
>>you "do X" ten times in a row, you will get nine different values of Y.

>I did The Test. The last ten times I turned my car's steering wheel
>to the right, the car was perceived to turn to the right. Not once to
>the left.

> What kind of car do you have?

In my car, the lateral acceleration (the curvature of the path) is proportional to the amount of turn of the steering wheel. In performing the action I call "turning a corner", the amount of lateral acceleration required depends on the tilt of the roadbed, the wind, the loading of the car, the speed of the car, the braking force, the width of the lane, the initial position and velocity of the car as I approach the corner, and numerous other factors which never repeat. If I simply repeat the same turning actions on the wheel that I used the last time, I will end up in oncoming traffic or clip the edge of the roadway on the inside of the turn. The errors are magnified by the fact that the position of the car relative to the correct path is a double time integral of the steering force.

Does your car run on rails?

Best,
Bill P.

Date: Thu, 19 Dec 1996 09:23:17 -0700
From: Bill Powers <powers_w@FRONTIER.NET>
Subject: Re: Lies, damned lies, and programs

[From Bill Powers (961218.0815 MST)]

Bruce Abbott (961219.0910 EST) --
Rick Marken (961218.1630) --

You two guys sound like squabbling siblings. You get so mad that neither of you thinks straight. What a waste of talent. Daddy loves you both.

Rick, if you had been less hasty to leap to the attack, you would have realized that dUnmod means "disturbance, unmodeled," and dMod means "disturbance, modeled." So you reversed their roles. And you missed the fact that the modeled CV, CVm, is calculated but not used.

Bruce, if you had been less eager to defend your model, you would have realized that dUnmod is never perceived by the system; it is only CV that is

perceived, so if there is "loss of input" in the sense in which we have been using the term in connection with Hans' model, it is CV that would no longer be perceived. Disturbances are not perceived in any case. What your program does in the so-called "blind" condition is to cease applying dMod to the CV and start applying it to CVm -- which isn't in the control loop.

Your model can be made to work with loss of input if you simply put in a switch so the perception depends on either CV (actual controlled variable) or CVm (modeled controlled variable):

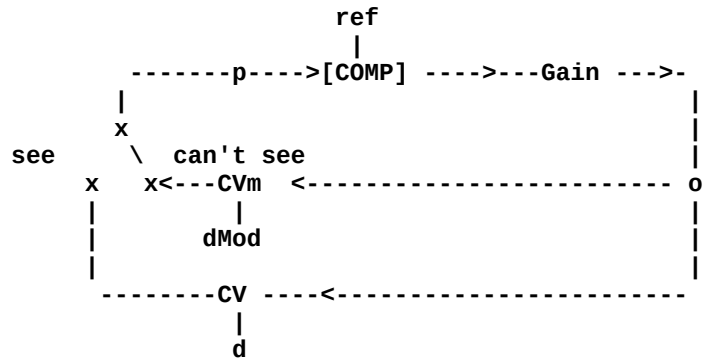
```
if CanSee then p := CV else p := CVm;
```

When you do that, the use of two disturbances becomes superfluous. Now you simply need

```
CV := o + d;
CVm := o + dMod;
```

where dMod is the model of the real disturbance d. You could, of course, also have a dUnmod affecting the real CV and not CVm.

What you have now is the same as my suggested modification of Hans' model, simplified, where either the modeled perception or the real-time perception is compared with the reference signal, the position of the switch depending on whether real-time input is available.



The switch is shown in the "blind" position. In the blind condition, CVm is perceived instead of CV. Now your model will work when the perceptual input from the environment is lost, so CV is no longer perceived. CVm is perceived instead.

However, you now have to explain how dMod can be made to be the same as d. There's nothing in the model that would make this happen.

Adding Hans' Extended Kalman Filter (EKF) would do the trick, because it compares CVm and CV and adjusts the model to make them the same under all conditions. This would include making dMod the same as d. No output function is shown in the above diagram -- it's just a unity multiplier -- so it's not obvious that the EKF would be altering a modeled output function to give o the same effect on CVm as it has on CV. Putting in an explicit feedback function would make this clearer. And of course if d is predictable only over a very short time span, then the model's dMod could be correct only over a similar short time span after input is lost.

Bill P.

Date: Thu, 19 Dec 1996 10:45:40 -0600
From: Bruce Abbott <abbott@CVAX.IPFW.INDIANA.EDU>
Subject: Re: Solving the "loss of input" problem: RunBlind.pas

[From Bruce Abbott (961219.1145 EST)]

>Tracy Harms 1996;12,19.09:00 --

>The problem I have now is, what can effect this switching between sensor
>and model? In your program you have a signal (change of space-bar status)
>which indicates whether or not to use one or the other. But as Bill Powers
>has recently (and I presume repeatedly) noted, signal-channels are not
>labelled. That is, there is no change to input-sensors which signals
>blindness. Another structure must evaluate whether or not to trust the
>sensors, or the model. What now exists as a combination of space-bar and
>"if CanSee" need to be represented *as a control structure* before I'll
>find your program persuasive.

You are quite right to note that the current program does not have a realistic means of detecting when its input has been lost, and that "another structure must evaluate whether or not to trust the sensors, or the model." The purpose of the current program is to explore how well a model-based system would work _if_ such a higher-level system existed and functioned properly. It seems to me that, before we spend a lot of time attempting to build the "loss of input" sensor (a perceptual function), we need to evaluate how well a model-based control system would perform under various realistic conditions. If it turns out to do poorly even under the circumstances most favorable to it, then there would be no need to develop the model further. The present program is offered as a first step in that evaluation. By trying different disturbance waveforms (with various mixes of amplitudes and frequencies), we could discover the conditions under which model-based control can provide a useful solution to the loss of input problem. A further set of experiments could be devised in which the modeled disturbance and/or modeled EFF depart in various ways from the way the real variables behave. I invite you to experiment.

That said, I do have complete confidence that a loss-of-input perceptual function can be constructed and implemented within a superordinate control system that would take over the function of the space bar in the present program. After all, _we_ seem to have no problem detecting such failures, and there are already missle systems that "recognize" when they have "acquired" or "lost" a target.

Regards,

Bruce

Date: Thu, 19 Dec 1996 10:28:40 -0700
From: "T. B. Harms" <tbh@TESSER.COM>
Subject: Re: RunBlind.pas

*From Tracy Harms 1996;12,19.10:30

Bruce Abbott (961219.1100 EST)

>I suggest replacing all instances of CVm in the program with EV, in order to
>make it clear that CVm is the environmental variable to be controlled.

Thanks for the clarification. Previously I hadn't a clue as to what CVm
was supposed to mean.

Now that I've heard, however, I have a new concern. What is an
"environmental variable to be controlled"? As I understand it only
perception is controlled, not behavior, and _not_ _environment_. Then
again, I'm fresh to programmatic representations of PCT, so you may have
something here which is perfectly fine, it just hasn't been adequately
explained for me.

Looking at the code, I'd say CVm-aka-EV is simply the value expected if
unblinded, and that it is desired for display purposes to show
discrepency-when-blind. In this case I'd suggest also adding another
variable which := o + dUnmod ---that is, an indicator of what a
non-modeling non-blinded system would control in response to.

Tracy Bruce Harms tbh@tesser.com
Boulder, Colorado

Date: Thu, 19 Dec 1996 13:42:52 EST
From: Bruce Gregory <gregory@CF4.HARVARD.EDU>
Subject: Re: In Defense of Popper & Harms

[From Bruce Gregory (961219.1345 EST)]

Tracy Harms (961219.08)

> Rather than ask these questions, I desire to explore this Faustian union
> between "the realm of knowledge" and "having no doubt". For starters let
> me propose that these two matters are very separate: improvement of
> knowledge is independent of increases in confidence.

No arguments from me. I was trying to get at a similar
distinction. From my point of view, improvements in knowledge
are what we get when our models get better at predicting our
experiences. An increase in confidence is what we get when we
perceive that our knowledge is improving.

Bruce Gregory

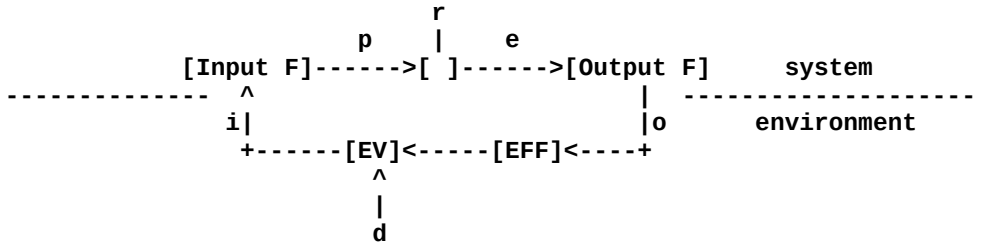
Date: Thu, 19 Dec 1996 13:08:18 -0600
From: Bruce Abbott <abbott@CVAX.IPFW.INDIANA.EDU>
Subject: Re: RunBlind.pas

[From Bruce Abbott (961219.1410 EST)]

>Tracy Harms 1996;12,19.10:30 --

>Thanks for the clarification. Previously I hadn't a clue as to what CVm
>was supposed to mean.
>
>Now that I've heard, however, I have a new concern. What is an
>"environmental variable to be controlled"? As I understand it only
>_perception_ is controlled, not behavior, and _not_ _environment_. Then
>again, I'm fresh to programmatic representations of PCT, so you may have
>something here which is perfectly fine, it just hasn't been adequately
>explained for me.

I've sketched in the block diagram of a simple control system below:



What I've called the "EV" is the environmental representation of the controlled perceptual variable, whose effect on the input function (sensors) produces the controlled perception, p. The usual PCT system knows nothing about the environment. The EV is acted upon by disturbances, d, and by the system output (acting through the environmental feedback function, or EFF).

For example, the EV might be the temperature of a room, p is the sensed temperature, o is the electrical signal to a furnace, which "converts" the electrical signal into heat. This heat and the disturbance (heat leakage into or out of the room) affect the value of the EV (i.e., the temperature of the room). The system controls p, not EV, but this works find as long as p and EV are properly related (i.e., the sensor works properly).

The model embodied in my program is assumed to be capable of determining the predictable (regular) portion of variation in d, which it can do because it "knows" how its own actions affect p and thus can presumably extract from p an estimated d. I am assuming that it uses something like Kalman filtering to extract any regular waveform from d, which becomes its model of the disturbance. Of course, in the program I skipped over all these details and just set dMod equal to a component of the total disturbance waveform.

>Looking at the code, I'd say CVm-aka-EV is simply the value expected if
>unblinded, and that it is desired for display purposes to show
>discrepency-when-blind. In this case I'd suggest also adding another
>variable which := o + dUnmod ---that is, an indicator of what a
>non-modeling non-blinded system would control in response to.

The first statement is correct, but the suggestion is not. Remember, the total disturbance is _assumed_ to be what I call dMod _plus_ dUnmod. That is, the disturbance as implemented in the program is

2.0*sin(t) + 0.5*sin(3.0*t/2.0) + 0.5*sin(t/3.0) + 0.5*cos(t/5.0).

This is what a non-modeling non-blinded system would control in response to (actually to variations in p induced by this disturbance), and _is_ what the top line of the computer display shows when the system given is controlling in "sighted" mode. It is assumed that the system's model of this

disturbance is 2.0*sin(t), extracted by the method alluded to above.

Regards,

Bruce

Date: Thu, 19 Dec 1996 11:31:07 -0800
From: Richard Marken <marken@AEROSPACE.AERO.ORG>
Subject: Re: Lies, damned lies, and programs

[From Rick Marken (961219.1130)]

Bill Powers (961218.0815 MST)]

>Rick, if you had been less hasty to leap to the attack, you would have
>realized that dUnmod means "disturbance, unmodeled," and dMod means
>"disturbance, modeled." So you reversed their roles. And you missed the fact
>that the modeled CV, CVm, is calculated but not used.

And if you had been less hasty to leap to the conclusion that I was "attacking" you would see that I knew what dMod and dUnmod meant and you would have seen that CVm has the role, in Bruce's model, not of the modeled CV but of the actual environmental variable; In Bruce's code, CV had the role of the neural input to a perceptual function (with p as the oputput).

>Your model can be made to work with loss of input if you simply put in a
>switch so the perception depends on either CV (actual controlled variable)
>or CVm (modeled controlled variable):

>if CanSee then p := CV else p := CVm;

>When you do that, the use of two disturbances becomes superfluous. Now you
>simply need

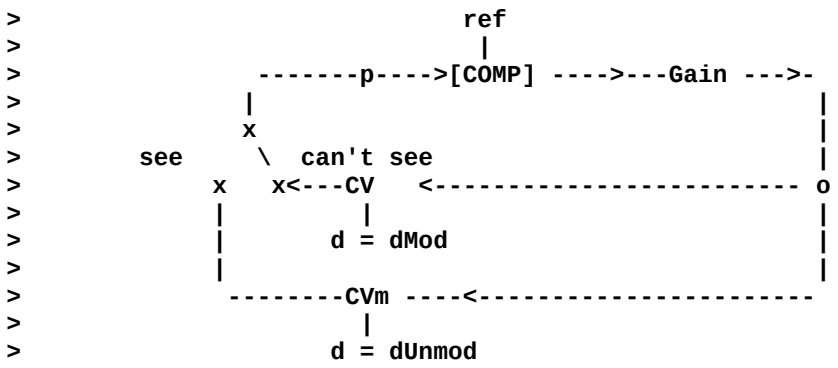
>CV := o + d;
>CVm := o + dMod;

But that changes the meaning of CV and CVm that Bruce was using (I know that CVm must have been named under the assumption that itt was the "modeled" CV but it didn't function that way in the program). My suggested change to Bruce's code (below) accomplishes _exactly_ what isaccomplished by your suggested code change -- but mine has the virtue of preserving the (admittedly weird) semantics of Bruce's variable names. Here, again is my code:

```
procedure StepModel;
 begin
 p := CV;
 e := r - p;
 o := o + (g*e - o)*s*dt;
 dMod := 2.0*sin(t);
 dUnmod := 0.5*sin(3.0*t/2.0) + 0.5*sin(t/3.0) + 0.5*cos(t/5.0);
 if CanSee then d := dUnmod
   else d := dMod;
 CV := o + d;
 CVm := o + dUnmod;
 t := t + dt;
```

```
 end;
```

And here how it maps into your diagram of control with and without the ability to "see":



So, you may love us both, but I'm actually the best person;-))

Love

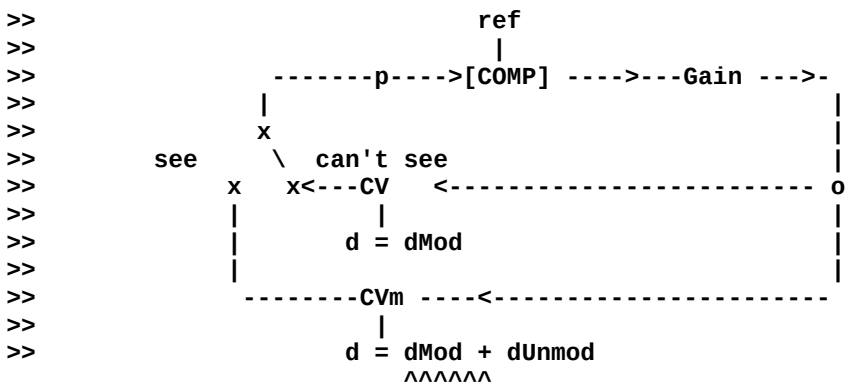
Rick

Date: Thu, 19 Dec 1996 13:11:16 -0700
From: "T. B. Harms" <tbh@TESSER.COM>
Subject: Re: Lies, damned lies, and programs

*From Tracy Harms 1996;12,19.13:00

Rick Marken (961219.1130)

Given what has been clarified by Bruce Abbott (961219.1410 EST) I'd say your diagram would need be corrected as follows:



Bruce has informed us that dMod is the portion of the actual disturbance, d, which is accurately modelled.

Tracy Bruce Harms tbh@tesser.com
Boulder, Colorado

Date: Thu, 19 Dec 1996 15:01:26 EST
From: Bruce Gregory <gregory@CF4.HARVARD.EDU>
Subject: Re: Lies, damned lies, and programs

[From Bruce Gregory (961219. 1500 EST)]

Rick Marken (961219.1130)

> So, you may love us both, but I'm actually the best person;-))

Actually, you are the _better_ person. _I_ am the best person
;-)

Bruce Gregory

Date: Thu, 19 Dec 1996 14:26:50 -0600
From: Bruce Abbott <abbott@CVAX.IPFW.INDIANA.EDU>
Subject: Sibling rivalry; solving the "loss of input" problem

[From Bruce Abbott(961219.1530 EST]

>Bill Powers (961218.0815 MST) --

>You two guys sound like squabbling siblings. You get so mad that neither of
>you thinks straight. What a waste of talent. Daddy loves you both.

Thanks "Dad." But he hit me first! (;->

Interesting concept, and makes good sense in the light of the recent discussion on birth order. As the younger intellectual offspring, I tend to be the rebellious, creative one, whereas Rick, being "older," tends to be more conservative, more comfortable defending the status quo. Hmm, yes, I see it all now . . . (;->

>Bruce, if you had been less eager to defend your model, you would have
>realized that dUnmod is never perceived by the system; it is only CV that is
>perceived, so if there is "loss of input" in the sense in which we have been
>using the term in connection with Hans' model, it is CV that would no longer
>be perceived. Disturbances are not perceived in any case. What your program
>does in the so-called "blind" condition is to cease applying dMod to the CV
>and start applying it to CVm -- which isn't in the control loop.

I hope my clarifications (which apparently hadn't reached you at the time you wrote this post) have helped. CVm just represents the true environmental variable of which p is sometimes a function, sometimes not, and was included only to so that its value could be displayed during the run. CV in the program sometimes represents CVm ("sighted mode") and sometimes the modeled CV ("blind mode").

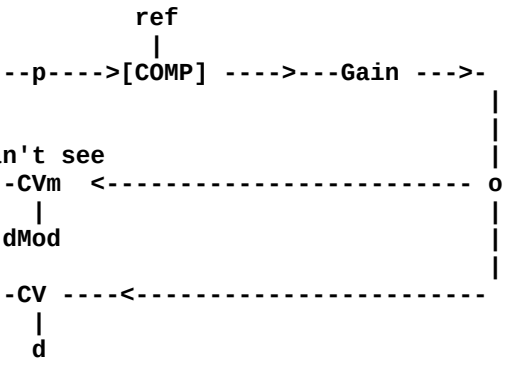
>Your model can be made to work with loss of input if you simply put in a

>switch so the perception depends on either CV (actual controlled variable)
>or CVm (modeled controlled variable):
>
>if CanSee then p := CV else p := CVm;
>
>When you do that, the use of two disturbances becomes superfluous. Now you
>simply need
>
>CV := o + d;
>CVm := o + dMod;

Yes, that would have been cleaner, and the change restores the original intended meaning of CM and CVm.

>where dMod is the model of the real disturbance d. You could, of course,
>also have a dUnmod affecting the real CV and not CVm.

I'm not sure what you mean by this. In CV := o + d, d already includes dUnmod. That is, d = dMod + dUnmod. It would be incorrect to use CV := o + dUnmod.

>What you have now is the same as my suggested modification of Hans' model,
>simplified, where either the modeled perception or the real-time perception
>is compared with the reference signal, the position of the switch depending
>on whether real-time input is available.
>


>
>The switch is shown in the "blind" position. In the blind condition, CVm is
>perceived instead of CV. Now your model will work when the perceptual input
>from the environment is lost, so CV is no longer perceived. CVm is perceived
>instead.

Although the variable definitions have been reogranized a bit, the logical effect has not -- the program will produce exactly the same result as it does now, after your suggested code changes have been implemented.

>However, you now have to explain how dMod can be made to be the same as d.
>There's nothing in the model that would make this happen.

Correct: as I explained in a post to Tracy Harms, I left out these details in order to examine the implications of being able to model a portion of the disturbance function, assuming that the relevant mechanism has already done its job.

>Adding Hans' Extended Kalman Filter (EKF) would do the trick, because it
>compares CVm and CV and adjusts the model to make them the same under all

>conditions. This would include making dMod the same as d. No output function
>is shown in the above diagram -- it's just a unity multiplier -- so it's not
>obvious that the EKF would be altering a modeled output function to give o
>the same effect on CVm as it has on CV. Putting in an explicit feedback
>function would make this clearer. And of course if d is predictable only
>over a very short time span, then the model's dMod could be correct only
>over a similar short time span after input is lost.

The difference is that Hans's model operates in "model" mode continuously, and uses the sensed behavior of the actual CV only to update the model, and to determine when CV and model are seriously discrepant. This model uses actual input so long as it remains available, then switches to the model when the system determines that the sensory input is missing. (As noted above, the mechanism whereby this is done was purposely left out of the program.) See my posts to Tracy for a discussion of the modeling of the disturbance and EFF.)

We seem to be in agreement that this is a viable model for a "run-blind" system, so far as it goes. I finessed some important details, of course, in order to present a model for investigation that would _already have_ accurate models of the EFF and the predictable portion of the disturbance waveform. The question to be addressed by this model is, how well does it do under various scenarios?

With respect to Han's model-based controller, I am reminded of a story about some oysters that were shipped from their original home on the Atlantic seaboard to an aquarium in the midwest. Oysters close up during periods of low tide and open up again when the tide rises. For the first week or so after their arrival in their new "digs," the oysters continued to open and close according to the tidal cycle _on the east coast_, then gradually resynchronized to the tides as they would have existed if the coastline were relocated to the midwest.

This behavior shows clearly that the cyclical opening and closing is orchestrated by an internal "clock" normally synchronized to the tides. In effect this internal, time-varying signal is the oyster's model of the tides, which is phase-locked to the real tides through some kind of signal as yet to be determined. A slow advancement or retardation of this cycle occurs when the signal and cycle are out of phase, until the error is canceled. Like Hans's controller, the oyster operates on an internal model and uses the outside world only as a way to update (correct) the model.

Regards,

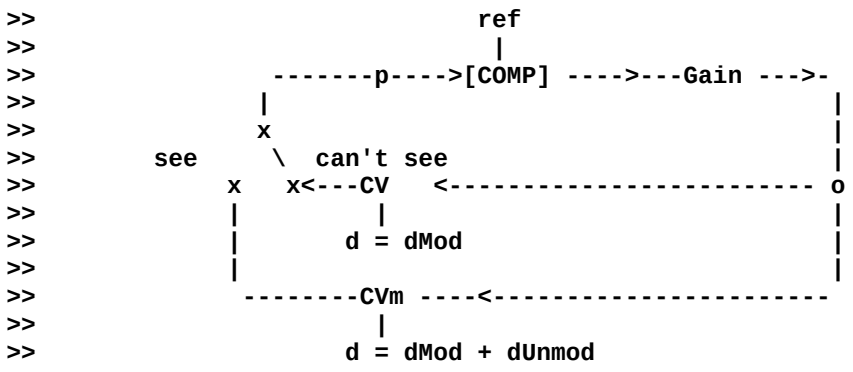
Bruce

Date: Thu, 19 Dec 1996 12:40:26 -0800
From: Richard Marken <marken@AEROSPACE.AERO.ORG>
Subject: Lies, damned lies and BYTE magazine

[From Rick Marken (961219.1240)]

Tracy Harms (1996;12,19.13:00)

>Given what has been clarified by Bruce Abbott (961219.1410 EST) I'd say
>your diagram would need be corrected as follows:



>Bruce has informed us that dMod is the portion of the actual disturbance,
>d, which is accurately modelled.

Sounds good to me. My prediction is that even if dUnmod is a very small component of the variance in d, any apparent control of CVm (in the blind) will disappear. If dUnmod = 0 it will _look like_ CVm is controlled perfectly; (and, according to cause-effect theorists, it _is_ being controlled perfectly).

Bruce Gregory (961219. 1500 EST)

>Actually, you are the _better_ person. _I_ am the best person;-)

I'll take it! I would have settled for anything above "asshole";-)

Stefan Balke (961218.1720 CET)--

>I would like to read this BYTE-articles, but unfortunately they are not
>available at our library.

I don't think the BYTE articles are reprinted anywhere. Looks like you're out of luck until you can find a library that stocks obsolete computer journals. Until then I recommend Bill's _Psych Review_ paper; it's the "Quantitative analysis of purposive systems" paper which is reprinted in _Living Control Systems_.

Best

Rick

Date: Thu, 19 Dec 1996 15:20:21 -0600
From: Bruce Abbott <abbott@CVAX.IPFW.INDIANA.EDU>
Subject: Re: Lies, damned lies, and Rick Marken

[From Bruce Abbott (961219.1620 EST)]

>Rick Marken (961219.1240) --

>>Tracy Harms (1996;12,19.13:00)

>>Bruce has informed us that dMod is the portion of the actual disturbance,
>>d, which is accurately modelled.

>Sounds good to me. My prediction is that even if dUnmod is a very small
>component of the variance in d, any apparent control of CVm (in the blind)
>will disappear. If dUnmod = 0 it will _look like_ CVm is controlled
>perfectly; (and, according to cause-effect theorists, it _is_ being
>controlled perfectly).

Why don't you just take a look at the program's output? It is _already_ operating with dUnmod a _fairly significant_ component of the variance in d. You will see that control under this condition isn't great, but is still better than what would be observed if CVm were not under control at all.

Also, I have also noted for the record that CVm only _appears_ to be controlled (to the extent that the model matches), so this is not a point of disagreement between us. It _appears_ to be controlled in the same sense that, in the standard PCT model, the EV whos value serves as input to the perceptual input function _appears_ to be controlled. Real control of the EV exists only to the extent that p and EV positively correlate.

Regards,

Bruce

Date: Thu, 19 Dec 1996 14:31:11 -0700
From: "T. B. Harms" <tbh@TESSER.COM>
Subject: apparent control

*From Tracy Harms (1996;12,19.15:20)

Rick Marken (961219.1240)]

>[...]My prediction is that even if dUnmod is a very small
>component of the variance in d, any apparent control of CVm (in the blind)
>will disappear.

That sounds a lot weaker than the denunciations of model-based control we heard earlier. I doubt there is any dispute that control deteriorates. The question seems to be the range over which models can aid control despite loss of relevant input. That's a question of cost/benefit ratios, much of which will depend on typical frequency and duration of interruptions, and also the severity of consequences for lacking an approximation versus the cost of approximating.

Tracy Bruce Harms tbh@tesser.com
Boulder, Colorado

Date: Thu, 19 Dec 1996 16:40:45 EST
From: Bruce Gregory <gregory@CF4.HARVARD.EDU>
Subject: Re: apparent control

[From Bruce Gregory (961219.1645 EST)]

Tracy Harms (1996;12,19.15:20)
>

>
> Rick Marken (961219.1240)]
>
> >[...]My prediction is that even if dUnmod is a very small
> >component of the variance in d, any apparent control of CVm (in the blind)
> >will disappear.
>
> That sounds a lot weaker than the denunciations of model-based control we
> heard earlier. I doubt there is any dispute that control deteriorates.
> The question seems to be the range over which models can aid control
> despite loss of relevant input. That's a question of cost/benefit ratios,
> much of which will depend on typical frequency and duration of
> interruptions, and also the severity of consequences for lacking an
> approximation versus the cost of approximating.

For some of us, the question is whether living control systems
employ this mechanism.

Bruce Gregory

Date: Thu, 19 Dec 1996 14:27:12 -0800
From: Richard Marken <marken@AEROSPACE.AERO.ORG>
Subject: Lies, damned lies and non-control

[From Rick Marken (961219.1430)]

>My prediction is that even if dUnmod is a very small component of the
>variance in d, any apparent control of CVm (in the blind) will disappear.

Bruce Abbott (961219.1620 EST)--

>Why don't you just take a look at the program's output? It is _already_
>operating with dUnmod a _fairly significant_ component of the variance in d.

I will compute this tonight when I have access to the program but I
think you will find that what is happening during the "blind" run is that
the dMod component of the disturbance is being completely opposed by o
while the dUnmod component goes completely unopposed. Since you have assured
yourself of a perfect model of the dMod component of the disturbance, it is
no surprise that this component of the disturbance is opposed by o. When I
say that there is _no control_ during the blind period I mean that there is
no opposition to the disturbance (dUnmod) that you have not had the courtesy
to tell the system about in the first place;-)

>Also, I have also noted for the record that CVm only _appears_ to be
>controlled (to the extent that the model matches), so this is not a point of
>disagreement between us.

Sorry. I guess I was misled by your continued claims that your model is
"closed loop" even in "blind" mode.

Me (again):

>My prediction is that even if dUnmod is a very small component of the
>variance in d, any apparent control of CVm (in the blind) will disappear.

Tracy Harms (1996;12,19.15:20) --

>That sounds a lot weaker than the denunciations of model-based control we
>heard earlier.

I was "denouncing" Hans' notion that model-based control is a reasonable model of most of the controlling done by living organisms. In fact, model-based control is 1) not control at all and 2) a ridiculously unrealistic model of organisms since it is virtually impossible for an organisms to model, with any degree of accuracy, the kinds of environmental changes it has to deal with every second of its life.

Believe me, I wouldn't be upset if Hans _were_ a model- based control system like the ones he builds. But, given the fact he's been around for so many years, it seems clear that he's almost certainly a perceptual control system like the rest of us.

>I doubt there is any dispute that control deteriorates [in model-based
>control].

I dispute it. Control doesn't deteriorate; it disappears. There is _no_ control when you are generating output based on a model. There is _only_ the appearance of control (if you are lucky).

>The question seems to be the range over which models can aid control
>despite loss of relevant input.

Not really. Model based control can only "work" (give the appearance of control) in a world with no disturbances or with disturbances that are highly predictable. That isn't this world.

>That's a question of cost/benefit ratios,

There is, indeed, a cost/benefit basis for the use of model-based (non) control systems in engineering. Engineers use model based "control" systems because sensors are often the most expensive component of a controller. So the engineers are willing to give up control of a variable and use non-control systems (model- based "control" systems) to stabilize the variable as best as they can in environments that the engineers themselves design (control) to be as disturbance free (or as predictable) as possible. For example, they can use model-based control to control a chemical reaction that occurs in an enviroment that is well protected (via thick walls, etc) from the effects of variations in temperature, pressure, etc. Evolution didn't have that design option with living systems.

Best

Rick

Date: Thu, 19 Dec 1996 17:31:47 -0500
From: Scott Stirling <scstirli@ANSELM.EDU>
Subject: Re: In Defense of Popper & Harms

> Perhaps it would help if you gave an example of what you _do_
> know. What _can_ you prove? How _do_ you prove it? What is more
> certain to you than the existence of a "real world"?
>

>
> Bruce Gregory

These are excellent questions. Descartes seems like a nice way to start proving knowledge, but the problem with "cogito ergo sum" is that to _know_ that you are thinking presumes that you know there is a you who is thinking. Descartes wanted to show that there was a world to which perception corresponds, which he saw needed to be proved somehow. The trouble is that he took as self-evident that he, the unextended thinker, existed, which was his foundation for everything that followed. Now you can do that if you want, but you must realize that an argument from self-evidence is fundamentally shaky--it depends on your willingness to accept a premise without a logical argument. The epistemologist wants to _prove_, somehow, without resorting to self-evidential claims, that knowledge is possible (or impossible, if he is a skeptic).

Now, the skeptic and the classical foundationalist both argue from the same self-evidential claim, typically. That is, they both begin with the premise that there is a thinking being who is going to decide whether or not knowledge is possible. From there, skepticism falls because it is fundamentally an illogical arument, viz., it is possible to know that knowing is impossible. The classical foundationalist states that certain claims are irrefutable, e.g., the law of non-contradiction, or the proposition that it is possible to entertain propositions. Other types of epistemology like Locke's, Kant's, Popper's, Lorenz', etc. depend on some basic assumption that the existence of the knower is self-evident, along with some classical foundationalist claims about the nature of logic and the mind.

The fundamental problem then, is that there is no way to step outside my mind and perceive, objectively, that I am indeed there thinking and even if I could, I would have to do it again ad infinitum. This is why in my first post on this whole subject I mentioned Godel and Hofstadter--it is in Richard Hofstadter's book (_Godel, Escher, Bach_) in analogizing Godel's theorem that these points about the infinite regress of "how do you know?" have been made most public.

So I, and everyone else whether they realize it or not, make some basic assumptions about individual existence, and then about the the correspondence of logical thought to the perceptions the mind has. Bill Power's epistemology, along with that of Karl Popper as modified by Donald Campbell and explained by Gary Cziko, seems the best one I have ever heard. It requires the least reliance on absolutes certainties and irrefutable truths and all that stuff, of any epistemology I know. It remains open to adptation, and is a damn good working hypothesis.

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http://www.anselm.edu/student/scstirli/welcome.html

Date: Thu, 19 Dec 1996 15:40:16 -0700
From: Bill Powers <powers_w@FRONTIER.NET>
Subject: Re: Lies, damned lies, and programs

[From Bill Powers (961219.1422 MST)]

Tracy Harms 1996;12,19.13:00 --

I'm picking you as representing neutral ground.

>Given what has been clarified by Bruce Abbott (961219.1410 EST) I'd say
>your diagram would need be corrected as follows:

```
>>>                              ref
>>>                               |
>>>                -------p---->[COMP] ---->---Gain --->-
>>>               |                                      |
>>>               x                                      |
>>>        see     \  can't see                          |
>>>             x   x<---CV   <------------------------ o
>>>             |         |                              |
>>>             |       d = dMod                         |
>>>             |                                        |
>>>              --------CVm ----<----------------------
>>>                       |
>>>                     d = dMod + dUnmod
>                           ^^^^^^
>
```

>Bruce has informed us that dMod is the portion of the actual disturbance,
>d, which is accurately modelled.

Yes, I noted that dUnmod could be added to CV[m] if desired, except of course that I called it CV. I suppose that Bruce's program actually does what I have above (with your addition), but it's a bit confusing to have a symbol change from having one physical meaning to having a different one in the middle of the program. Can we pretty please now rearrange the definitions so that "m" indicates "model", and rewrite the program so CV always meams the real controlled variable outside the system,while CVm always means the modeled or imagined controlled variable inside the system?

Using "dMod" to indicate the modeled part of the real disturbance is confusing to me, because there is only one real disturbance. If you look at variations in the modeled disturbance, you'd end up using the same symbol, dMod, for two different variables (as, in fact, you're using "d").

It seems to me that the simplest way is to use dMod for the modeled disturbance and d for the real one. If dMod is different from d, then there will be a difference between CVm and CV. The unmodeled part of the real disturbance is then just dUnmod = d - dMod. dUnmod isn't another disturbance; it's just the discrepancy between the modeled and real disturbance, a calculation we make on a scratchpad somewhere else. It's not another variable in the model.

Bruce Abbott, will you take care of getting the program straightened out so you don't confuse poor old Dad?

Best,

Bill P.

Date: Thu, 19 Dec 1996 15:40:19 -0700
From: Bill Powers <powers_w@FRONTIER.NET>
Subject: Re: Sibling rivalry; solving the "loss of input" problem

[From Bill Powers (961219.1500 MST)]

Bruce Abbott(961219.1530 EST] --

>The difference is that Hans's model operates in "model" mode continuously,
>and uses the sensed behavior of the actual CV only to update the model, and
>to determine when CV and model are seriously discrepant. This model uses
>actual input so long as it remains available, then switches to the model
>when the system determines that the sensory input is missing. (As noted
>above, the mechanism whereby this is done was purposely left out of the
>program.) See my posts to Tracy for a discussion of the modeling of the
>disturbance and EFF.)

Yes, that is how Hans' model operates. My rearrangement was a suggestion that allows use of real-time input when it's available, and use of the model when it's not. This means that when the real input is available, disturbances can be resisted without any need to model or predict them. The model of the disturbance comes into play only when real input is lost (or perhaps when it's momentarily too noisy to use). It's always better to use the real input when possible.

>We seem to be in agreement that this is a viable model for a "run-blind"
>system, so far as it goes. I finessed some important details, of course,
>in order to present a model for investigation that would _already have_
>accurate models of the EFF and the predictable portion of the disturbance
>waveform. The question to be addressed by this model is, how well does it
>do under various scenarios?

Your program (as revised for clarity) makes it plain that given an accurate model of the EFF, the critical factor is how well the modeled disturbance goes on matching the real disturbance after the input is lost. This suggests a simplification of the Vancouver Experiment, which you might like to program. Just have a cursor moved by a mouse and disturbed in one dimension by a simple sine-wave disturbance, with the subject trying to keep the cursor next to a stationary mark on the screen. Loss of input is simulated by blanking the cursor.

A regular sine-wave disturbance is certainly easy to predict. In your model you assumed that a modeled sine-wave could be made to match a real sine-wave exactly. If the real disturbance were actually just a sine-wave, then the model-based behavior would be exactly like the behavior when the system can see the real CV. So we can test to see if a human being can actually construct a modeled sine-wave that imitates a real sine-wave disturbance, and see how accurately and for how long the model-based mode of operation can be carried on after loss of input. We can also compare the accuracy of control for a (hypothetical) model-based system and a PCT-type system.

There are some interesting alternate scenarios. One would be the use of a smoothed random disturbance as you suggested. Here, obviously, some method of generating a _predicted_ disturbance is required, because the real disturbance will go on varying as the random generator determines, while the internal model has to use past experience as a way of generating the predicted disturbance. You might try using a running extrapolation of the

disturbance. Since CVm = o + dMod, it follows that dMod = CVm - o. By using first, second, and perhaps third differences of the deduced dMod, you could extrapolate the disturbance ahead into a table. Then when the input is lost, the values of dMod could be taken from this table for use in the model. This is basically what Hans' EKF model does, although it is also adjusting the model parameters.

Obviously, how well the model does will depend on how long the modeled disturbance goes on matching the real one. It won't be nearly as long as it would be with a sine-wave disturbance. You could establish an upper limit just by taking the autocorrelation of the random disturbance.

Another scenario would involve using an integral EFF (environmental feedback function). In that case the output of the control system would be simply gain times error rather than the integral of gain times error. Now small deviations of the modeled disturbance from the real one would be integrated, and would have a much larger effect. In the test with a human being, you would just make the cursor position the integral of mouse deviations from some center point. My guess is that even with a simple slow sine-wave disturbance, the human being would fail very rapidly after the cursor is blanked out.

Best,

Bill P.

Date: Thu, 19 Dec 1996 17:34:15 -0500
From: Scott Stirling <scstirli@ANSELM.EDU>
Subject: Re: In Defense of Popper & Harms

> *from Tracy Harms (961219.08)

> Rather than ask these questions, I desire to explore this Faustian union
> between "the realm of knowledge" and "having no doubt". For starters let
> me propose that these two matters are very separate: improvement of
> knowledge is independent of increases in confidence.

I agree, T.B. Any misrepresentation on my part in the post of mine that you quoted was due to a syntactic ambiguity.
Scott

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Date: Thu, 19 Dec 1996 16:58:24 -0600
From: Bruce Abbott <abbott@CVAX.IPFW.INDIANA.EDU>
Subject: RunBlind.pas: New and improved

[From Bruce Abbott (961219.1800 EST)]

Given the confusion surrounding the model code in the previous version of RunBlind, I've taken Bill Power's suggestions and made the state of CanSee determine whether CV or CVm is used to determine the value of p, the controlled perception. This allows me to use CV and CVm as I had originally intended -- to stand for the controlled environmental variable and its model, respectively. The model behaves exactly as it did in the first version, but I think that it is much easier to understand from reading the code in StepModel just how the model works. Just replace this new version of RunBlind.pas for the old one and run.

In this revision, I've also changed the labeling of two of the traces. The top trace shows the state of the _controlled perception_ at all times. When running in sighted mode, this will be identical to CV, whereas when running in blind mode, it will be identical to CVm. The second trace shows the state of CV at all times, whether being directly controlled (sighted mode) or only changing as a side-effect of controlling CVm (blind mode). The third trace shows the controller's output, the fourth the actual disturbance to CV and the fifth the modeled disturbance waveform. Compare the excursions of the environmental variable (CV, red trace) to those of the actual disturbance (yellow trace) both in sighted and blind mode. Even in blind mode, the excursions are less than those seen in the disturbance, indicating some improvement owing to model-based control, although they are clearly greater than the those seen in sighted mode.

For those who may be running this program on a slower (or faster!) machine than mine, you can determine the speed at which the graphs are drawn by setting the constant in the line within the main procedure that currently reads:

```
  delay(10);
```

Larger numbers produce slower speeds. The new version uses the same GrUtils.pas unit distributed with the first version, so I have not included GrUtils here.

Regards,

Bruce

```
-------------------------------------------------------------------------------
program RunBlind;
{ Simulation to demonstrate a controller capable of continuing to control
  (to some degree) after its input has been lost.

  Compile in Turbo Pascal 7.0 or Borland Pascal 7.0 or above

  For simplicity, I have given the controller a perfect model of one of
  the disturbance waveforms, which it uses as a substitute for the real
  disturbance when running "blind." There is also an unmodeled disturbance
  waveform present, and the actual disturbance waveform is the sum of the
  modeled and unmodeled disturbance waveforms.

  The program begins with the controller running in "sighted" mode,
  controlling a perceptual variable that is identical to the environmental
  variable of which the perceptual variable is a function. By pressing
  the space bar, you can force the controller to run "blind," relying on
  its model of the disturbance to determine the state of the controlled
  perceptual variable. You will note that when the controller runs in the
  "blind" mode, it is no longer able to counter the unmodeled disturbance,
  but still does counter the modeled disturbance. Consequently, the
```

```
  environmental variable does not vary as much as it would in the absence
  of all controll.

  Written by Bruce Abbott
  Indiana - Purdue Fort Wayne
  (961218)
}

uses dos, crt, graph, grutils;

var
  ch: char;
  Stop, CanSee: boolean;
  p, r, e, g, o, s, CV, CVm, d, dMod, dUnmod, dt, t: real;
  MaxX, MaxY, MaxColor: integer;
  X, Y1, Y2, Y3, Y4, Y5: integer;

procedure InitScreen;
begin
  ClrScr;
  InitGraphics;
  MaxX := GetMaxX; MaxY := GetMaxY;
  MaxColor := GetMaxColor;
  OutTextXY(200, 10, 'RunBlind Controller Demo');
  SetColor(Yellow);
  OutTextXY(145, 30, 'Press Spacebar to toggle Sighted/Blind');
  SetColor(LightCyan);
  OutTextXY(10, 60, 'Controlled Perceptual Variable');
  SetColor(LightRed);
  OutTextXY(10, 75, 'Environmental Variable');
  SetColor(White);
  OutTextXY(10, MaxY - 90, 'Controller Output');
  SetColor(Yellow);
  OutTextXY(10, MaxY - 75, 'Actual Disturbance');
  SetColor(LightCyan);
  OutTextXY(10, MaxY - 60, 'Modeled Disturbance');
  SetColor(White);
end;

procedure InitModel;
begin
  CanSee := true;
  t := 0.0;
  dt := 0.1;
  s := 0.1;
  CV := 90.0;
  o := CV;
  r := 100.0;
  g := 100.0;
  dMod := 0.0;
  dUnmod := 0.0;
  d := dMod + dUnmod;
  CanSee := true;
end;

procedure StepModel;
begin
  if CanSee then p := CV else p := CVm;
  e := r - p;
```

```
  o := o + (g*e - o)*s*dt;
  dMod := 2.0*sin(t);
  dUnmod := 0.5*sin(3.0*t/2.0) + 0.5*sin(t/3.0) + 0.5*cos(t/5.0);
  d := dMod + dUnmod;
  CV := o + d;
  CVm := o + dMod;
  t := t + dt;
end;

procedure GraphVars;
begin
  X := X + round(dt*10.0);
  if X > 640 then
    begin
      X := 1;
      SetViewPort(1, 85, MaxX, MaxY-100, ClipOff);
      ClearViewPort;
      SetViewPort(1, 1, MaxX, MaxY, ClipOff);
    end;
  Y1 := MaxY - round(o*10.0) + 725;
  Y2 := MaxY - round(d*10.0) - 200;
  Y3 := MaxY - round(p*10) + 625;
  Y4 := MaxY - round(CV*10) + 660;
  Y5 := MaxY - round(dMod*10) - 150;
  putpixel(X, Y1, white);
  putpixel(X, Y2, yellow);
  putpixel(X, Y3, lightcyan);
  putpixel(X, Y4, lightred);
  putpixel(X, Y5, lightcyan);
end;

procedure ShowStatus;
begin
  SetViewport(250,MaxY-50, 350, MaxY, ClipOff);
  ClearViewPort;
  SetViewport(1, 1, MaxX, MaxY, ClipOff);
  if CanSee then OuttextXY(250, MaxY-25, 'Sighted')
    else OuttextXY(250, MaxY-25, 'Blind');
end;

begin
  TextMode(co80);
  ClrScr;
  InitScreen;
  Stop := false;
  InitModel;
  ShowStatus;
  repeat
    StepModel;
    GraphVars;
    delay(10);
    if keypressed then
      begin
        ch := readkey;
        Case ch of
          #27: Stop := true;
          ' ': If CanSee = true then
            begin
              CanSee := false;
```

```
              ShowStatus;
            end
            else
              begin
                CanSee := true;
                ShowStatus;
              end;
          end;
      end;
  until Stop;
end.
```

Date: Thu, 19 Dec 1996 17:52:31 EST
From: Bruce Gregory <gregory@CF4.HARVARD.EDU>
Subject: Re: In Defense of Popper & Harms

[From Bruce Gregory (961219.1750 EST)]

Scott Sterling 19 Dec 1996 17:31:47

> So I, and everyone else whether they realize it or not, make
> some basic assumptions about individual existence, and then about the
> the correspondence of logical thought to the perceptions the mind has.
> Bill Power's epistemology, along with that of Karl Popper as modified by
> Donald Campbell and explained by Gary Cziko, seems the best one I have
> ever heard. It requires the least reliance on absolutes certainties and
> irrefutable truths and all that stuff, of any epistemology I know. It
> remains open to adptation, and is a damn good working hypothesis.

Seems we agree on all important points. Epistemologists seem to
invariably imply that we somehow infer the existence of a real
world. Heideggar's early work (I have actually read much of
Being and Time. I say this not to brag, but simply to show
where desperation can drive one ;-)) provides what I find to
be a more satisfactory description of how the world "shows up"
for us, and it is not as an inference.

Bruce Gregory

Date: Thu, 19 Dec 1996 17:08:56 -0600
From: Bruce Abbott <abbott@CVAX.IPFW.INDIANA.EDU>
Subject: Re: Lies, damned lies, and programs

[From Bruce Abbott (961219.1810 EST)]

This came in _after_ I sent out my revision of RunBlind.pas:

>Bill Powers (961219.1422 MST) --

>It seems to me that the simplest way is to use dMod for the modeled
>disturbance and d for the real one. If dMod is different from d, then there
>will be a difference between CVm and CV. The unmodeled part of the real
>disturbance is then just dUnmod = d - dMod. dUnmod isn't another
>disturbance; it's just the discrepancy between the modeled and real

>disturbance, a calculation we make on a scratchpad somewhere else. It's not
>another variable in the model.

>Bruce Abbott, will you take care of getting the program straightened out so
>you don't confuse poor old Dad?

Already done, Dad. Another case of mind reading?

Regards,

Bruce

Date: Thu, 19 Dec 1996 15:30:09 -0800
From: Richard Marken <marken@AEROSPACE.AERO.ORG>
Subject: Re: Lies, damned lies, and programs

[From Rick Marken (961219.1530)]

Daddy Bill:

>Bruce Abbott, will you take care of getting the program straightened out so
>you don't confuse poor old Dad?

Baby Bruce:

>Already done, Dad. Another case of mind reading?

Oh, come on. That was model-based control if I ever saw it.

Best

Redeemable Rick (especially at this time of year;-))

Date: Thu, 19 Dec 1996 22:07:22 -0500
From: Scott Stirling <scstirli@ANSELM.EDU>
Subject: Re: false knowledge

On Wed, 18 Dec 1996, T. B. Harms wrote:

> *From Tracy Harms 1996;12,18.10:00
>
>
> Bowing to the noisy insistence of the eager mob, I here will post a bit
> more on that perverse creature, false knowledge. For the moment I do so by
> transcribing part of a paragraph written by Peter Munz in _Our Knowledge of
> the Growth of Knowledge_ (Routledge, 1985) p282:
>
>
>   With the emergence of consciousness, we get a further change in the
> nature of change. Conscious organisms can create falsehoods: they can lie
> and delude and deceive both themselves and others. Using deceits and
> falsehoods, they can construct artificial species which we call human
> societies and thus surround themselves with a wall of protection against
> the ravages of the environment. Hence, natural selection can be made to
> cease. In this way, cultures are created. The most elementary strategy

> used in the development of cultures is the artificial protection of
> knowledge from criticism. Certain pieces of knowledge, though obviously
> not all knowledge, are set aside and protected from critical appraisal.
> The thunder is identified with a god, the shadow of a man with his soul,
> and twins with cucumbers. Rational doubts are nipped in the bud by the
> mere absence of competing alternative proposals. Such protected knowledge
> can be used as a social bond. People who subscribe to it are members of a
> society; people who don't are outside that society. In this way, a lot of
> knowledge is syphoned off and used for non-cognitive purposes--that is, as
> a catechism. But such syphoning-off, though initially obviously
> counter-adaptive, is an oblique advantage. A society so constituted is
> larger than a group of people bonded by nothing but the web of kinship and
> is therefore capable of effective division of labour and co-operation.
> Thus, we get the astonishing spectacle of societies which cherish a
> mountain of false knowledge and which thrive for a long time not in spite
> of that false knowledge, but because of it.
>

T.B. seems to be a brilliant guy to me, so I am wondering what is so compelling to him about Munz' work here (?). It seems that Munz is merely using the term "knowledge" in an equivocal way. This excerpt reminds me of Richard Dawkins' essay _Viruses of the Mind_, where the term "virus" is used in a metaphorical/analogical way. Why couldn't Munz use the term "belief" or "hypothesis" here? I think the use of "false knowledge" misleads, where he really seems just to be talking about "sacred cows." I was expecting something much more earth-shattering than this!

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Date: Thu, 19 Dec 1996 20:56:56 -0700
From: Bill Powers <powers_w@FRONTIER.NET>
Subject: RunBlnd2.pas

[From Bill Powers (961219.2030 MST)]

I've taken Bruce Abbott's latest version of RunBlind and juggled things around a bit to show what's happening more clearly. I made dt smaller and raised the gain, and also raised the frequency of the disturbances. The main changes, however, are in the sizes of the disturbances and in the way the results are presented.

In the previous version we were looking at the variations in perception and CVs through a magnifying glass with no point of reference to tell us if we were looking at big effects or small effects. The peak value of the modeled disturbance was only 2 units out of a reference level of 100, and for the unmodeled disturbance, only 1.5 units. I raised the peak values so both disturbances have a maximum possible value of plus or minus 75 units, and I plotted all the variables on scales that run from 0 to 200 units.

Now we can see that the disturbances cause variations from the reference

level of roughly plus or minus 10 per cent max in the sighted condition, and plus or minus about 60 percent max in the blind condition. So even though half of the total disturbance is being cancelled in the blind condition, the errors as a percentage of the reference level are six times worse, in terms of the controlled variable. Of course the perceptual signal remains within about 10 percent of the reference signal in either condition, which is what we expect when the system is controlling an imagined signal. Control looks good to the system, but the external correlate of the perception is going all over the place.

Program follows.

Best,

Bill P.

```
------------------------------------------------------------------------
program runblnd2;
{ Simulation to demonstrate a controller capable of continuing to control
  (to some degree) after its input has been lost.

  Compile in Turbo Pascal 7.0 or Borland Pascal 7.0 or above

  For simplicity, I have given the controller a perfect model of one of
  the disturbance waveforms, which it uses as a substitute for the real
  disturbance when running "blind."  There is also an unmodeled disturbance
  waveform present, and the actual disturbance waveform is the sum of the
  modeled and unmodeled disturbance waveforms.

  The program begins with the controller running in "sighted" mode,
  controlling a perceptual variable that is identical to the environmental
  variable of which the perceptual variable is a function.  By pressing
  the space bar, you can force the controller to run "blind," relying on
  its model of the disturbance to determine the state of the controlled
  perceptual variable.  You will note that when the controller runs in the
  "blind" mode, it is no longer able to counter the unmodeled disturbance,
  but still does counter the modeled disturbance.  Consequently, the
  environmental variable does not vary as much as it would in the absence
  of all controll.

  Written by Bruce Abbott
  Indiana - Purdue Fort Wayne
  (961218)

   Modified by WTP, 961219
}

uses dos, crt, graph, grutils;

var
  ch: char;
  Stop, CanSee: boolean;
  p, r, e, g, o, s, CV, CVm, d, dMod, dUnmod, dt, t: real;
  MaxX, MaxY, MaxColor: integer;
  X, Y1, Y2, Y3, Y4, Y5: integer;

procedure InitScreen;
begin
  OutTextXY(200, 10, 'RunBlnd2 Controller Demo');
  SetColor(Yellow);
```

```
  OutTextXY(145, 30, 'Press Spacebar to toggle Sighted/Blind');
  SetColor(LightCyan);
  OutTextXY(10, 60, 'Controlled Perceptual Variable');
  SetColor(LightRed);
  OutTextXY(10, 75, 'Environmental Variable');
  SetColor(White);
  OutTextXY(10,90,'Reference level');
  SetColor(White);
  OutTextXY(350, 60, 'Controller Output');
  SetColor(Lightred);
  OutTextXY(350, 75, 'Actual Disturbance');
  SetColor(LightGray);
  OutTextXY(350, 90, 'Modeled Disturbance');
  SetColor(White);
  line(50,maxy - 100,320, Maxy - 100);
  line(370,maxy - 100,639, Maxy - 100);
  line(50,maxy - 100,50, Maxy - 300);
  line(370,maxy - 100,370, Maxy - 300);
  outtextxy(32,maxy - 104,'0');
  outtextxy(354,maxy - 104,'0');
  outtextxy(16,maxy - 204,'100');
  outtextxy(338,maxy - 204,'100');
  outtextxy(16,maxy - 304,'200');
  outtextxy(338,maxy - 304,'200');
end;

procedure InitModel;
begin
  CanSee := true;
  t := 0.0;
  dt := 0.01;
  s := 0.1;
  CV := 90.0;
  o := CV;
  r := 100.0;
  g := 900.0;
  dMod := 0.0;
  dUnmod := 0.0;
  d := dMod + dUnmod;
  CanSee := true;
  X := 51;
end;

procedure StepModel;
begin
  if CanSee then p := CV else p := CVm;
  e := r - p;
  o := o + (g*e - o)*s*dt;
  dMod := 75.0*sin(4.9*t);
  dUnmod := 25*sin(4.4*t) + 25*sin(5.4*t) + 25*cos(6.3*t);
  d := dMod + dUnmod;
  CV := o + d;
  CVm := o + dMod;
  t := t + dt;
end;

procedure ShowStatus;
begin
  bar(250,maxy-25,310,maxy - 15);
```

```
  if CanSee then OuttextXY(250, MaxY-25, 'Sighted')
    else OuttextXY(250, MaxY-25, 'Blind');
end;

procedure GraphVars;
begin
  if X = 300 then
    begin
      X := 51;
      clearviewport;
      initscreen;
      showstatus;
    end;
  Y1 := MaxY - round(o) - 100;
  Y2 := MaxY - round(d) - 100;
  Y3 := MaxY - round(p) - 100;
  Y4 := MaxY - round(CV) - 100;
  Y5 := MaxY - round(dMod) - 100;
  putpixel(X,Maxy - 100 - round(r),White);
  putpixel(X + 320, Y1, white);
  putpixel(X + 320, Y2, lightred);
  putpixel(X, Y3, lightcyan);
  putpixel(X, Y4, lightred);
  putpixel(X + 320, Y5, blue);
  inc(X);
end;


begin
  TextMode(co80);
  ClrScr;
  InitGraphics;
  MaxX := GetMaxX; MaxY := GetMaxY;
  MaxColor := GetMaxColor;
  Stop := false;
  InitModel;
  setfillstyle(0,0);
  ShowStatus;
  initscreen;
  repeat
    StepModel;
    GraphVars;
    delay(10);
    if keypressed then
      begin
        ch := readkey;
        Case ch of
          #27: Stop := true;
          ' ': If CanSee = true then
            begin
              CanSee := false;
              ShowStatus;
            end
            else
              begin
                CanSee := true;
                ShowStatus;
              end;
          end;
```

```
      end;
  until Stop;
end.
```

Date: Thu, 19 Dec 1996 21:33:23 +0000
From: Richard marken <marken@AERO.ORG>
Subject: Re: RunBlind.pas: New and improved

[From Rick Marken (961219.2030)]

Bruce Abbott (961219.1800 EST) --

```
> procedure StepModel;
> begin
>   if CanSee then p := CV else p := CVm;
>   e := r - p;
>   o := o + (g*e - o)*s*dt;
>   dMod := 2.0*sin(t);
>   dUnmod := 0.5*sin(3.0*t/2.0) + 0.5*sin(t/3.0) + 0.5*cos(t/5.0);
>   d := dMod + dUnmod;
>   CV := o + d;
>   CVm := o + dMod;
>   t := t + dt;
> end;
```

This is very nice.

Now notice what happens in the "blind" case when dMod is set to zero. The variance in CV is now exactly equal to that of the disturbance. So the apparent control we see when dMod := 2.0*sin(t) (the fact that the variance of the CV is less than the variance of d) is completely due to the fact that the control system is computing output that is exactly equal to -dMod. The "improved control" when the blinded system controls CVm is a result of the fact that the system has an exact model of a component of the disturbance variance.

So this computed output model "works" (appears to control) to the extent that the basis for computation of output (dMod in this case) accurately represents the disturbance to be opposed by output.
When dMod accurately represents a portion of the variance in the disturbance, the computed output accurately opposes that component.
If dMod accurately represents all of the variance in the disturbance (as it will if d := dMod + dUnmod is changed to d := dMod) then the computed output will accurately oppose _all_ the variance in the disturbance; "control" in the blind will be perfect.

So this computed output model will appear to be able to control in the blind to the degree that dMod = d. Now the question is "Can people develop an accurate model of the net disturbance to a controlled variable"? Bill Powers suggested an experiment that would be a start at answering this question. Why don't we do that experiment next?

Best

Rick

Date: Thu, 19 Dec 1996 23:38:14 -0500
From: Scott Stirling <scstirli@ANSELM.EDU>
Subject: ShuffleBrain

Hello there,

Have you all heard of Paul Pietsch and his experiments with salamanders and the holographic theory of mind? I just read several papers of his at his relatively new website, but the papers are all at least ten years old. The website rates as one of the Top 5%, if that makes anyone want to rush out and check it out. Anyway, his papers are beautifully written, and they are definitely worth a look. This is the first time I've been impressed with the holographic theory of mind--this guy actually did research that supports it. Any of you lil' ol' engineers or anyone else who knows more about this than me are invited to enlighten me.

Scott

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http://www.anselm.edu/student/scstirli/welcome.html

Date: Thu, 19 Dec 1996 23:42:01 -0500
From: Scott Stirling <scstirli@ANSELM.EDU>
Subject: URL for ShuffleBrain

Hey, how about I give you a URL for that page I just gushed over? Sorry, that slipped my hologram...

Here's the main page. The main article on Punky is probably the best over all: http://www.indiana.edu/~pietsch/home.html

and this paper is real good too, especially the comments at the end: http://www.indiana.edu/~pietsch/swapping.html

Scott

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http://www.anselm.edu/student/scstirli/welcome.html

Date: Thu, 19 Dec 1996 19:13:48 PDT
From: CCHERPAS <CCHERPAS@CCCPP.COM>

Subject: Executive VP of Product Development position

Y'all,

Does anybody out there have expertise in PCT, experience in planning and managing _big_ projects, and who wants a 6-figure salary, and wants to save education from the mediocracy?

Computer Curriculum Corporation (CCC), where I am a research scientist, is going to be looking for someone to be our Executive VP of Product Development, starting tomarrow.

Correct me if I am wrong, but could there be a better lab for studying the development of hierarchical control systems than in a computer-based environment which models each individual student over the course of years, and performing tasks which are explicitly _intended_ to lead to increasingly sophisticated hierarchical control of perceptions?

Gary Cziko and Hugh Petrie have specifically cited the need for formulating educational objectives in terms of controlling perceptions. Let's construct CCC's computer-based exercises to incorporate The Test, and, in case you haven't read my previous posts, we can continuously refine the control system already embedded in the CCC software today, which dynamically/individually structures each student's educational process. I'm working on these goals now, but I need help. I need brains. I need guts. I need a PCTer.

Again, this is not a place where you would have to start from scratch. We've been implementing control systems for individualizing educational processes for a long time. There's a good based to build on. Over 1.5 million students (got that?) are currently generating data.

Let's teach PCT to kids -- it's actually one of the core elements missing in education today, wouldn't you agree? Kids could do "psychophysical experiments" (perceptual control) and learn about their own perceptual competencies and potential, while _we_ learn how to teach them better and better from the data.

Please respond asap as we are moving swiftly. Send me email at ccherpas@cccpp.com and let me know if you are interested or know someone. Hugh Petrie, where's your school spirit? Bill, Rick, everybody, let's give PCT a chance in an area that could really turn heads and do some good too.

Best regards,
cc
(408)541-3303

Date: Thu, 19 Dec 1996 23:53:08 -0800
From: David Wolsk <dow@PINC.COM>
Subject: categorization repeated

191296 23:25 David Wolsk wrote:

my last post got emasculated somehow. Is that what happens to us Jews at Christmas time? I suspect I have to accept the blame myself.

Hans Blom 18/12/96
2. If we believe we can detect "consistency over time", it seems to me that we have some inner mechanism that allows this detection. Since this detection is primarily some type of comparison between what occurs now and what occurred previously, it appears that we have some type of inner "store" where previous occurrences are somehow saved, possibly encoded and/or compacted. It also appears that, due to brain size limitations, it is impossible to store the raw data of _all_ previous occurrences in their entirety. So what is stored? Only occurrences that are recognized as "lawful"? But initially there are none. Are occurrences that are similar to a previous one somehow discarded? But how to establish what is similar right from the start?

DW
What I feel goes on is that a good portion of cortical functioning depends on our
using categorization as the basic memory storehouse. First order perceptions get an immediate tag, an identification. Anxiety is the outcome whenever I am unable to label what I just saw or heard or tasted. Thus, I picture this inner "store" of yours as a personal "dictionary". A car passes me on the road: I see a blue sedan, a few years old. Someone else sees a1991 Chevy Caprice. I tend to link a whole set of these "simplified" categories" into a stored and recallable experience. My wife, Ingrid, links a set of highly detailed labels that have the appearancxe of a photographic memory .... and that for many events of many years ago. Yet, in my attempts to see what she actually sees, it all seems linked by a set or sequence of actions.

I used to teach a curriculum developed by Reuven Feuerstein called Instrumental Enrichment (a terrible label). It developed out of his research with children after WWII who all scored very low on IQ tests. The curriculum works towards developing the basic prerequisites of intelligent thought. The earliest needs are to improve notions and systems of categorization. Somehow, all this is connected in my own mind. Probably seems mindless to many others ..... with their own categorization systems. I guess that's why Bill is always hoping we can link back to experimental data-based discussions.

David Wolsk
Victoria, BC Canada

End of CSGNET Digest - 19 Dec 1996 to 20 Dec 1996
****

Date: Sat, 21 Dec 1996 08:05:08 +1800
Subject: CSGNET Digest - 20 Dec 1996 to 21 Dec 1996

There are 29 messages totalling 1832 lines in this issue.

Topics of the day:

1. URL for ShuffleBrain
2. Lies, damned lies and non-control (17)
3. RunBlnd3.pas
4. Controlled environmental variable?
5. Sibling rivalry; solving the "loss of input" problem
6. Environmental variables are not controlled (2)

7. reply to Martin: combining goals
8. Executive VP position
9. DIRECT Re: Lies, damned lies and non-control
10. HPCT vs Simulation-based control
11. How imagination helps control (2)

Date: Fri, 20 Dec 1996 09:03:56 -0700
From: Bill Powers <powers_w@FRONTIER.NET>
Subject: Re: URL for ShuffleBrain

[From Bill Powers (961220.0900 MST)]

Scott Stirling (961219) --
>Hey, how about I give you a URL for that page I just gushed >over? Sorry, that slipped my hologram...

OK. I've had a brief look at it. One question that crosses my mind is "Why does the world look the way it does?" In other words, what's _reading_ these holograms? Obviously the world doesn't look like overlapping wave patterns. Pietsch may answer this question somewhere in the book, but I haven't read it all (on line!).

I think Pietsch has a tendency to exaggerate his findings. In the intro, he says that a grafted salamander brain is "psychologically normal." How did he determine THAT?

I'm put off by the hard-sell language.

Best,

Bill P.

Date: Fri, 20 Dec 1996 10:51:31 -0600
From: Bruce Abbott <abbott@CVAX.IPFW.INDIANA.EDU>
Subject: Re: Lies, damned lies and non-control

[From Bruce Abbott (961220.1150 EST)]

>Rick Marken (961219.1430) --

>Bruce Abbott (961219.1620 EST)

>>Why don't you just take a look at the program's output? It is _already_
>>operating with dUnmod a _fairly significant_ component of the variance in d.

>I will compute this tonight when I have access to the program but I
>think you will find that what is happening during the "blind" run is that
>the dMod component of the disturbance is being completely opposed by o
>while the dUnmod component goes completely unopposed. Since you have assured
>yourself of a perfect model of the dMod component of the disturbance, it is
>no surprise that this component of the disturbance is opposed by o. When I
>say that there is _no control_ during the blind period I mean that there is
>no opposition to the disturbance (dUnmod) that you have not had the courtesy
>to tell the system about in the first place;-)

You keep changing your story. Before you said that there would be _no_ opposition to the disturbance; now you want us to believe that you _meant_ that there would be no opposition to the _unmodeled portion_ of the disturbance! There's a huge difference between those two positions; if you "really" meant the latter, why didn't you make that clear in the first place?

Of course there will be no opposition to the unmodeled portion of the disturbance. I have not claimed otherwise. What I _have_ claimed is that the error between the CV and its reference will be _reduced_ to the extent that the controller is able to successfully model the predictable component of the disturbance. Logic says that this will be true, and RunBlind demonstrates that this analysis is correct.

>>Also, I have also noted for the record that CVm only _appears_ to be
>>controlled (to the extent that the model matches), so this is not a point of
>>disagreement between us.

>Sorry. I guess I was misled by your continued claims that your model is
>"closed loop" even in "blind" mode.

(Now that we've changed the variable names and computations as Bill suggested, the CVm in the above-quoted sentence should be changed to CV.) As to my claim, what I have claimed is that the _controller_ continues to operate closed loop with respect to its own controlled variable. When running sighted, this is CV; when running blind, this is CVm. Because the loop is always closed, there is no change of gain etc. that would occur if the system had to operate without feedback. If you have been misled, it has been by your own misconceptions, not by what I have stated. If you paid closer attention instead of jumping to confusions, we could avoid these problems.

+>Tracy Harms:

+>I doubt there is any dispute that control deteriorates [in model-based
+>control].

>I dispute it. Control doesn't deteriorate; it disappears. There is _no_
>control when you are generating output based on a model. There is _only_
>the appearance of control (if you are lucky).

Rick, Rick, Rick. There is only the _appearance_ of control of the CV when you are doing the standard tracking task used to illustrate a perceptual control system. That appearance occurs because of the "lucky" relationship between the cursor's position and the participant's perception of the cursor's position (i.e., neural current p varies systematically with the cursor's position). The _cursor_ (the thing out there on the screen) isn't really controlled; what is controlled is the _perception_ of the cursor. In model-based control, the relationship between the perception and the CV will be identical to that found in the normal PCT system if the model is perfect, and the _appearance_ of control will be the same as in the normal PCT system. THERE IS NO DIFFERENCE -- either way, control of the CV is only an appearance. As the accuracy of the model (in model-based control) deterioriates, the appearance of control deteriorates with it. But let's not pretend that the two systems differ in that PCT systems "really" control the CV and model-based systems only "appear" to control the CV. That's nonsense.

+>The question seems to be the range over which models can aid control
+>despite loss of relevant input.

>Not really. Model based control can only "work" (give the appearance of
>control) in a world with no disturbances or with disturbances that are
>highly predictable. That isn't this world.

This world is _full_ of highly predictable disturbances, Rick. Example: Even in California, some of the trees there shed their leaves in the fall, preparing for the coming freeze. (I know because I grew up there.) These systems are acting to oppose a disturbance that only correlates with the season, by using the length of daylight as a surrogate variable. I think the problem with the perception that disturbances are highly unpredictable is that one tends to focus on the lowest-level systems, for which disturbances are frequent and largely unpredictable. But there are lower-frequency systems whose CVs tend to behave rather predictably, and for these systems, model-based control might be an option. In the winter I always seem to be putting on my coat _before_ my skin begins to chill from exposure to the frigid air, not after. Why do I do that if not for a model I have of the temperature to expect outside?

> So
>the engineers are willing to give up control of a variable and use non-
>control systems (model- based "control" systems) to stabilize the variable as
>best as they can in environments that the engineers themselves design
>(control) to be as disturbance free (or as predictable) as possible.
>For example, they can use model-based control to control a chemical reaction
>that occurs in an enviroment that is well protected (via thick walls,
>etc) from the effects of variations in temperature, pressure, etc.
>Evolution didn't have that design option with living systems.

What about the oysters, Rick? And the trees? And . . .

Regards,

Bruce

Date: Fri, 20 Dec 1996 09:53:29 -0700
From: Bill Powers <powers_w@FRONTIER.NET>
Subject: RunBlnd3.pas

[From Bill Powers (961220.0930 MST)]

Included below is RunBlnd3.pas, which adds an illustration of what I mean by a "big" effect versus a "small" effect.

Let's postulate that the maximum range of the perceptual signal is around 200 units -- any bigger amount would start getting painful. So the reference signal is most likely to be found somewhere in the range of 0 to 200 units. We can also say that this is the useful range of variation of the controlled variable, CV.

A natural way of judging the quality of control is to compare the effects of disturbances with the natural range of the reference signal. In this version of Bruce's program, the reference signal starts at 100. After 80 units of time it jumps to 175 units, where it remains until t = 120. Then it drops to 50 units until t = 220, after which it returns to 100.0 units for the rest of the scan.

In the sighted condition you can see that the control system is actually quite fast (I've slowed it down a little so you can see the transitions). The large disturbances cause a clearly visible deviation of the controlled variable from the reference signal, but you can also see that in comparison with the changes in the reference signal, the effects of the disturbance are kept pretty small. The disturbance alone, if unopposed, would cause variations in the controlled quantity of plus or minus 150 units relative to the average value of zero. The actual range is only about plus or minus 15-20 units relative to the reference value. Obviously the reference signal is having a much more determining effect on the CV than the disturbance is having.

The error that does occur is due primarily to the dynamics of the control system. The leaky integrator can't quite follow the continual variations in the disturbances. The loop gain in the frequency band where the disturbance variations occur is only about 10, although for much slower variations it would approach 900 (the steady-state gain I've given the system).

In the blind condition we can now see how the controlled variable behaves as the reference signal changes. If the object of this control process is to make the controlled variable follow the variations in reference signal, the "control" is pretty poor. Of course the _imagined_ controlled variable is controlled equally well in either condition -- but controlling what you imagine to be happening isn't very useful in terms of the actual effects of the environment on you.

Program follows.

Best,

Bill P.

```
============================================================================
program runblnd3;
{ Simulation to demonstrate a controller capable of continuing to control
  (to some degree) after its input has been lost.

  Compile in Turbo Pascal 7.0 or Borland Pascal 7.0 or above

  For simplicity, I have given the controller a perfect model of one of
  the disturbance waveforms, which it uses as a substitute for the real
  disturbance when running "blind."  There is also an unmodeled disturbance
  waveform present, and the actual disturbance waveform is the sum of the
  modeled and unmodeled disturbance waveforms.

  The program begins with the controller running in "sighted" mode,
  controlling a perceptual variable that is identical to the environmental
  variable of which the perceptual variable is a function.  By pressing
  the space bar, you can force the controller to run "blind," relying on
  its model of the disturbance to determine the state of the controlled
  perceptual variable.  You will note that when the controller runs in the
  "blind" mode, it is no longer able to counter the unmodeled disturbance,
  but still does counter the modeled disturbance.  Consequently, the
  environmental variable does not vary as much as it would in the absence
  of all controll.

  Written by Bruce Abbott
  Indiana - Purdue Fort Wayne
  (961218)
```

```
   Modified by WTP, 961219

   Ref signal variations added by WTP 961220
}

uses dos, crt, graph, grutils;

var
  ch: char;
  Stop, CanSee: boolean;
  p, r, e, g, o, s, CV, CVm, d, dMod, dUnmod, dt, t: real;
  MaxX, MaxY, MaxColor: integer;
  X, Y1, Y2, Y3, Y4, Y5: integer;

procedure InitScreen;
begin
  OutTextXY(200, 10, 'RunBlnd2 Controller Demo');
  SetColor(Yellow);
  OutTextXY(145, 30, 'Press Spacebar to toggle Sighted/Blind');
  SetColor(LightCyan);
  OutTextXY(10, 60, 'Controlled Perceptual Variable');
  SetColor(LightRed);
  OutTextXY(10, 75, 'Environmental Variable');
  SetColor(White);
  OutTextXY(10,90,'Reference level');
  SetColor(White);
  OutTextXY(350, 60, 'Controller Output');
  SetColor(Lightred);
  OutTextXY(350, 75, 'Actual Disturbance');
  SetColor(LightGray);
  OutTextXY(350, 90, 'Modeled Disturbance');
  SetColor(White);
  line(50,maxy - 100,320, Maxy - 100);
  line(370,maxy - 100,639, Maxy - 100);
  line(50,maxy - 100,50, Maxy - 300);
  line(370,maxy - 100,370, Maxy - 300);
  outtextxy(32,maxy - 104,'0');
  outtextxy(354,maxy - 104,'0');
  outtextxy(16,maxy - 204,'100');
  outtextxy(338,maxy - 204,'100');
  outtextxy(16,maxy - 304,'200');
  outtextxy(338,maxy - 304,'200');
end;

procedure InitModel;
begin
  CanSee := true;
  t := 0.0;
  dt := 0.01;
  s := 0.07;
  CV := 90.0;
  o := CV;
  r := 100.0;
  g := 900.0;
  dMod := 0.0;
  dUnmod := 0.0;
  d := dMod + dUnmod;
  CanSee := true;
  X := 51;
```

```
end;

procedure StepModel;
begin
  if CanSee then p := CV else p := CVm;
  e := r - p;
  o := o + (g*e - o)*s*dt;
  dMod := 75.0*sin(4.9*t);
  dUnmod := 25*sin(4.4*t) + 25*sin(5.4*t) + 25*cos(6.3*t);
  d := dMod + dUnmod;
  CV := o + d;
  CVm := o + dMod;
  t := t + dt;
end;

procedure ShowStatus;
begin
  bar(250,maxy-25,310,maxy - 15);
  if CanSee then OuttextXY(250, MaxY-25, 'Sighted')
    else OuttextXY(250, MaxY-25, 'Blind');
end;

procedure GraphVars;
begin
  if X = 300 then
    begin
      X := 51;
      clearviewport;
      initscreen;
      showstatus;
    end;
  Y1 := MaxY - round(o) - 100;
  Y2 := MaxY - round(d) - 100;
  Y3 := MaxY - round(p) - 100;
  Y4 := MaxY - round(CV) - 100;
  Y5 := MaxY - round(dMod) - 100;
  putpixel(X,Maxy - 100 - round(r),White);
  putpixel(X + 320, Y1, white);
  putpixel(X + 320, Y2, lightred);
  putpixel(X, Y3, lightcyan);
  putpixel(X, Y4, lightred);
  putpixel(X + 320, Y5, blue);
  inc(X);
end;


begin
  TextMode(co80);
  ClrScr;
  InitGraphics;
  MaxX := GetMaxX; MaxY := GetMaxY;
  MaxColor := GetMaxColor;
  Stop := false;
  InitModel;
  setfillstyle(0,0);
  ShowStatus;
  initscreen;
  repeat
    if X < 80 then r := 100.0 else
```

```
    if X < 120 then r := 175.0 else
    if X < 220 then r := 50.0 else
    r := 100.0;
    StepModel;
    GraphVars;
    delay(10);
    if keypressed then
      begin
        ch := readkey;
        Case ch of
          #27: Stop := true;
          ' ': If CanSee = true then
            begin
              CanSee := false;
              ShowStatus;
            end
            else
              begin
                CanSee := true;
                ShowStatus;
              end;
          end;
      end;
  until Stop;
end.
```

Date: Fri, 20 Dec 1996 12:05:35 -0500
From: Martin Taylor <mmt@DCIEM.DND.CA>
Subject: Controlled environmental variable?

[Martin Taylor 961220 11:30]
> Tracy Harms 1996;12,19.10:30

> What is an
> "environmental variable to be controlled"?

It's a commonly used shorthand term. But in the context of Bruce's model it's something more than that.

> As I understand it only
> _perception_ is controlled, not behavior, and _not_ _environment_. Then
> again, I'm fresh to programmatic representations of PCT, so you may have
> something here which is perfectly fine, it just hasn't been adequately
> explained for me.

One has to ask _why_ we use this common shorthand. Why do we say "I moved the cursor" rather than "I moved my perception of the position of the cursor" when the latter is all we _can_ do.

The answer is in the dual aphorism: All we can control is perception, but it's the world that hits us.

In other words (assuming that some "real world" exists), what we perceive is all we can know of the outer world, but what we don't perceive can kill us. To survive, we have to affect the _real world_ in ways that are to the benefit of our (unperceived) intrinsic variables. We can only do this by acting on the real world so that our _perceptions_ stay near their reference

values.

And since we can't perceive the current or the appropriate values of our intrinsic variables, why is it of any use at all to control those other perceptual correlates of states and events in the real world? The perceptions we control are not the variables that really matter, and nor do the "real-world" variables matter that happen to be stabilized as a consequence (the critical consequence) of stabilizing the perceptual variables. At least, they don't matter in themselves.

Why is it of any use to control these perceptions that are apparently irrelevant to the intrinsic variables? Because of some stable associations (contingencies, correlations--call it what you will) that exist in the real world, whether we observe them or not. The contingencies may have been the same for the lifetime of the universe, or they may have been set up by an experimenter or by a grocer who says "give me some money and I'll give you some food." It so happens that we or our ancestors over the last 4*10^9 years have survived when we controlled certain perceptual variables. We and they survived because controlling those variables had the _real world_ consequence that our and their intrinsic variables stayed near their survival values (which is not a tautology, but an assertion that the world has some consistency over at least one life-span, and probably at least one planet's life-span).

We call the processes that allow us to control the intrinsic variables (for that's what is happening) "reorganization" when it occurs within an individual organism, and "evolution" when it happens within the gene structure that passes down a family tree.

So...the REALLY important stabilization is of the intrinsic variables. This stabilization happens because of actions that stabilize related aspects of the (unknowable) "real world." And the best we can do to affect the important aspects of the real world is to control perceptions that are of variables in some way associated (con...,corr...) with the unknown really important aspects of the real world.

Hence PCT. Perceptual Control Theory. Perception is what we "really" control. When we do that, something in the world that corresponds to the perceptual variable is, not entirely by happenstance, also stabilized (and we say, for short, that the external variable is "controlled."). And, if we are controlling for "good" values of the "right" perceptual variables, those "somethings" that are stabilized in the real world have real-world effects that keep our intrinsic variables where they must be.

And if we are not controlling for "good" values of the "right" perceptual variables, we may well die if we don't reorganize effectively first. Reorganization is the action mechanism of the control loop that keeps the intrinsic (real-world) variables controlled.

Go back to the "side-effects" discussion of a week or two ago, and in particular to Bill Powers' masterly summing-up. The effects on the world that are part of the control loop for the intrinsic variables are pure side-effects of our perceptual control actions, but the reverse is not true. The perceptual control actions are in the loop of control of the intrinsic variables.

A long time ago, I coined the term "Complex Environmental Variable" to refer to any function of measurable of perceivable aspects of the world. Some people have taken "CEV" to mean "controlled environmental variable",

to mark the function of sensor inputs defined by the Perceptual Input Function (PIF) of some control system. Those special CEVs, which I'd prefer to call CCEVs, are stabilized as a part of the action of the control of the associated perception. And their stabilization is what is important in the feeback path of intrinsic variable control.

It's quite fair to use the shorthand "environmental variable to be controlled", and useful also, inasmuch as it's the only thing about a control loop that is in principle observable by an outside analyst. But one should always be aware both that it _is_ a shorthand, and why the shorthand is acceptable.

Martin

Date: Fri, 20 Dec 1996 12:41:26 EST
From: Bruce Gregory <gregory@CF4.HARVARD.EDU>
Subject: Re: Lies, damned lies and non-control

[From Bruce Gregory (961220.1240 EST)]

Bruce Abbott (961220.1150 EST)

> This world is _full_ of highly predictable disturbances, Rick. Example:
> Even in California, some of the trees there shed their leaves in the fall,
> preparing for the coming freeze. (I know because I grew up there.) These
> systems are acting to oppose a disturbance that only correlates with the
> season, by using the length of daylight as a surrogate variable. I think
> the problem with the perception that disturbances are highly unpredictable
> is that one tends to focus on the lowest-level systems, for which
> disturbances are frequent and largely unpredictable. But there are
> lower-frequency systems whose CVs tend to behave rather predictably, and for
> these systems, model-based control might be an option. In the winter I
> always seem to be putting on my coat _before_ my skin begins to chill from
> exposure to the frigid air, not after. Why do I do that if not for a model
> I have of the temperature to expect outside?

To this country boy, there appears to be an ambiguity in the use of the the word "control" in this exchange. I-90 is wide and has many straight stretches. Despite this, I would predict disaster if I were close my eyes for thirty seconds while driving at 65 mph even if there were no other cars on the road. I would also be very reluctant to say that I was controlling the car while my eyes were closed. Praying would seem more descriptive than controlling ;-)

It seems to me that you put your coat on before you go out because you have a reference state for not going outdoors in the winter without a coat. You can certainly call this reference state a model of the world, but it doesn't seem to me that what you are doing involves this model except as a reference level. But as I say, I'm just a country boy and therefore several steps below even a simple engineer.

Bruce Gregory

Date: Fri, 20 Dec 1996 13:23:38 -0500
From: Martin Taylor <mmt@DCIEM.DND.CA>
Subject: Re: Lies, damned lies and non-control

[Martin Taylor 961220 13:10
> Rick Marken (961219.1430)]
>

> So
> the engineers are willing to give up control of a variable and use non-
> control systems (model- based "control" systems) to stabilize the variable as
> best as they can in environments that the engineers themselves design
> (control) to be as disturbance free (or as predictable) as possible.
> For example, they can use model-based control to control a chemical reaction
> that occurs in an enviroment that is well protected (via thick walls,
> etc) from the effects of variations in temperature, pressure, etc.
> Evolution didn't have that design option with living systems.
>
One might, if one felt mischievous, enquire as to the effect of turtle shells, and whether they just might possibly alter the influence of variations in the external environment on the turtle's intrinsic variables.

As to whether model-based systems "control" or not...If the term "control" means to maintain a variable near some reference value despite influences that would otherwise make its value deviate substantially from that value, then of course there is no such thing as model-based or open-loop "control."

But if one forgets about the word, and asks whether model-based systems can sustain a real-world variable in a condition satisfactory for some other purpose (e.g. maintenance of intrinsic variables near _their_ reference values), then model-based systems may very well be useful, even if they don't control. If the environment happens to be reasonably predictable, and the models are good, real control is only required as a kind of monitoring, to see whether the model is still keeping the variable on track.

Engineered "control" systems don't care whether an internal perception is controlled. They "care" whether the job the designer wants done is done...in other words, the designer and the user may be control systems, when all the engineered system does is to keep something in the outer world stable for them, with or without "control". (As evolution did for us by providing skeletons, without which the muscular control systems would have a difficult time keeping our bodies within a small vertically oriented space for very long:-)

Think more about what control is doing, and less about what it _is_ in the dictionary sense. That ought to reduce the intensity of many arguments.

Martin

Date: Fri, 20 Dec 1996 10:37:15 -0800
From: Richard Marken <marken@AEROSPACE.AERO.ORG>
Subject: Re: Lies, damned lies and non-control

[From Rick Marken (961220.1030)]

Bruce Abbott (961220.1150 EST) --

>You keep changing your story. Before you said that there would be _no_
>opposition to the disturbance;

No. I said there would be no _control_ in the "blind". I should probably call the outputs that cancel the dMod portion of the disturbance "coincidental opposition" rather than "opposition". There is no control going on because the outputs that are "coincidentally" opposing dMod during the blind portion of the run (giving the appearance that the CV is being controlled) are not actively generated as part of a control loop of which CV is a part.

This is a subtle point but I think it is important. We are getting to the basic difference between a "generated output" and a "closed loop" view of control. Your model is able to generate outputs (o) in the blind condition which, when added to the real disturbance, oppose the disturbance and keep the CV near the reference value. If dMod=d your model will do this perfectly; it will generate outputs such that o + d = CVr, where CVr is the reference state of the CV. It _looks like_ the CV is controlled. But it is _not_. The CV is only _coincidentally_ kept in the reference state (CVr). As soon as the disturbance changes so that d<>dMod, this coincidence will be revealed. The CV was _never_ really under control in the "blind" situation. If the CV were under control, any change in the disturbance (within the limits of the system's ability to oppose such changes) would be opposed by system output and the CV would be maintained at the reference state (CVr).

Me:

>Control doesn't deteriorate [in model based "control']; it disappears.

You:

>Rick, Rick, Rick. There is only the _appearance_ of control of the CV
>when you are doing the standard tracking task used to illustrate a
>perceptual control system. That appearance occurs because of the "lucky"
>relationship between the cursor's position and the participant's
>perception of the cursor's position (i.e., neural current p varies
>systematically with the cursor's position). The _cursor_ (the thing out
>there on the screen) isn't really controlled; what is controlled is the
>_perception_ of the cursor. In model-based control, the relationship
>between the perception and the CV will be identical to that found in the
>normal PCT system if the model is perfect, and the _appearance_ of control
>will be the same as in the normal PCT system. THERE IS NO DIFFERENCE --
>either way, control of the CV is only an appearance.

I think you will have a real _feel_ for how control actually works when you understand why most of what you said here is wrong. I can't _make_ you understand it; but I hope I can provide the opportunity for your understanding. If you are willing to consider this issue carefully you may be able to finally make the shift from a cause-effect to a control view of life. Here's what I ask you to consider: The simultaneous equations that describe the behavior of your model (when blind) are:

p = o+dMod = CVm
o = (r-p)

and for a control model they are:

p = o+d = CV
o = (r-p)

The only difference is that the controlled variable (CV) is _not_ in the loop in your model; it _is_ in the loop in a control model. Think about that difference for a while; imagine the difference between what happens when we add o to d to get CV in the top (blind) model and what happens when we add o to d to get CV in the bottom (control) mode. It is a VERY different situation.

While it is technically true that p is the variable that is controlled by both models, the input (CVm or CV) that corresponds to p is (for all intents and purposes) a controlled variable; it is protected from the effects of disturbance. The only way to "detach" p from the input, so that the controlled value of p does not correspond to the controlled input (CVm or CV) is by directly disturbing p. But when this is true, The Test (using disturbances applied to CV) would show that CV is _not_ a controlled variable.

I think it's reasonable to assume that in the intact, relatively drug free organism, variations in p are typically a pretty good reflection of variations in the input (CVm or CV) so that the controlled input to a perceptual function (CV or CVm) can be considered the controlled variable. When your model is "blind" it controls CVm, _not_ CV. So the apparent control of CV produced by this "blind" controller is a completely accidental side effect of its control of CVm; the model is _not_ controlling CV (it doesn't act to protect CV from disturbances). When the control model sees (and controls) CV, it is _actively_ generating opposition to disturbances to a controlled variable (CV); the model _is_ controlling CV because it acts to protect CV from disturbance.

I feel like I may not be getting this accross. The difference between what is happening when a system adds computed outputs to a CV versus what is happening when it generates these outputs in a closed loop of which CV is a part is more than just equations to me; it is a palpable _feeling_. I can _feel_ (in imagination) the difference between computed output "coincidental control" and the real thing. Help me Bill Powers. How can I communicate this difference in words?

>This world is _full_ of highly predictable disturbances, Rick.

Well, let's do Bill's suggested experiment (tracking with a predictable disturbance to an invisible cursor) and see how well people deal with them.

>What about the oysters, Rick?

Delicious.

>And the trees?

Beautiful.

Best

Rick

Date: Fri, 20 Dec 1996 13:41:04 -0500
From: Martin Taylor <mmt@DCIEM.DND.CA>
Subject: Re: Sibling rivalry; solving the "loss of input" problem

[Martin Taylor 961220 13:30]
>Bill Powers (961219.1500 MST)

>My rearrangement was a suggestion
>that allows use of real-time input when it's available, and use of the model
>when it's not. This means that when the real input is available,
>disturbances can be resisted without any need to model or predict them. The
>model of the disturbance comes into play only when real input is lost (or
>perhaps when it's momentarily too noisy to use). It's always better to use
>the real input when possible.

Yes, but it isn't always better to use the real input _exclusively_. If there's any delay between output and perceptual effect of output, and there is some predictability about the disturbance (dMod != 0), control can be better by using that advance expectation. That's what incorporating the first derivative does for you. It's a trivial model that says "It's going that way, so it will keep going that way." And much of the time, it does. But it doesn't if its a pendulum.

Imagine trying to bring a swinging pendulum to rest when you can't hold it but can only push or pull a little with (say) a magnetic field. If you can make your push or pull almost as soon as you see the pendulum bob, you can push or pull in the direction opposite to the way it is going. But suppose that your influence is delayed until a half-cycle after your observation (or alternatively, the influence of action on perception is delayed a half cycle), then all your best efforts based on the real, current value will do is to increase the amplitude of oscillation. You'd have to reorganize in some way, or, equivalently, use a model of the future course of the pendulum (or of the delay of effect of you action). Either way, the _regularity_ of the world would be taken into account in allowing you to control.

I think a better statement would be: "It's always better to use what you can get hold of that's relevant." And what could be more relevant than the real, current, value of the very variable that you are interested in? Of course, I mean "relevant" to that variable at the time when your influence will come to bear. If that time is now, there isn't anything that can add to the present value. But that time cannot be "now" in any physical system. So other things you know, guess, or estimate could be valuable.

Martin

Date: Fri, 20 Dec 1996 13:48:52 EST
From: Bruce Gregory <gregory@CF4.HARVARD.EDU>
Subject: Re: Lies, damned lies and non-control

[From Bruce Gregory (962020.1350 EST)]

Martin Taylor 961220 13:10

> But if one forgets about the word, and asks whether model-based systems
> can sustain a real-world variable in a condition satisfactory for some
> other purpose (e.g. maintenance of intrinsic variables near _their_

> reference values), then model-based systems may very well be useful, even
> if they don't control. If the environment happens to be reasonably predictable,
> and the models are good, real control is only required as a kind of
> monitoring, to see whether the model is still keeping the variable on track.

It is not clear to me how control can be "a kind of monitoring".
The two functions seem quite distinct to this country boy.
>
> Engineered "control" systems don't care whether an internal perception is
> controlled. They "care" whether the job the designer wants done is done...in
> other words, the designer and the user may be control systems, when all
> the engineered system does is to keep something in the outer world stable
> for them, with or without "control". (As evolution did for us by providing
> skeletons, without which the muscular control systems would have a difficult
> time keeping our bodies within a small vertically oriented space for very
> long:-)

Certainly. But how much bandwith does CSGNet want to devote to
the optimal thickness of insulation for houses built in
Connecticut?

Bruce Gregory

Date: Fri, 20 Dec 1996 13:56:08 EST
From: Bruce Gregory <gregory@CF4.HARVARD.EDU>
Subject: Re: Lies, damned lies and non-control

[From Bruce Gregory (61220.1400 EST)]

Rick Marken (961220.1030)

> I think it's reasonable to assume that in the intact, relatively drug free
> organism, variations in p are typically a pretty good reflection of
> variations in the input (CVm or CV) so that the controlled input to a
> perceptual function (CV or CVm) can be considered the controlled variable.

The basis for a great "public service" campaign.

> I feel like I may not be getting this accross. The difference between
> what is happening when a system adds computed outputs to a CV versus what is
> happening when it generates these outputs in a closed loop of which CV is a
> part is more than just equations to me; it is a palpable _feeling_. I can
> _feel_ (in imagination) the difference between computed output "coincidental
> control" and the real thing. Help me Bill Powers. How can I communicate this
> difference in words?

Don't even try. Suggest that someone get on a straight section
of freeway when there is little traffic and close their eyes
for thirty seconds while driving. The effect is both dramatic
and compelling ;-)

Bruce Gregory

Date: Fri, 20 Dec 1996 13:10:44 -0600
From: Bruce Abbott <abbott@CVAX.IPFW.INDIANA.EDU>
Subject: Environmental variables are not controlled

[From Bruce Abbott (961220.1415 EST)]

>Rick Marken (961220.1030) --

>Bruce Abbott (961220.1150 EST)

>>You keep changing your story. Before you said that there would be _no_
>>opposition to the disturbance;

>No. I said there would be no _control_ in the "blind".

Ah, but there _is_ control. First of all, the _perception_ is under control at all times. Second, to the extent that CVm and CV match, CV is under control, too. In the case of a perfect match, the CV will do everything CVm does, including approaching and maintaining a near-reference value against disturbances.

I do understand the distinction you have in mind. As soon as an unmodeled disturbance hits the CV (during the blind condition), the difference between control via CV and control via CVm will become immediately apparent.

The problem is, that there is no mathematical distinction to be found between a system that uses dMod to establish _apparent_ control over the environmental variable, and a normal control system operating under the condition that d = dMod.

>>Rick, Rick, Rick. There is only the _appearance_ of control of the CV
>>when you are doing the standard tracking task used to illustrate a
>>perceptual control system. That appearance occurs because of the "lucky"
>>relationship between the cursor's position and the participant's
>>perception of the cursor's position (i.e., neural current p varies
>>systematically with the cursor's position). The _cursor_ (the thing out
>>there on the screen) isn't really controlled; what is controlled is the
>>_perception_ of the cursor. In model-based control, the relationship
>>between the perception and the CV will be identical to that found in the
>>normal PCT system if the model is perfect, and the _appearance_ of control
>>will be the same as in the normal PCT system. THERE IS NO DIFFERENCE --
>>either way, control of the CV is only an appearance.

>I think you will have a real _feel_ for how control actually works when you
>understand why most of what you said here is wrong. I can't _make_ you
>understand it; but I hope I can provide the opportunity for your
>understanding. . . .

>I feel like I may not be getting this accross. The difference between
>what is happening when a system adds computed outputs to a CV versus what is
>happening when it generates these outputs in a closed loop of which CV is a
>part is more than just equations to me; it is a palpable _feeling_. I can
>_feel_ (in imagination) the difference between computed output "coincidental
>control" and the real thing. Help me Bill Powers. How can I communicate this
>difference in words?

The reason you have having so much trouble getting this distinction across to me is that you are unable to find any justification for it. I doubt that even Bill will be able to rescue you from this one . . .

And cut out the condescending crap about my not understanding control theory. It would appear from our recent exchanges that I understand it better than you do.

Regards,

Bruce

Date: Fri, 20 Dec 1996 14:11:54 -0500
From: Martin Taylor <mmt@DCIEM.DND.CA>
Subject: Re: reply to Martin: combining goals

[Martin Taylor 961220 14:00]
> Hans Blom, 961219]

> >You can now replace "control for x=3 and for y=2 and for z=1" by
> >"minimize (x-3)^2 + (y-2)^2 + (z-1)^2". Which you can translate back
> >into "control for the minimum of (x-3)^2 + (y-2)^2 + (z-1)^2", if
> >you want. This is how you find the "top" goal.
>
> >So the analyst says. But the organism doesn't (by construction; the
> >simulated organism has been _defined_to have no such "top" goal).
>
> This is the core of the problem, I think:
>
> 1. You construct a control system with 3 independent, parallel goals.
>
> 2. I demonstrate to you that there is 1 mathematically equivalent
> "top-level" goal that subsumes your three goals.
>
> 3. You say "No, I constructed 3 goals". I say "Sure, but those 3 can
> be compacted into 1".
>
> That is, your construction seems to have an implication ("unintended
> side effect"?) that you neither intended nor now wish to intend. And
> therefore you cannot -- and do not want to -- see that super-goal as
> something that _causes_ the behavior. And it doesn't, of course; the
> 3 goals that you specified for and designed into the controller cause
> its behavior. In that sense, the super-goal is not causal; it is only
> mathematically equivalent to your 3 causal parallel goals combined.
> Or does that make the super-goal causal by implication?

No it doesn't, because (by design), there is no representation anywhere except in the analyst's mind of the value of the super-goal. If the fact that the system is being observed and analyzed makes the variables in the analysis causal for the system, then you and I have very different views on the meaning of causality.

> >The _analyst_ sees that the organism seems to behave _as if_ it did
> >have such a top goal. But in fact (as we know, having constructed
> >the organism to be so) the organism has three independent "top"
> >goals, and the analyst has engaged in self-deception--or at least is
> >wrong--in saying that it has one "top" goal.
>
> Beautiful! Here we have the raw contrast between "as if" and "in
> fact" ;-). We see one and the same thing, the 3-goal controller that

> you designed. You see that it has properties P1, P2, ..., PN. I see
> an additional property PN+1. Is that property "in fact" there, or is
> it only "as if" it is there, but not really? What kind of test would
> you propose to make this question decidable?

I would have to dissect the system and see whether I could find any representation of property PN+1 of the same kind as the representations that I will find of P1, P2, ... PN. I can't tell from the external analysis whether I will find PN+1, because we have started from the hypothesis that the system will behave in all ways as if that property was represented internally in the organism.

Look, the issue wasn't whether one can determine whether a control hierarchy has a single top level goal in any particular case, but whether ALL control hierarchies must be BUILT so that they support one top-level goal. We know, for example, that all living control systems had as ancestors control systems that survived long enough to propagate their genes. We do NOT know that any of these ancestral control hierarchies were built so that they had a single top-level elementary control unit whose reference level for a perception of surviving had the value "long enough to propagate my genes." But we do know that they ALL, over billions of years, without exception, behaved as if they did have such a goal.

> I know a different test. We must both design a control system whose 3
> goals (x=3, etc.) are provided to us. You build a controller which
> implements those 3 goals directly. I first combine the three goals
> into one super-goal, after which I design my controller to control
> for that super-goal. When testing the behaviors of both controllers,
> they turn out to be exactly identical. Can we now answer the
> following questions:
>
> - do both controllers have one super-goal?

No.

> - do both controllers have 3 independent, parallel goals?

Yes, but in your case those parallel goals support the one super-goal. In mine, they don't, because mine was built not to have such a super-goal.

> - does your controller have 3 goals whereas mine has one?

Yours has four, mine has three. The referents for three of your goals are set by the control system for the top one.

>
> >> ... different weights can be attached to the individual terms
>
> >Now your "top" goal has three extra degrees of freedom that the
> >analyst can use to get around the problem that the gains on the x,
> >y, and z "real" perceptual control systems may be different, leading
> >to ellipsoidal rather than spherical iso-deviation contours.
>

> A lot of different roads lead to Rome, as they say here.
> That doesn't mean that Rome's location has "degrees of freedom".
>
> >The analyst can still delude himself into seeing a non-existent top

> >goal by using these extra degrees of freedom provided by the
> >weights.
>
> This I do not understand. Can you explain?
>

It's not the location of Rome that has extra degrees of freedom, but the ability to describe the behaviour used in getting to Rome--the behaviour you use to determine whether Rome is a "top-level goal" or simply the place arrived at because you wanted to get to longitude X and latitude Y for two irrelevant reasons. Your weights correspond to differences in the gain functions for X and Y control at the top-level of a hierarchy with no single (multidimensional) "Rome" ("perceiving myself to be at x,y") goal.

Martin

Date: Fri, 20 Dec 1996 11:31:36 -0800
From: Richard Marken <marken@AEROSPACE.AERO.ORG>
Subject: Re: Environmental variables are not controlled

[From Rick Marken (961220.1130)]

Bruce Abbott (961220.1415 EST)--

>I do understand the distinction you have in mind. As soon as an unmodeled
>disturbance hits the CV (during the blind condition), the difference between
>control via CV and control via CVm will become immediately apparent.

>The problem is, that there is no mathematical distinction to be found
>between a system that uses dMod to establish _apparent_ control over the
>environmental variable, and a normal control system operating under the
>condition that d = dMod.

But there is. I gave it in my previous post. CV is mathematically _not_ in the loop when the system uses dMod to establish _apparent_ control of CV;CV is in the loop when the system has _actual_ control of CV.

>The reason you have having so much trouble getting this distinction across
>to me is that you are unable to find any justification for it.

The distintion is simply that CV is in the loop (with o) in one case and not in the other.

>I doubt that even Bill will be able to rescue you from this one . . .

Maybe not;-(

>And cut out the condescending crap about my not understanding control
>theory.

Ah. Now you know how I feel about your posts;-)

>It would appear from our recent exchanges that I understand it better than
>you do.

Yes, if "better" means "in a way that allows you to see no conflict between PCT and the foundations of experimental psychology".

Best

Rick

PS. I was reading the "After Galileo" chapter of Living Control Systems II last night. I highly recommend it to everyone but especially to you, Bruce. It might help you understand some of the variables I am controlling, which make so much of what you say a rather massive disturbance.

Date: Fri, 20 Dec 1996 10:41:18 PDT
From: CCHERPAS <CCHERPAS@CCCPP.COM>
Subject: Re: Executive VP position

[From Chris Cherpas (961220.1025 PT)]

Thanks to all who responded to my search for an Executive VP.

Some commonly asked questions so far:

- CCC is located in Sunnyvale California which in the middle of Silicon Valley (around the southern tip of SF Bay, from east to west, there is San Jose, Santa Clara, Sunnyvale, Mountain View, and Palo Alto)

- I will get on our HR manager's schedule to pursue details about how we can put together some arrangements for everybody to get to know each other, etc.

- It would be a good move to send me a resume; if you want to send something fast, but feel you'd like more time and information to refine it before actual hiring officials (not just a back-room guy like me), then let me know. I want the caliber of people who have responded to get every advantage I can possibly provide in this process.

Once again, I am very grateful for your replies and would have responded to each of you individually by now, but I just got overwhelmed with more projects than HUD -- all demanding attention today! So this message is mostly to explain why I haven't replied seriously/individually yet. Thanks for your patience.

Best to all,
cc

Date: Fri, 20 Dec 1996 14:48:21 -0500
From: Martin Taylor <mmt@DCIEM.DND.CA>
Subject: Re: Lies, damned lies and non-control

[Martin Taylor 961220 14:30]
> Bruce Gregory (962020.1350 EST)]
>
> Martin Taylor 961220 13:10
>
> >... model-based systems may very well be useful, even

> > if they don't control. If the environment happens to be reasonably
predictable,
> > and the models are good, real control is only required as a kind of
> > monitoring, to see whether the model is still keeping the variable on track.
>
> It is not clear to me how control can be "a kind of monitoring".
> The two functions seem quite distinct to this country boy.

It's not clear to me which two functions you are talking about, of the three in question. However, rather than trying to guess at that, let me try to put into other words what I was getting at.

(1) I see little value in squabbling over words when there are issues at hand referring to the way things work. Hence, I referred to "stabilizing" environmental variables and "controlling" perception, just to keep the peace. One aspect of this point is that it is the stabilization of the environmental variable that matters, but the perceptual control is all we can actually do. Model-based (planned) output may aid stabilization sometimes, when disturbances are well predicted.

(2) By "monitoring" I did not mean anything more clever than "control at much lower bandwidth than would otherwise be necessary." If the model keeps working well and seems ordinarily likely to do so, the only "control" needed to sustain the stabilization of the environmental variable over prolonged periods (impossible with model-based outflow alone) can be accomplished by a much slower and lower gain control system than would otherwise be necessary. If the model can't be trusted to work well, full fledged perceptual control would be necessary. It's control, either way, but I used "monitoring" with the connotation that the control is far less aggressive.

> > Engineered "control" systems don't care whether an internal perception is
> > controlled. They "care" whether the job the designer wants done is done...in
> > other words, the designer and the user may be control systems, when all
> > the engineered system does is to keep something in the outer world stable
> > for them, with or without "control". (As evolution did for us by providing
> > skeletons, without which the muscular control systems would have a difficult
> > time keeping our bodies within a small vertically oriented space for very
> > long:-)
>
> Certainly. But how much bandwith does CSGNet want to devote to
> the optimal thickness of insulation for houses built in
> Connecticut?

Oh, CSGnet could restrict itself to "monitoring" that, were it not for the fact that certain voluble participants assert that it matters that the thickness of turtle-shells doesn't contribute to the performance of the control hierarchy--that model-based stabilization of environmental variables is not control and shouldn't be talked about on CSGnet.

If we can keep straight that _both_ perceptual control _and_ model-based output can, under different curcumstances, be useful in stabilizing important environmental variables, then perhaps we can deal with them not as conflicting propositions but as complementary possible strategies that could support each other. A control loop in a model-based system could reduce the "unmodelled deviation" (not "disturbance") to the stabilized variable; a good model (such as the Artificial Cerebellum) somewhere in a control loop could reduce the effect of the disturbance and enhance control, if the environmental feedback loop or the disturbance had

partly predictable characteristics.

Perhaps it would be a good idea if we were to use "control" for perceptual control, and "stabilization" for what happens to an environmental variable whose perceptual correlate is controlled. Stabilization, but not control, also happens when a good outflow model opposes a well-predicted disturbance.

Martin

Date: Fri, 20 Dec 1996 15:28:23 EST
From: Bruce Gregory <gregory@CF4.HARVARD.EDU>
Subject: Re: Lies, damned lies and non-control

[From Bruce Gregory (961220.1530 EST)]

Martin Taylor 961220 14:30

> If we can keep straight that _both_ perceptual control _and_ model-based
> output can, under different curcumstances, be useful in stabilizing
> important environmental variables, then perhaps we can deal with them
> not as conflicting propositions but as complementary possible strategies
> that could support each other. A control loop in a model-based system
> could reduce the "unmodelled deviation" (not "disturbance") to the
> stabilized variable; a good model (such as the Artificial Cerebellum) somewhere
> in a control loop could reduce the effect of the disturbance and enhance
> control, if the environmental feedback loop or the disturbance had
> partly predictable characteristics.
>
> Perhaps it would be a good idea if we were to use "control" for perceptual
> control, and "stabilization" for what happens to an environmental variable
> whose perceptual correlate is controlled. Stabilization, but not control,
> also happens when a good outflow model opposes a well-predicted disturbance.

Could it not be that, as far as living control systems are concerned, the time scale for the results of outflow models such as the thickness of turtle shells or the behavior of deciduous trees is much much longer than the time scales for perceptual control? In which case might the former be of limited value in understanding the behavior of living systems?

Bruce Gregory

Date: Fri, 20 Dec 1996 15:03:27 -0600
From: Bruce Abbott <abbott@CVAX.IPFW.INDIANA.EDU>
Subject: Re: Lies, damned lies and non-control

[From Bruce Abbott (961220.1405 EST)]

>Bruce Gregory (961220.1240 EST) --

>>Bruce Abbott (961220.1150 EST)

>> This world is _full_ of highly predictable disturbances, Rick. Example:
>> Even in California, some of the trees there shed their leaves in the fall,

>> preparing for the coming freeze. (I know because I grew up there.) These
>> systems are acting to oppose a disturbance that only correlates with the
>> season, by using the length of daylight as a surrogate variable. I think
>> the problem with the perception that disturbances are highly unpredictable
>> is that one tends to focus on the lowest-level systems, for which
>> disturbances are frequent and largely unpredictable. But there are
>> lower-frequency systems whose CVs tend to behave rather predictably, and for
>> these systems, model-based control might be an option. . . .

>To this country boy, there appears to be an ambiguity in the
>use of the the word "control" in this exchange. I-90 is wide
>and has many straight stretches. Despite this, I would predict
>disaster if I were close my eyes for thirty seconds while
>driving at 65 mph even if there were no other cars on the road.
>I would also be very reluctant to say that I was controlling
>the car while my eyes were closed. Praying would seem more
>descriptive than controlling ;-)

You have chosen for your illustration just the sort of situation in which model-based control would be at its worst, with fast-acting and unpredictable disturbances acting continuously. This of course ignores entirely the content of the paragraph to which this is supposedly a reply.

>It seems to me that you put your coat on before you go out
>because you have a reference state for not going outdoors in
>the winter without a coat. You can certainly call this reference
>state a model of the world, but it doesn't seem to me that what
>you are doing involves this model except as a reference level.
>But as I say, I'm just a country boy and therefore several steps
>below even a simple engineer.

I'm probably even simpler, Bruce, so if you can offer a cogent argument I'm likely to buy it.

I think it would clarify your argument to rephrase it in ordinary English:

  It seems to me that you put your coat on before you go out because you
  don't want to go outdoors without a coat.

Well, that's true enough, so far as it goes.

But _why_ do you want to go outdoors with your coat on? Clearly there is no current temperature-disturbance acting on your system, for which setting a reference for putting on your coat would be an appropriate action, so it would appear that there is no reason for you to do so.

Regards,

Bruce

Date: Fri, 20 Dec 1996 13:42:51 -0700
From: "T. B. Harms" <tbh@TESSER.COM>
Subject: DIRECT Re: Lies, damned lies and non-control

Martin,

I found your recent post most persuasive, and thank you for arguing in this

direction.

Tracy

>[...]
>If we can keep straight that _both_ perceptual control _and_ model-based
>output can, under different curcumstances, be useful in stabilizing
>important environmental variables, then perhaps we can deal with them
>not as conflicting propositions but as complementary possible strategies
>that could support each other. [...]
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>Perhaps it would be a good idea if we were to use "control" for perceptual
>control, and "stabilization" for what happens to an environmental variable
>whose perceptual correlate is controlled. Stabilization, but not control,
>also happens when a good outflow model opposes a well-predicted disturbance.

Date: Fri, 20 Dec 1996 13:21:11 -0800
From: Richard Marken <marken@AEROSPACE.AERO.ORG>
Subject: Re: Lies, damned lies and non-control

[From Rick Marken (961220.1330)]

Bruce Gregory (962020.1350 EST) --

> It is not clear to me how control can be "a kind of monitoring".
> The two functions seem quite distinct to this country boy.

Martin Taylor (961220 14:30) --

>By "monitoring" I did not mean anything more clever than "control at
>much lower bandwidth than would otherwise be necessary."

Boy, these city slickers can sure shine ya on, can't they Bruce?

>If we can keep straight that _both_ perceptual control _and_ model-based
>output can, under different curcumstances, be useful in stabilizing
>important environmental variables, then perhaps we can deal with them
>not as conflicting propositions but as complementary possible strategies
>that could support each other.

Spoken like a true non-experimentalist.

>Perhaps it would be a good idea if we were to use "control" for perceptual
>control, and "stabilization" for what happens to an environmental variable
>whose perceptual correlate is controlled.

I don't think so. Martin. Nice try. I think we should call control "control".

The environmental variable in a control loop is controlled. Try it and see
for yourself. Do what I now call the "Nature of Control" tracking demo at my
web site. I now provide two measures of control: RMS Error and Stability.
Both of these measures are computed on the basis of the value of an
environmental variable (cursor position in pixels) _not_ a perceptual
variable (the neural correlate of cursor position or whatever the "real"
controlled variable is in your brain). Lo and behold you will see that the

cursor, an environmental variable -- the variable that you and Bruce Abbott seem to think is _not_ controlled by a control system -- is under control.

Best

Rick

Date: Fri, 20 Dec 1996 14:22:21 -0700
From: "T. B. Harms" <tbh@TESSER.COM>
Subject: Re: Lies, damned lies and non-control

*From Tracy Harms 1996;12,20.14

First of all, I see that a message I intended to send privately to Martin Taylor went to the whole group. Ooops.

Now in response to Bruce Gregory (961220.1530 EST)

>Could it not be that, as far as living control systems are
>concerned, the time scale for the results of outflow models
>such as the thickness of turtle shells or the behavior of
>deciduous trees is much much longer than the time scales for
>perceptual control? In which case might the former be of limited
>value in understanding the behavior of living systems?

What looms large for me as the place we're going to find applicability is the setting of reference levels. If anticipatory aspects of organisms drive their reference levels -- and I'd say the examples from zoology are overwhelmingly abundant -- this has a very clear connection to the behavior of living systems.

Tracy Bruce Harms tbh@tesser.com
Boulder, Colorado

Date: Fri, 20 Dec 1996 16:34:09 EST
From: Bruce Gregory <gregory@CF4.HARVARD.EDU>
Subject: Re: Lies, damned lies and non-control

[From Bruce Gregory (961220.1635)]

>From Tracy Harms 1996;12,20.14

> What looms large for me as the place we're going to find applicability is
> the setting of reference levels. If anticipatory aspects of organisms
> drive their reference levels -- and I'd say the examples from zoology are
> overwhelmingly abundant -- this has a very clear connection to the behavior
> of living systems.

If the setting of reference levels is all that is meant by model-based behavior, then we are all one happy family. When people argue that environmental variables are not controlled, I suspect we are on the other side of the looking glass.

Bruce Gregory

Date: Fri, 20 Dec 1996 15:15:58 -0700
From: Bill Powers <powers_w@FRONTIER.NET>
Subject: HPCT vs Simulation-based control

[From Bill Powers (961220.1500 MST)]

Bruce Abbott suggests that the shedding of leaves in the autumn is an example of model-based or simulation-based control. But that is to accept the model-based idea as simpler than an alternative HPCT-based idea. What variable do you think that trees might control by shedding their leaves, and what perception do you think might be affected by this means of action? Is the behavior we see (the changes in color, the drying out, and breaking off of the stems) likely to be the relevant aspect of the tree's behavior? Are we seeing the variable that the tree's action is controlling?

I think there's a great temptation for some to offer what seems to be a simple cause-effect explanation of phenomema like these. What could be simpler than to say that cold weather or short days cause trees to shed their leaves? The pitfalls of this approach become evident when you start asking HOW these causes could produce these effects. Can trees perceive the length of a day or the temperature of the air and ground? Can they anticipate that winter is coming? Where do these computations take place? In the leaves? The roots? The channels beneath the bark? Does the environment as we know it even exist for a tree?

I think that when you look beneath the surface appearances, it is the control-system model that comes out looking the simplest.

Best,

Bill P.

Date: Fri, 20 Dec 1996 22:50:12 +0100
From: Oded Maler <Oded.Maler@IMAG.FR>
Subject: Re: Lies, damned lies and non-control

(from Oded Maler 961220)
>[From Bruce Gregory (61220.1400 EST)]
>Don't even try. Suggest that someone get on a straight section
>of freeway when there is little traffic and close their eyes
>for thirty seconds while driving. The effect is both dramatic
>and compelling ;-)

How about a person daily getting out of his door without being able to perceive whether during the night someone was putting a bomb under his doorstep. I am not sure whether a "real' controller (in contrast to a model-based) can be implemented in flesh and blood.

Besides it's only a question of time-scales.

--Oded

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Oded Maler, VERIMAG, Miniparc ZIRST, 38330 Montbonnot, France
Phone: 76909635 Fax: 76413620 e-mail: Oded.Maler@imag.fr
Home page: http://www.imag.fr/VERIMAG/PEOPLE/Oded.Maler
==========

Date: Fri, 20 Dec 1996 14:56:59 -0700
From: Bill Powers <powers_w@FRONTIER.NET>
Subject: How imagination helps control

[From Bill Powers (961220.1030 MST)]

Bruce Abbott's program has helped me to see something. Imagination can help with control even if no attempt is made to model the disturbance (which in general is pretty futile if you try to model it for any substantial time into the future). Looking at RunBlnd3, you can see that while the unmodeled disturbance is having some pretty large effects on the controlled variable, the AVERAGE value of the controlled variable is jumping up and down with the reference signal. So if there were no unmodeled disturbance, the CV would be behaving nearly correctly, as correctly as the accuracy of the world-model (here a multiplier of 1) permits.

It's not necessary to have a complete world-model or even a very good world-model in order to get some benefits from it, as Hans has pointed out. Even if the world-model represents only _part_ of the actual external situation, it can make it possible for changes in the reference signal to have effects in the right general direction, because the outputs will vary in the right direction (as long as the partial world-model is reasonably representative of the whole situation). The effect bears a resemblance to the Marken Effect, where an external control system that conflicts with the internal one can actually improve control.

As we can see from the program, the perception is controlled in all cases, sometimes much better than its external correlate is controlled. As far as the organism is awaredly concerned it is control of perception that is real, and all else is hypothetical. But as a number of people have pointed out (Martin Taylor said this long ago, and today and yesterday has been amplifying on this idea to our benefit), the _actual_ effects of behavior reflected onto the organism are the ultimately critical factor, because those are what determine intrinsic error, and correcting intrinsic error determines when reorganization will cease. It's the reorganizing system that provides the reality test. And this is what shapes the world-models -- not necessarily by the EKF method, but by SOME method that continually compares actual physical effect with the effects that are desired, and adjusts the model to eliminate the difference, as much as possible.

Long parenthesis:

I think "world-model" is a rather overblown way of describing the kind of internal imagination connection that gives us these advantages. Especially in a modular hierarchical system, the "world-model" associated with any one module (if it exists) is simply an approximation of the effect of the output signal on the one-dimensional perceptual signal. In Bruce's program it's just a multiplier of 1, corresponding to the external multiplier of 1. In some (maybe all) spinal motor neurons, there's a negative feedback

connection from the output signal to the input signal that goes through an internuncial neuron which has special properties: in terms of frequency it's a leaky integrator. Hit it with a sudden train of constant-frequency input impulses, and its output signal will slowly rise in frequency to an asymptote. Remove the input signal, and its output will slowly decline in frequency to zero, along an exponential decay curve. You could say that this is an imagination connection, a world model, that represents the effect of a muscle on the muscle tension signal. But you could also say that it just causes a phase advance in the transfer function of the motor neuron, which compensates partly for the lag in the muscle response.

In lower-level systems, calling this sort of feedback connection a world-model is, in my opinion, overkill. It's reading too much into a simple connection. It's not a model of very much of the world, if calling it a model is justified at all.

Anyway, back to the main thread before I forget my point again (long parentheses require a well-functioning short-term memory).

There is an _objective_ sense in which model-based control can have beneficial effects, "objective" meaning "acting through the physical world" instead of only through perceptions. But the model-based control is always a second choice, because the tightest and most reliable control comes through real-time perception. In Bruce's program you can see how using the model at least produces changes in the CV of about the right kind -- but when you switch to sighted control, the improvement is immediate and drastic.

Furthermore, the model-based and real-time control connections can't coexist. Bruce, you might want to try adding the two signals together to see what happens to CV and CVm). You can probably guess what would happen even without trying it. Both CV and CVm would come to half the value of the reference signal. There's no _simple_ way I can think of to combine the imaginary and real information and still get control. If you average the two, then when you lose the input information the model's CVm will be controlled at half the reference value. You have to choose which to control.

Finally, I don't want the above musings to leave the wrong impression. Unless you've played with control models for some time, it's easy to get the impression that "an effect in the right direction" belongs in the same discussion with real control. Just look at RunBlnd3: when you switch to sighted control, the CV immediately starts following the reference signal with only a very small error, despite the perfectly enormous disturbances. Look at the white "output" trace -- it's varying rapidly and by extreme amounts, actively countering the effects of the large unmodeled disturbance and keeping the CV within 10% of the value of the reference signal. Most disturbances are like this: they would, without the counteracting behavior, cause the CV to depart radically from the reference level. Practically all of our muscle forces are used to counteract disturbances of one sort or another, starting with gravity. When you lift a 20-pound suitcase, 19.99 pounds of your effort are going into cancelling the disturbance of the suitcase's weight and mass. And if the suitcase weighs only 2 pounds, you don't fling it over your head, contrary to well-known thought experiments that keep being cited. If that were true, UPS would go out of business.

Last comment. The term "model" keeps cropping up in places where I think it's inappropriate. Hans' world-model is a _simulation_ of the environment. But where Martin wants to use the term "model" often would't fit the concept of a simulation. Adding a first derivative to a perceptual signal is not a simulation of anything. So if there is a persistent usage of the term model

in these other connections, perhaps we should call Hans' model a simulation-based control system.

Best to all,

Bill P.

Date: Fri, 20 Dec 1996 16:17:39 EST
From: Bruce Gregory <gregory@CF4.HARVARD.EDU>
Subject: Re: Lies, damned lies and non-control

[From Bruce Gregory (961220.1620)

Bruce Abbott (961220.1405 EST)]

> You have chosen for your illustration just the sort of situation in which
> model-based control would be at its worst, with fast-acting and
> unpredictable disturbances acting continuously. This of course ignores
> entirely the content of the paragraph to which this is supposedly a reply.

Really? I had in mind a smooth flat section of highway without any traffic. Normally, the disturbances would not even be noticable. If these are fast acting unpredictable disturbances, you must have a _very_ isolated system in mind.

> I think it would clarify your argument to rephrase it in ordinary English:
>
> It seems to me that you put your coat on before you go out because you
> don't want to go outdoors without a coat.
>
> Well, that's true enough, so far as it goes.
>
> But _why_ do you want to go outdoors with your coat on? Clearly there is no
> current temperature-disturbance acting on your system, for which setting a
> reference for putting on your coat would be an appropriate action, so it
> would appear that there is no reason for you to do so.

Many of my actions are not taken in response to disturbances. (That's because I'm an autonomous perceptual control system ;-)) I assume you are too. Therefore I assume you establish your reference levels levels and then act so that your perceptions match them. I want to go home home at the end of the day. I set a reference level for perceiving myself leaving the office. It is December. I set a reference level for putting on my coat before I leave my office. If setting reference levels is what you mean by acting on the basis of a model, then we work in the same way ;-)

Bruce Gregory

Date: Fri, 20 Dec 1996 17:10:50 -0600
From: Bruce Abbott <abbott@CVAX.IPFW.INDIANA.EDU>
Subject: Re: Lies, damned lies and non-control

[From Bruce Abbott (961220.1810 EST)]

>Rick Marken (961220.1330) --

Boy, these city slickers can sure shine ya on, can't they Bruce?

Actually, it's those country boys you have to watch out for. They pretend to be ignorant and unsophisticated and then WHAM, they hit you between the eyes, just when you've let your guard down. Remember when Sam Irvin chaired the Watergate hearings? He said that he was "just a country lawyer." Yeah, right. (:->

>The environmental variable in a control loop is controlled. Try it and see
>for yourself.

Well, sure had ME fooled! Guess lil' ol' me was misled by the name of the theory -- _perceptual_ control theory -- into the mistaken belief that _perceptions_ are under control. To prevent simple guys like me from being snookered like this in the future, I think we MUST change the name immediately, to (what else?) Environmental Control Theory (ECT).

Oh, my, I must be getting delusional. I have this clear recollection of Bill P. offering the example of a thermostat with a defective sensor. Seems that the damned thing kept the room temperature way over set point, merrily going along controlling its _perception_ of the room's temperature (as given by the defective sensor) rather than the actual temperature of the room. But if what you say is correct, this can't be true.

Deleriously,

Bruce

P.S. For what it's worth (given my current delusional state), I agree with Martin. Well said, Martin.

Date: Fri, 20 Dec 1996 17:53:27 EST
From: Bruce Gregory <gregory@CF4.HARVARD.EDU>
Subject: Re: How imagination helps control

[From Bruce Gregory (961220.1800 EST)]

Bill Powers (961220.1030 MST)]

> Last comment. The term "model" keeps cropping up in places where I think
> it's inappropriate. Hans' world-model is a _simulation_ of the environment.
> But where Martin wants to use the term "model" often would't fit the concept
> of a simulation. Adding a first derivative to a perceptual signal is not a
> simulation of anything. So if there is a persistent usage of the term model
> in these other connections, perhaps we should call Hans' model a
> simulation-based control system.

I believe the common expression for such a simulation is "thinking". What do we lose by saying that thinking is one way in which we set reference levels. What else can we do but set reference levels?

Bruce Gregory

Date: Fri, 20 Dec 1996 17:47:26 -0600
From: Bruce Abbott <abbott@CVAX.IPFW.INDIANA.EDU>
Subject: Re: Lies, damned lies and non-control

[From Bruce Abbott (961220.1850 EST)]

>Martin Taylor 961220 14:30 --

>(1) I see little value in squabbling over words when there are issues at
>hand referring to the way things work. Hence, I referred to "stabilizing"
>environmental variables and "controlling" perception, just to keep the
>peace. One aspect of this point is that it is the stabilization of the
>environmental variable that matters, but the perceptual control is all
>we can actually do. Model-based (planned) output may aid stabilization
>sometimes, when disturbances are well predicted.

Very nice, Martin. This is one of the ideas I've been trying to get across, without much success I'm afraid, owing to confusion about what should or should not be called "control." I like your distinction between "stabilizing" and "controlling," although it does tend to muddy the similarity I was attempting to bring out between normal and model-based control.

>If we can keep straight that _both_ perceptual control _and_ model-based
>output can, under different curcumstances, be useful in stabilizing
>important environmental variables, then perhaps we can deal with them
>not as conflicting propositions but as complementary possible strategies
>that could support each other. A control loop in a model-based system
>could reduce the "unmodelled deviation" (not "disturbance") to the
>stabilized variable; a good model (such as the Artificial Cerebellum) somewhere
>in a control loop could reduce the effect of the disturbance and enhance
>control, if the environmental feedback loop or the disturbance had
>partly predictable characteristics.

Hear hear! That's exactly what I had in mind.

I believe there may be a perception that I have been _advocating_ model-based control over ordinary feedback control, but I have not been. My objective has been to move the discussion toward _evaluating_ the usefulness of such systems, which is in my view a better strategy than outright rejection based on assorted verbal arguments and gut feelings. There's an old saying I like very much that goes as follows: "It is better to debate a question without settling it than to settle a question without debating it." (I'm sorry, but I've forgotten the author of this quote.)

With respect to the computer simulations Bill and I have posted, I would like to know who has actually compiled and run any of them (other than Bill and Rick, who I know have). Thus far, I have received not one request for a compiled version that could be run without the need for a compiler.

Regards,

Bruce

Date: Fri, 20 Dec 1996 19:23:53 -0500
From: David <dgoldstein@P3.NET>
Subject: Re: Lies, damned lies and non-control

Bruce Abbott wrote:
>
> [From Bruce Abbott (961220.1850 EST)]
>
> >Martin Taylor 961220 14:30 --
>
> >(1) I see little value in squabbling over words when there are issues at
> >hand referring to the way things work. Hence, I referred to "stabilizing"
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> >peace. One aspect of this point is that it is the stabilization of the
> >environmental variable that matters, but the perceptual control is all
> >we can actually do. Model-based (planned) output may aid stabilization
> >sometimes, when disturbances are well predicted.
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> Very nice, Martin. This is one of the ideas I've been trying to get across,
> without much success I'm afraid, owing to confusion about what should or
> should not be called "control." I like your distinction between
> "stabilizing" and "controlling," although it does tend to muddy the
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>
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> >in a control loop could reduce the effect of the disturbance and enhance
> >control, if the environmental feedback loop or the disturbance had
> >partly predictable characteristics.
>
> Hear hear! That's exactly what I had in mind.
>
> I believe there may be a perception that I have been _advocating_
> model-based control over ordinary feedback control, but I have not been. My
> objective has been to move the discussion toward _evaluating_ the usefulness
> of such systems, which is in my view a better strategy than outright
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> like to know who has actually compiled and run any of them (other than Bill
> and Rick, who I know have). Thus far, I have received not one request for a
> compiled version that could be run without the need for a compiler.
>
> Regards,
>
> Bruce

Bruce,

Let me be the first to request a compiled version. I forgot that this was possible.

Are you going to send it as an attachment? I have Netscape Navigator and I believe that I could unattach it and save it and run it.

Thanks,

David M. Goldstein

End of CSGNET Digest - 20 Dec 1996 to 21 Dec 1996
****

Date: Sun, 22 Dec 1996 08:00:08 +1800
Subject: CSGNET Digest - 21 Dec 1996 to 22 Dec 1996

There are 19 messages totalling 2166 lines in this issue.

Topics of the day:

1. Lies, damned lies and non-control (4)
2. Driving blind; wearing coats (6)
3. Autonomy (3)
4. Control of perception
5. ECT (3)
6. Determinacy
7. Bruce's runblind compiled program

Date: Sat, 21 Dec 1996 07:23:55 -0700
From: Bill Powers <powers_w@FRONTIER.NET>
Subject: Re: Lies, damned lies and non-control

--=====================_851203435==_
Content-Type: text/plain; charset="us-ascii"

Hi, David --

I'm sending you my latest revision of Bruce's program as an executable file, attached to this post. Hope it works.
Best,
Bill P.

--=====================_851203435==_
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Content-Transfer-Encoding: base64
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--=====================_851203435==_--

Date: Sat, 21 Dec 1996 11:56:19 -0600
From: Bruce Abbott <abbott@CVAX.IPFW.INDIANA.EDU>
Subject: Re: Driving blind; wearing coats

[From Bruce Abbott (961221.1255 EST)]

>Bruce Gregory (961220.1620) --

>>Bruce Abbott (961220.1405 EST)

>> You have chosen for your illustration just the sort of situation in which
>> model-based control would be at its worst, with fast-acting and
>> unpredictable disturbances acting continuously. This of course ignores
>> entirely the content of the paragraph to which this is supposedly a reply.

>Really? I had in mind a smooth flat section of highway without
>any traffic. Normally, the disturbances would not even be
>noticable. If these are fast acting unpredictable disturbances,
>you must have a _very_ isolated system in mind.

There are plenty of large, fast-acting disturbances acting on the car's direction of travel even on supposedly smooth, flat section of empty highway. It's not the sort of situation I'd want to rely on model-based control for long in. However, when I drive around town I do seem to have some sort of internal representation of the layout of the city streets. That representation usually allows me to get from Point A to Point B without a map, usually without a hitch, even if I've never followed that particular

route before. The reason is that disturbances of this variable are for all practical purposes absent. Not once have I arrived in my neighborhood to discover, much to my shock, that my house is no longer there. (California residents may have a different experience!)

>> But _why_ do you want to go outdoors with your coat on? Clearly there is no
>> current temperature-disturbance acting on your system, for which setting a
>> reference for putting on your coat would be an appropriate action, so it
>> would appear that there is no reason for you to do so.

>Many of my actions are not taken in response to disturbances.
>(That's because I'm an autonomous perceptual control system ;-))
>I assume you are too. Therefore I assume you establish your
>reference levels levels and then act so that your perceptions
>match them. I want to go home home at the end of the day. I set
>a reference level for perceiving myself leaving the office. It
>is December. I set a reference level for putting on my coat
>before I leave my office. If setting reference levels is what
>you mean by acting on the basis of a model, then we work in the
>same way ;-)

I, I, I. What is this "I" of which you speak? How does it set reference levels? How does it know what reference levels will need to be set to achieve its various goals? How does it know that it will probably be cold outside, and that being exposed to the cold will bring about a discomfort it wishes to avoid? How does it know that putting on a coat will help to minimize the effect of this disturbance to comfort?

As for me, I'm _not_ an autonomous perceptual control system, if by "autonomous" you mean "acting independently of physical causes," I'm a completely determinate machine, down to (but not including) the quantum level. I'm autonomous only in the sense of having internal references that determine which variables I will control at what levels.

Regards,

RoboBruce

Date: Sat, 21 Dec 1996 13:02:38 -0500
From: Bruce Gregory <gregory@CF4.HARVARD.EDU>
Subject: Re: Lies, damned lies and non-control

[From Bruce Gregory (961221.1300 EST)]

> Hi, David --
>
> I'm sending you my latest revision of Bruce's program as an executable file,
> attached to this post. Hope it works.
> Best,
> Bill P.
>
This went out to everybody, Bill. Bruce's program ran, but so fast that I couldn't make sense of it. Either I'm too slow (very likely) or my 133 MHz Pentium is too fast.

Bruce Gregory

Date: Sat, 21 Dec 1996 13:12:33 -0500
From: Bruce Gregory <gregory@CF4.HARVARD.EDU>
Subject: Re: Driving blind; wearing coats

[From Bruce Gregory (961221.1310 EST)]

Bruce Abbott (961221.1255 EST)]

> There are plenty of large, fast-acting disturbances acting on the car's
> direction of travel even on supposedly smooth, flat section of empty
> highway. It's not the sort of situation I'd want to rely on model-based
> control for long in. However, when I drive around town I do seem to have
> some sort of internal representation of the layout of the city streets.
> That representation usually allows me to get from Point A to Point B without
> a map, usually without a hitch, even if I've never followed that particular
> route before. The reason is that disturbances of this variable are for all
> practical purposes absent. Not once have I arrived in my neighborhood to
> discover, much to my shock, that my house is no longer there. (California
> residents may have a different experience!)

I would say that you use your internal map to set reference levels, but
since

> As for me, I'm _not_ an autonomous perceptual control system, if by
> "autonomous" you mean "acting independently of physical causes," I'm a
> completely determinate machine, down to (but not including) the quantum
> level. I'm autonomous only in the sense of having internal references that
> determine which variables I will control at what levels.

I can see that my explanation would make no sense to you. Your self
image will also make it clear to Rick why he has so little success in
communicating with you -- you really _are_ a stimulus response
machine ;-)

Bruce Gregory

Date: Sat, 21 Dec 1996 11:32:08 -0700
From: "T. B. Harms" <tbh@TESSER.COM>
Subject: Re: Driving blind; wearing coats

*From Tracy Harms 1996;12,21.11:30

RoboBruce Abbott (961221.1255 EST)

>As for me, I'm _not_ an autonomous perceptual control system, if by
>"autonomous" you mean "acting independently of physical causes," I'm a
>completely determinate machine, down to (but not including) the quantum
>level. I'm autonomous only in the sense of having internal references that
>determine which variables I will control at what levels.

I don't think Bruce G. brought up the question of the nature of autonomy in
relation to material cause, and it seems pointless for you to do so,

RoboBruce. CSGnet strikes me as a neutral ground where those who wish to entertain delusions of Laplacian determinism may join fruitfully in discussion of autonomy with vehement anti-deterministsts such as myself. Insofar as we all agree that autonomy shall be spoken of on this list as a property of perceptual control systems, there is no need to feud. Your jump to interpret it to mean "acting independently of physical causes" was unwarranted and inflamatory.

Tracy Bruce Harms tbh@tesser.com
Boulder, Colorado

Date: Sat, 21 Dec 1996 12:40:51 -0600
From: Bruce Abbott <abbott@CVAX.IPFW.INDIANA.EDU>
Subject: Re: Driving blind; wearing coats

[From Bruce Abbott (961221.1340 EST)]

>Bruce Gregory (961221.1310 EST) --

>>Bruce Abbott (961221.1255 EST)

>> That representation usually allows me to get from Point A to Point B without
>> a map, usually without a hitch, even if I've never followed that particular
>> route before. The reason is that disturbances of this variable are for all
>> practical purposes absent. Not once have I arrived in my neighborhood to
>> discover, much to my shock, that my house is no longer there. (California
>> residents may have a different experience!)

>I would say that you use your internal map to set reference levels,

And what is an internal map if not a model of some aspect of the external world?

>> As for me, I'm _not_ an autonomous perceptual control system, if by
>> "autonomous" you mean "acting independently of physical causes," I'm a
>> completely determinate machine, down to (but not including) the quantum
>> level. I'm autonomous only in the sense of having internal references that
>> determine which variables I will control at what levels.

>I can see that my explanation would make no sense to you. Your self
>image will also make it clear to Rick why he has so little success in
>communicating with you -- you really _are_ a stimulus response
>machine ;-)

Come'on, even us determinate machines are capable of thought! Give it a shot! Besides, you missed something: I did admit to being a perceptual control system. That's _hardly_ an S-R machine, would't you agree?

>>I, I, I. What is this "I" of which you speak? How does it set reference
>>levels? How does it know what reference levels will need to be set to
>>achieve its various goals? How does it know that it will probably be cold
>>outside, and that being exposed to the cold will bring about a discomfort it
>>wishes to avoid? How does it know that putting on a coat will help to
>>minimize the effect of this disturbance to comfort?

I missed your response to the above. You're not ducking the issue are you, you cagy country boy? (;->

Regards,

Bruce

Date: Sat, 21 Dec 1996 13:00:48 -0600
From: Bruce Abbott <abbott@CVAX.IPFW.INDIANA.EDU>
Subject: Re: Autonomy

[From Bruce Abbott (961221.1400)]

>I don't think Bruce G. brought up the question of the nature of autonomy in
>relation to material cause, and it seems pointless for you to do so,
>RoboBruce. CSGnet strikes me as a neutral ground where those who wish to
>entertain delusions of Laplacian determinism may join fruitfully in
>discussion of autonomy with vehement anti-determinists such as myself.
>Insofar as we all agree that autonomy shall be spoken of on this list as a
>property of perceptual control systems, there is no need to feud. Your
>jump to interpret it to mean "acting independently of physical causes" was
>unwarranted and inflamatory.

I didn't mean to be inflamatory; rather, I'm enquiring into Bruce Gregory's understanding of HPCT as a scientific construct. I did not jump to interpret "autonomy" to mean "acting independently of physical causes," I merely asked Bruce _whether_ this is what he meant by it.

This is the second time you have attempted to control my behavior with respect to posts on CSGnet. I leave it to you to judge what implications this has for your philosophy.

Regards,

Bruce

Date: Sat, 21 Dec 1996 12:08:14 +0000
From: Richard marken <marken@AERO.ORG>
Subject: Control of perception

[From Rick Marken (961221.1100)]

Me:

>The environmental variable in a control loop is controlled. Try it
>and see for yourself.

Bruce Abbott (961220.1810 EST)--

> Well, sure had ME fooled! Guess lil' ol' me was misled by the name >of the
theory -- _perceptual_ control theory -- into the mistaken
>belief that _perceptions_ are under control.

Yes, you have been fooled. But I think you have managed to fool yourself. It's all part of your efforts to see PCT as compatible with the fundamental cause-effect assumptions of conventional experimental psychology. If you would stop trying to control for such compatibility,

you would be _there_ -- in spades.

At the risk (or hope;-)) of sounding condescending, I will explain why _control of perception_ does not mean _no control (or accidental "stabilization", to use Martin's felicitous term, which you liked so much) of environmental variables_. I am developing a demo to illustrate my point. When it is completed (hopefully by tommorrow) I'll give you the lecture. I'm sure you'll want to be there;-)

Best

Rick

Date: Sat, 21 Dec 1996 12:18:01 -0700
From: Bill Powers <powers_w@FRONTIER.NET>
Subject: Re: Lies, damned lies and non-control

--=====================_851221081==_
Content-Type: text/plain; charset="us-ascii"

[From Bill Powers (961221.1200 MST)]

Bruce Gregory (961221.1300 EST)]
>
>> Hi, David --
>>
>> I'm sending you my latest revision of Bruce's program as an executable
>>file, attached to this post. Hope it works.
>> Best,
>> Bill P.
>>
>This went out to everybody, Bill. Bruce's program ran, but so fast that
>I couldn't make sense of it. Either I'm too slow (very likely) or my 133
>MHz Pentium is too fast.

Software, thy name is incompatibility.

I meant it to go to everybody, since some people can't compile the source code. I didn't mean the program to leave the screen in graphics mode (thanks, Bruce A.), or to run too fast.

In this version, you need a command line argument that tells how many milliseconds to delay during each iteration. To make it slow, use a number like 50; 10 works on my machine. If it's too fast, just hit Esc to exit and try it with a larger number.

The command for a delay of 50 looks like this:

runblnd3 50

This version has somewhat higher control gain, and you can also toggle control on and off using 'c'. When control is off, the output of the control system is simply clamped to zero, so you can see the effect the disturbances would have on CV and CVm in the absence of control.

Best,

Bill P.

--=====================_851221081==_
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Date: Sat, 21 Dec 1996 12:47:03 -0700
From: Bill Powers <powers_w@FRONTIER.NET>
Subject: Re: Driving blind; wearing coats

[From Bill Powers (961221.1215 MST)]

Bruce Abbott (961221.1255 EST)]

>I, I, I. What is this "I" of which you speak? How does it set reference
>levels? How does it know what reference levels will need to be set to
>achieve its various goals? How does it know that it will probably be cold
>outside, and that being exposed to the cold will bring about a discomfort
>it wishes to avoid? How does it know that putting on a coat will help to
>minimize the effect of this disturbance to comfort?

That's what we're trying to model, isn't it?

>As for me, I'm _not_ an autonomous perceptual control system, if by
>"autonomous" you mean "acting independently of physical causes," I'm a
>completely determinate machine, down to (but not including) the quantum
>level. I'm autonomous only in the sense of having internal references
>that determine which variables I will control at what levels.

Assuming that we're completely determinate machines doesn't help us much when most of the "determining" processes occurred anywhere up to 3.8 billion years ago. We are certainly not "determinate machines" with respect to the _present_ physical world. It is not the present physical world that tells us that pain hurts, pleasure feels good, and sex is fun.

Furthermore, it is perfectly possible that some of the internal processes we have accumulated through the course of evolution contain indeterminacies. A reorganizing system needs a source of variation that is not systematic; such a source could be provided in many ways, for example a detector of cosmic rays (our eyes can detect them) or simply a high-gain neural amplifier with its input set so close to threshold that thermal agitation produces impulses that are, for any practical purposes, randomly distributed.

Perhaps you're saying that you don't believe there are any causal factors in behavior that can't in principle be traced back to the physical world (the standard behavioristic assumption, I presume). If this is what you're saying, I have to ask which physics you're talking about -- the physics of 1400 AD, 1750, 1850, 1996, or 2996. I, for one, am quite sure there there is a physical explanation for everything. However, I am not so sure that physics (1996) contains all those explanations. For example, I'm still waiting for someone to explain consciousness, or better, awareness. Physics isn't quite up to that yet, is it? I'm sure it will be able to explain such things some day, but I doubt that physics will then much resemble our present understanding of it.

Best,

Bill P.

Date: Sat, 21 Dec 1996 13:54:13 -0600
From: Bruce Abbott <abbott@CVAX.IPFW.INDIANA.EDU>
Subject: Re: ECT

[From Bruce Abbott (961221.1455 EST)]

>Rick Marken (961221.1100)

>At the risk (or hope;-)) of sounding condescending, I will explain
>why _control of perception_ does not mean _no control (or accidental
>"stabilization", to use Martin's felicitous term, which you liked so
>much) of environmental variables_. I am developing a demo to illustrate
>my point. When it is completed (hopefully by tommorrow) I'll give you
>the lecture. I'm sure you'll want to be there;-)

That will be me in the front row, center seat.

But let me guess at what your lecture will have to say about environmental control theory (ECT). I'm guessing it will say that, so long as p is a stable function of CV, controlling p will control CV, too. That is, CV will be maintained at some reasonably stable value (depending on gain etc.) against disturbances to CV. If this is what you mean by "control," then the CV is controlled. However, note that the CV may be brought to a value that is not what is needed, due to an error in the perceptual input function. The temperature of the room is controlled, but not at the temperature you wanted.

However, if the level to which CV is brought is such that the controlled perception of the higher-level system (whose output defines the reference level for the lower-level system) is _not_ brought near its own reference level, then reorganization will take place until control at the higher level is restored. The reference for the lower-level p produced by a given higher-level error may be changed, or the input function may be altered; the result is that CV will be brought to right value that will permit the higher-level system to do its job. You (the higher-level system) will alter the way you set the thermostat so as to bring your perception of comfort near its own reference.

Just in case I've missed the boat (again!), please keep my seat reserved for the show. Some of us are mighty slow learners . . . (:->

Regards,

Bruce

Date: Sat, 21 Dec 1996 13:36:31 +0000
From: Richard marken <marken@AERO.ORG>
Subject: Re: ECT

[From Rick Marken (961221.1230)]

Bruce Abbott (961221.1455 EST) --

> But let me guess at what your lecture will have to say about
> environmental control theory (ECT). I'm guessing it will say...

Not even close;-)

>Some of us are mighty slow learners ... (:->

That's certainly not because of any shortage of intelligence on

your part!

Best

Rick

Date: Sat, 21 Dec 1996 15:18:13 -0600
From: Bruce Abbott <abbott@CVAX.IPFW.INDIANA.EDU>
Subject: Determinacy

[From Bruce Abbott (961221.1620 EST)]

>Bill Powers (961221.1215 MST) --

>>Bruce Abbott (961221.1255 EST)

>>I, I, I. What is this "I" of which you speak? How does it set reference
>>levels? How does it know what reference levels will need to be set to
>>achieve its various goals? How does it know that it will probably be cold
>>outside, and that being exposed to the cold will bring about a discomfort
>>it wishes to avoid? How does it know that putting on a coat will help to
>>minimize the effect of this disturbance to comfort?

>That's what we're trying to model, isn't it?

Yes, exactly.

>Assuming that we're completely determinate machines doesn't help us much
>when most of the "determining" processes occurred anywhere up to 3.8 billion
>years ago. We are certainly not "determinate machines" with respect to the
>_present_ physical world. It is not the present physical world that tells us
>that pain hurts, pleasure feels good, and sex is fun.

See below.

>Furthermore, it is perfectly possible that some of the internal processes we
>have accumulated through the course of evolution contain indeterminacies.

I rather expect that they do (although I would prefer the phrase "random
elements" to "indeterminacies," as random elements can be fully determined
by physical processes); however, that's not what I'm getting at.

>Perhaps you're saying that you don't believe there are any causal factors in
>behavior that can't in principle be traced back to the physical world (the
>standard behavioristic assumption, I presume). If this is what you're
>saying, I have to ask which physics you're talking about -- the physics of
>1400 AD, 1750, 1850, 1996, or 2996. I, for one, am quite sure there there is
>a physical explanation for everything.

That last line -- that's my viewpoint. It is not a viewpoint derived from
behaviorism, or even physics (no matter what the century). It is just that,
if we start with the assumption of _indeterminism_, then there is no reason
to believe that there are any physical explanations to be found -- things
just happen, and that's that. This is especially important for a science
dealing with human mind and behavior, where it is all too easy to fall back
on appeal to the homunculus -- the "little man in the head" -- for
"explanations." Having satisfied ourselves that we have our answer, we stop

looking for one.

I may personally believe that I possess an autonomous soul whose thoughts are not constrained by the physical interactions going on in my brain, but as a scientist I must assume the contrary; otherwise it makes no sense to search for the "causes" of behavior, either internally, in the workings of the brain, or externally. It may very well be that my thoughts, feelings, decisions emanate from an unconstrained "free will," which somehow sets the references for high-level physical systems of the brain, but for research purposes I should not allow such a belief to prevent me from attempting to find physical mechanisms to account for even the highest level actions. If free will is there, I will simply be unable to find such mechanisms at the highest levels.

Regards,

Bruce

Date: Sat, 21 Dec 1996 15:24:58 -0600
From: Bruce Abbott <abbott@CVAX.IPFW.INDIANA.EDU>
Subject: Re: ECT

[From Bruce Abbott (961221.1625 EST)]

>Rick Marken (961221.1230) --

>>Bruce Abbott (961221.1455 EST)

>> But let me guess at what your lecture will have to say about
>> environmental control theory (ECT). I'm guessing it will say...

>Not even close;-)

Ulp. I hope you saved my seat. . .

>>Some of us are mighty slow learners ... (:->

>That's certainly not because of any shortage of intelligence on
>your part!

You're not exactly slow, Rick. I'm running flat out just trying to keep up with you.

Regards,

Bruce

Date: Sat, 21 Dec 1996 15:03:32 -0700
From: "T. B. Harms" <tbh@TESSER.COM>
Subject: Re: Autonomy

*From Tracy Harms 1996;12,21.14:40

Bruce Abbott (961221.1400)

>This is the second time you have attempted to control my behavior with
>respect to posts on CSGnet. I leave it to you to judge what implications
>this has for your philosophy.
>
>Regards,
>
>Bruce

It seems to me that any disagreement could be (mis)interpreted as an attempt to control anothers behavior. Or is there something special about my remarks which somehow make it clear that this was my intent, whereas the objections of others are nonmanipulative?

If I were to put it in PCT terminology I'd say I was controlling for the peace of mind which comes with my imagining that readers of CSGnet encounter these discussions without thinking that PCT necessarily involves a deterministic interpretation.

If you *desire* to go head-to-head over this, perhaps I'll join in. My point was that I think such a matter is outside of PCT to a degree that it would be considered unproductive for this list. If you think that topic is productive to better understanding PCT, make a claim along those lines, or criticize my assertions of compatability. My philosophy is not authoritarian, my motives were not manipulative. I simply reject the interpretation of autonomy you brought before us, and deny the relevance to PCT of disputes over determinism. In this I may have erred, but not for attempting to control your behavior. If you still think that was my motive, significant persuasion remains to be done.

Tracy Bruce Harms tbh@tesser.com
Boulder, Colorado

Date: Sat, 21 Dec 1996 18:18:38 -0500
From: Bruce Gregory <gregory@CF4.HARVARD.EDU>
Subject: Re: Driving blind; wearing coats

[From Bruce Gregory (961221.1815 EST)]

Bruce Abbott (961221.1340 EST)
>
> >Bruce Gregory (961221.1310 EST) --
>
> >I would say that you use your internal map to set reference levels,
>
> And what is an internal map if not a model of some aspect of the external world?

An internal map is certainly a model of the world. The debate _seems_ to be about the role that such models play in control. My position is that we use them to help set reference levels.

> Come'on, even us determinate machines are capable of thought! Give it a
> shot! Besides, you missed something: I did admit to being a perceptual

> control system. That's _hardly_ an S-R machine, would't you agree?

True. You are just more like my thermostat then I realized.

> >>I, I, I. What is this "I" of which you speak? How does it set reference
> >>levels? How does it know what reference levels will need to be set to
> >>achieve its various goals? How does it know that it will probably be cold
> >>outside, and that being exposed to the cold will bring about a discomfort
it
> >>wishes to avoid? How does it know that putting on a coat will help to
> >>minimize the effect of this disturbance to comfort?
>
> I missed your response to the above. You're not ducking the issue are you,
> you cagy country boy? (;->

I'll go with Dad's answer. (There I am, currying favor again ;-))

Bruce Gregory

Date: Sat, 21 Dec 1996 18:25:58 -0500
From: Bruce Gregory <gregory@CF4.HARVARD.EDU>
Subject: Re: Lies, damned lies and non-control

 [From Bruce Gregory (961221.1825 EST)]

Bill Powers (961221.1200 MST)]
>
> I meant it to go to everybody, since some people can't compile the source
> code. I didn't mean the program to leave the screen in graphics mode
> (thanks, Bruce A.), or to run too fast.

The lack of a header misled me.

> In this version, you need a command line argument that tells how many
> milliseconds to delay during each iteration. To make it slow, use a number
> like 50; 10 works on my machine. If it's too fast, just hit Esc to exit and
> try it with a larger number.

Thanks. It looks fine with 100 as the delay on my machine.

Bruce Gregory

Date: Sat, 21 Dec 1996 17:47:14 -0600
From: Bruce Abbott <abbott@CVAX.IPFW.INDIANA.EDU>
Subject: Re: Autonomy

[From Bruce Abbott (961221.1850 EST)]

>Tracy Harms 1996;12,21.14:40 --

>It seems to me that any disagreement could be (mis)interpreted as an
>attempt to control anothers behavior. Or is there something special about
>my remarks which somehow make it clear that this was my intent, whereas the

>objections of others are nonmanipulative?

It seems to me that there is an important distinction to be made between criticizing one's ideas and one's actions. I don't mind -- indeed I welcome -- the former, but the latter represents an infringement on my autonomy -- an attempt to control by behavior through contingent scolding words. The Skinnerian term for it is "punishment." You may not have intended your words to be perceived in this way, but that is how you came across to me.

Put another way, when you attack my ideas, you do not attack me. When you attack my behavior, your attack is directed against me, personally, for I am the originator of those actions.

Furthermore, no one, to my knowledge, has made you the gatekeeper of CSGnet. If no one wishes to discuss a topic that is raised, it dies for lack of nourishment; there is generally no need for self-appointed censors to "police" the posts. If anyone doesn't like what I or anyone else on CSGnet has to say, they have a simple option available to them: don't read the posts of those people.

>If you *desire* to go head-to-head over this, perhaps I'll join in. My
>point was that I think such a matter is outside of PCT to a degree that it
>would be considered unproductive for this list. If you think that topic is
>productive to better understanding PCT, make a claim along those lines, or
>criticize my assertions of compatability. My philosophy is not
>authoritarian, my motives were not manipulative. I simply reject the
>interpretation of autonomy you brought before us, and deny the relevance to
>PCT of disputes over determinism. In this I may have erred, but not for
>attempting to control your behavior. If you still think that was my
>motive, significant persuasion remains to be done.

Tracy, I'm quite willing to accept that your intent was as you describe. I hope you understand that my intent was not to inflame passions, but to point out that commonsense answers in terms of "I" do not provide satisfactory _scientific_ explanations, but rather, only raise more questions.

As for our going "head to head" on this topic, I've already said everything I have to say about it in my piece entitled "Determinacy" (961221.1620 EST). But if you'd like to talk about it privately (which seems appropriate given your feelings about its relevance to PCT), I wouldn't mind hearing you out.

Regards,

Bruce

Date: Sat, 21 Dec 1996 23:42:18 -0500
From: David <dgoldstein@P3.NET>
Subject: Bruce's runblind compiled program

From: David Goldstein
To: Bruce Abbott
Date: 12/21/96
Subject: Bruce's runblind compiled program

The program worked fine. Thanks Bruce.

What I saw was that the main difference between the blind versus sighted

condition was in the variability of the actual environmental variable. It was greater under the blind than the sighted condition.

The controlled perceptual variable line seemed to stay about the same under both conditons.

The output variable and disturbance curves seemed to be just as out of phase under both conditions.

So it seems that the person would be convinced that the subjective sense of control would be the same under both conditions but that the objective viewpoint would be different. Is this an example of an illusion?

End of CSGNET Digest - 21 Dec 1996 to 22 Dec 1996
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Date: Mon, 23 Dec 1996 08:01:40 +1800
Subject: CSGNET Digest - 22 Dec 1996 to 23 Dec 1996

There are 18 messages totalling 1832 lines in this issue.

Topics of the day:

1. Driving blind; wearing coats (2)
2. Bruce's runblind compiled program
3. Lecture and demo (3)
4. Autonomy (7)
5. RunBlnd4.pas -- human experiment
6. Fw: Driving blind; wearing coats (3)
7. PCT

Date: Sun, 22 Dec 1996 10:47:33 -0600
From: Bruce Abbott <abbott@CVAX.IPFW.INDIANA.EDU>
Subject: Re: Driving blind; wearing coats

[From Bruce Abbott (961222.1150 EST)]

>Bruce Gregory (961221.1815 EST) --

>>Bruce Abbott (961221.1340 EST)

>I would say that you use your internal map to set reference levels,

>> And what is an internal map if not a model of some aspect of the external
>world?

>An internal map is certainly a model of the world. The debate _seems_
>to be about the role that such models play in control. My position is
>that we use them to help set reference levels.

Apparently I misunderstood you: I thought your position was that models are not used at all. I can easily imagine a two-level system in which the upper-level uses its model (when information is not available directly from sensors) to set the lower-level reference. Is this what you have in mind?

>> Come'on, even us determinate machines are capable of thought! Give it a
>> shot! Besides, you missed something: I did admit to being a perceptual
>> control system. That's _hardly_ an S-R machine, would't you agree?

>True. You are just more like my thermostat then I realized.

That's a bit like calling Intel's new teraflop parallel supercomputer a two-dollar calculator. True, both are built on the same principles, but . . . And the human brain makes the today's most advanced supercomputers look like counting-stones by comparison. Also, the true comparison would be with the whole heating system, not the thermostat. We all come equipped with such a system, by the way; the comparator is located in the hypothalamus, whose outputs run both to the autonomic nervous system and to the pituitary gland (the so-called "master endocrine gland"). The system usually does an excellent job of keeping core body temperature near its reference value.

>> I missed your response to the above. You're not ducking the issue are you,
>> you cagy country boy? (;->

>I'll go with Dad's answer. (There I am, currying favor again ;-))

Always a good tactic!

Regards,

Bruce

Date: Sun, 22 Dec 1996 11:14:20 -0600
From: Bruce Abbott <abbott@CVAX.IPFW.INDIANA.EDU>
Subject: Re: Bruce's runblind compiled program

[From Bruce Abbott (961222.1215 EST)]

>David Goldstein (21 Dec 1996 23:42:18) --

>The program worked fine. Thanks Bruce.

Glad to hear that.

>What I saw was that the main difference between the blind versus sighted
>condition was in the variability of the actual environmental variable.
>It was greater under the blind than the sighted condition.

Yes. In the blind condition, the controller can only counteract the predictable (modeled) portion of the disturbance. Under the best possible condition, in which the entire disturbance is accurately modeled, the system's operation when running blind or sighted would be indistinguishable. As the proportion of variation in the disturbance that is accurately modeled decreases, the variability of the environmetal variable (CV) becomes correspondingly greater.

>The controlled perceptual variable line seemed to stay about the same
>under both conditons.

Yes. That is because the controlled preception remains under control at all times, whether the controller is in sighted or blind mode. In sighted mode,

the controlled perception = CV; in blind mode it equals CVm.

>The output variable and disturbance curves seemed to be just as out of
>phase under both conditions.

Yes. Remember, the output acts to counter the disturbance, so (at least in the sighted condition) it will be a near mirror image of the disturbance. In the blind condition, the output will be a near mirror image of the modeled portion of the disturbance.

>So it seems that the person would be convinced that the subjective sense
>of control would be the same under both conditions but that the
>objective viewpoint would be different. Is this an example of an
>illusion?

I suppose this would be true if there were no way for the person to tell that normal input was lost, but of course this normally is far from the case. It's usually pretty clear what is "real" and what is imaginary. A person who was unable to distinguish the two would indeed have the illusion that control was being maintained in the blind condition.

Regards,

Bruce

Date: Sun, 22 Dec 1996 11:19:14 -0600
From: Bruce Abbott <abbott@CVAX.IPFW.INDIANA.EDU>
Subject: Re: Lecture and demo

[From Bruce Abbott (961222.1220 EST)]

>Rick Marken (961221.1100) --

>I am developing a demo to illustrate
>my point. When it is completed (hopefully by tommorrow) I'll give you
>the lecture. I'm sure you'll want to be there;-)

Will the show be starting soon? I'm running out of popcorn . . .

Regards,

Bruce

Date: Sun, 22 Dec 1996 10:50:47 -0700
From: "T. B. Harms" <tbh@TESSER.COM>
Subject: Re: Autonomy

*From Tracy Harms 1996;12,22.10:47

Bruce Abbott (961221.1850 EST)

I've reconsidered my hot retort, especially where I called your choice inflamatory. This did go beyond criticism either of your ideas or your actions, and impugned your motives. I apologize. (There is a difference between our standards regarding criticism of action, which you see as an

infringement on autonomy whereas I don't. Nevertheless, I strayed beyond even those bounds.)

I have also been reconsidering whether and how the general questions of causal regularity apply to HPCT. The difference still seems to be subtle enough that I think practical questions can be addressed despite huge disagreement on that matter, but I'm now seeing that opinion here will make an enormous difference at some point where implications become consequential to action. I think somebody (Rick?) already made a call along these lines: an important difference between non-deterministic and deterministic cosmological views is whether HPCT explains autonomy, or merely the *illusory impression of* autonomy. In light of that I must retract my principal statement as well; this dispute *is* relevant to this list.

Tracy Bruce Harms tbh@tesser.com
Boulder, Colorado

Date: Sun, 22 Dec 1996 11:30:49 +0000
From: Richard marken <marken@AERO.ORG>
Subject: Re: Lecture and demo

[From Rick Marken (961222.1030)]

Bruce Abbott (961222.1220 EST) --

> Will the show be starting soon? I'm running out of popcorn . . .

Welcome to the breakfast show;-)

OK. The "Control of Perception" Demo is completed (http://www.leonardo.net/Marken/ControlDemo/ControlP.html) so here is my take on why we say "Behavior: The control of perception" rather than "Behavior: The control of the environment".

I assume that we all know why we say that behavior is the control of _something_. I also assume that we all (with the possible exception of Hans) know that it's not _output_ that is controlled. But why do we say that the “something” we control is perception rather than the environment that most of us are comfortably confident that perception is a function of?

Bruce Abbott (961221.1455 EST) guessed that its because

>the CV [environmental correlate of a controlled perception]
>may be brought to a value that is not what is needed, due to
>an error in the perceptual input function.

This is certainly a possibility. The observer could guess that the system is controlling its perceptions if the system explicitly said “I’m trying to control CV (a particular environmental variable)” and the observer, measuring CV, could see that CV was not under control. But this assumes that the system can accurately report on the assumed environmental correlates of its own perceptions. I don’t think we have to make this assumption in order to show that behavior is the control of perception.

I think that we can see that behavior is the control of perception (not the environment) because we can see organisms controlling _different_ perceptual representations of the _same_ environmental state of affairs. Of course, the “environmental state of affairs” is a _model_ of the environment; this model is _part_ of the PCT model; the part that is in the loop between output and input. It is the physics model of what is “out there” causing our perceptions. I think that model is so accurate that it is safe to talk about it as if it _were_ the environment.

In my demo, the environment is a quadrilateral arrangement of four lines. I will refer to this environment as though it consists of two variables; x (the length of the horizontal lines) and y (the length of the vertical lines). Physics tells me that these lines are really strings of varying numbers of equal sized pixels and I’m confident that the values of x and y are reasonably accurate measures of the length of the strings of pixels that are _really_ out there.

In this demo, the subject can control any one of three _perceptual_ representations of an environmental state of affairs (the quadrilateral arrangement of x and y). The subject can control the width of the quadrilateral (x), the size (area) of the quadrilateral (x*y) or the shape of the quadrilateral (x/y). I have set up the demo so that all three variables are affected by disturbances that can be offset by the subjects actions. For example, x = o + d (where o is the subject’s output, which is proportional to mouse position and d is a time varying disturbance). In order to control width x (keep it constant) the subject must vary o so that o = -d. Since y varies independent of the subject’s actions it is a disturbance too. In order to control x*y the subject must vary o so that o is proportional to (1/y)-d.

The demo shows that the subject can control any one of these three perceptual representations of the _same_ objective situation _at will_. Some perceptions (like shape) can be controlled more skillfully than others (like area). But all three perceptions can be controlled. You can see this in a graph of the state of the three perceptual variables over time. The controlled perceptual variable remains relatively stable while the uncontrolled variables vary all over the place.

Note that all three perceptual representations are still “there” even when they are not controlled. The quadrilateral still has area (x*y) when the subject is controlling shape (x/y). There are an infinite number of other perceptual aspects of the quadrilateral (besides the three calculated in the demo) that _could_ be controlled; indeed, any function of two variables (x,y) is a possible controlled perceptual variable. My guess, however, is that people can’t perceive (and, hence, control) most of the possible perceptual representations of the quadrilateral -- functions like ax^3/y, say.

Note also that when we control a perceptual representation of the environment we are also controlling the aspects of the environment that correspond to that perceptual representation. When we control x/y (keeping it 1.0 -- a square) the lines of the quadrilateral remain in a square. Is the square shpae “really”

there? I would say “no”. What is “really” there is strings of pixels -- x and y; x/y = 1.0 is just one way of representing this (modeled) reality. The square shape doesn’t exist until a system that can represent shape (x/y) as a perceptual variable and bring it to the reference state (1.0 = “square”).

The demo illustrates a point that Bill Powers has made in many contexts: we don’t control things (like cars, houses, people etc.); we control variable perceptual _aspects_ of things; the color or speed of the car; the location or shape of the house, the politeness or ideas expressed by people. Behavior is the control of perception. But the environment is in the control loop; and the environmental reality that corresponds to computed perceptions is also being controlled.

Best

Rick

Date: Sun, 22 Dec 1996 12:16:08 -0700
From: Bill Powers <powers_w@FRONTIER.NET>
Subject: RunBlnd4.pas -- human experiment

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Content-Type: text/plain; charset="us-ascii"

[From Bill Powers (961222.1100 MST)]

The attached executable program for PC compatibles (MIME encoded) is a first version of a tracking experiment to test the idea that human beings can model disturbances and maintain an approximately correct output for some time after the controlled variable becomes invisible.

The basic task is to maintain a cursor underneath a stationary target mark, while a slow sine-wave of constant frequency and amplitude disturbs the cursor. Left and right movements only of the mouse are required. The experiment is intended to last about one minute. Halfway through the experiment, the cursor is blanked out; the subject is asked to try to keep moving the mouse in a way that would keep the cursor under the target if it were visible. The run is terminated automatically and the results are displayed.

I recommend running this program under the DOS prompt for Windows machines. The speed of the program is set by using a command-line argument that specifies an extra delay in milliseconds inserted on each iteration. On my machine I use a value of 35, which makes the run last close to 1 minute. Faster machines will need a larger value, slower machines a smaller one. The program is started by the command

runblnd3 xx

where xx is the value of the delay to be used. I recommend running the program several times to find the right value for your machine to get about a 1-minute run. You can just space through the mouse calibration sequence when adjusting timing -- if the mouse doesn't move a default calibration is used.

WHAT TO LOOK FOR:

There are four full sine waves in each 30-second half of the experiment. In the data display, time runs from left to right. The red trace shows handle position, the green trace the disturbance. When you're tracking well, these two traces look equal and opposite. The white trace shows the tracking error (how far the cursor is from the target at all times).

At the center is a vertical line separating the cursor-visible condition from the cursor-invisible condition. If the subject is using an accurate internal simulation of the disturbance, the handle movements should simply continue as before, opposing the disturbance, with the error remaining about the same. The usefulness of an internal world-model or simulation will depend on how accurately the person can model the disturbance.

As you do this experiment, try to see what you're paying attention to in both the visible and invisible conditions. What information are you conscious of using during the tracking process? When the cursor is not visible, are you imagining a moving cursor? Is there any difference in the amount of attention you pay to your arm, hand, and mouse in the two conditions? Any other comments on what you experience during this experiment will be appreciated.

It is to be expected that your performance will improve with practice, so please do as many runs as you can stand to do before you think about evaluating your performance. It would probably be a good idea to use spaced practice, doing a series of runs, going away and doing something else for a while, and coming back at intervals to do more. Whatever you say about the results should be said after you're reasonably sure you won't improve a great deal more.

If anyone wants to make a research project out of this, I can add a routine to the program that will output the data as an ASCII file which could be used as input to any analysis program.

Best,

Bill P.

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Date: Sun, 22 Dec 1996 13:19:40 -0600
From: Bruce Abbott <abbott@CVAX.IPFW.INDIANA.EDU>
Subject: Re: Autonomy

[From Bruce Abbott (961222.1420 EST)]

>Tracy Harms 1996;12,22.10:47 --

>I've reconsidered my hot retort, especially where I called your choice
>inflamatory. This did go beyond criticism either of your ideas or your
>actions, and impugned your motives. I apologize. (There is a difference
>between our standards regarding criticism of action, which you see as an
>infringement on autonomy whereas I don't. Nevertheless, I strayed beyond
>even those bounds.)

Apology accepted. (:->

>I have also been reconsidering whether and how the general questions of
>causal regularity apply to HPCT. The difference still seems to be subtle
>enough that I think practical questions can be addressed despite huge
>disagreement on that matter, but I'm now seeing that opinion here will make
>an enormous difference at some point where implications become
>consequential to action. I think somebody (Rick?) already made a call
>along these lines: an important difference between non-deterministic and
>deterministic cosmological views is whether HPCT explains autonomy, or
>merely the *illusory impression of* autonomy. In light of that I must
>retract my principal statement as well; this dispute *is* relevant to this
>list.

Do you wish to discuss this issue? I'd be interested to hear what you have
to say along those lines.

Regards,

Bruce

Date: Sun, 22 Dec 1996 16:12:43 -0500
From: Bruce Gregory <gregory@CF4.HARVARD.EDU>
Subject: Re: Driving blind; wearing coats

[From Bruce Gregory (961222.1615 EST)]

Bruce Abbott (961222.1150 EST)]

> Apparently I misunderstood you: I thought your position was that models are
> not used at all. I can easily imagine a two-level system in which the
> upper-level uses its model (when information is not available directly from
> sensors) to set the lower-level reference. Is this what you have in mind?

Yes.

> >True. You are just more like my thermostat then I realized.
>
> That's a bit like calling Intel's new teraflop parallel supercomputer a
> two-dollar calculator. True, both are built on the same principles, but . .

Date: Sun, 22 Dec 1996 18:26:24 -0500
From: Bruce Gregory <gregory@CF4.HARVARD.EDU>
Subject: Fw: Driving blind; wearing coats

Apparently the Internet butchered this so I'm trying again.

[From Bruce Gregory (961222.1615 EST)]

Bruce Abbott (961222.1150 EST)]

> Apparently I misunderstood you: I thought your position was that models are
> not used at all. I can easily imagine a two-level system in which the
> upper-level uses its model (when information is not available directly from
> sensors) to set the lower-level reference. Is this what you have in mind?

Yes.

> >True. You are just more like my thermostat then I realized.
>
> That's a bit like calling Intel's new teraflop parallel supercomputer a
> two-dollar calculator. True, both are built on the same principles, but . .
.
> And the human brain makes the today's most advanced supercomputers look like
> counting-stones by comparison. Also, the true comparison would be with the
> whole heating system, not the thermostat. We all come equipped with such a
> system, by the way; the comparator is located in the hypothalamus, whose
> outputs run both to the autonomic nervous system and to the pituitary gland
> (the so-called "master endocrine gland"). The system usually does an
> excellent job of keeping core body temperature near its reference value.

I likened you to my thermostat because something _outside_ the heating
system sets the reference value for the temperature -- me. The thermostat
is _not_ part of an autonomous control system. You tell me this is true of
you as well. Having a direct experience of my ability to set reference
levels, I am compelled to admit that I _am_ autonomous. You must have
a very different experience than I do, or you are letting your model
invalidate your experience ;-)

Bruce Gregory

Date: Sun, 22 Dec 1996 21:13:30 -0500
From: Bruce Abbott <abbott@CVAX.IPFW.INDIANA.EDU>
Subject: Autonomy

[From Bruce Abbott (961222.2110 EST)]

>Bruce Gregory (961222.1615 EST) --

> I likened you to my thermostat because something _outside_ the heating
> system sets the reference value for the temperature -- me. The thermostat
> is _not_ part of an autonomous control system. You tell me this is true of
> you as well. Having a direct experience of my ability to set reference
> levels, I am compelled to admit that I _am_ autonomous. You must have

> a very different experience than I do, or you are letting your model
> invalidate your experience ;-)

I'm sure I feel just as autonomous as you do. But when you decide to set some reference level, why (not to mention how) do you do it? According to HPCT, you do it because your history of experience has resulted in the building of certain control systems which in the past have been reasonably successful in stabilizing your "intrinsic" variables. You may have learned many alternative ways to keep your various controlled variables near their reference levels, and it may be little more than what is more convenient, or what is possible at the time, or even "noise" in the neural circuitry, that results in a given high-level control system setting one lower-level reference over another at any given moment. The system has a lot of choices; determinism does not imply that there is one and only one course of action open to a system at any given time, but only that the choices made are themselves the products of physical interactions. I can feel that those choices were made autonomously, simply because I have no other conscious experience that informs me of the physical interactions whereby those choices were determined, in the same way that I have no idea how I manage to fetch up a memory. Both the decision and the memory just seem to happen because I will them, but that does not prove that they happen without physical cause.

Regards,

Bruce

Date: Sun, 22 Dec 1996 22:02:38 -0700
From: "T. B. Harms" <tbh@TESSER.COM>
Subject: Re: Autonomy

*From Tracy Harms 1996;12,22.22:00

Bruce Abbott (961222.2110 EST)

It appears that you hold a presumption that genuine autonomy implies a source or manner of change somehow outside the bounds of physical contingency. I do not share this supposition. My initial guess is that this reflects a difference in conceptualizing the nature of nature.

The scientific traditions have tended to teach causality in terms of positive laws, i.e. in terms of processions of specific transformations. In contrast with this I think of things in terms of the metaphor of prohibitory laws, and thus a nature of limits which constrain what occurs from a manifold of impossible states. But within these limits, what occurs is spontaneous -- and local in its spontaneity. For me there is no exception to the status of quantum mechanics, nor does indeterminacy at larger scales occur only by means of a ripple-effect from sub-atomic quantum states. Rather, quantum indeterminacy is indicative of universally pervasive indeterminacy which has unique properties at every scale.

At the scale of our bodies, in the context of our nervous systems and our literate culture, one property is human autonomy as we know it: People are genuinely self-directed. The choices we make within our personhood are, like all changes, constrained by the historical limits of physical reality. But within those limits, what we become is an artifact of our unique,

independent, immediate being. The autonomy this involves is as rich, satisfying, and frightening as any vision of human autonomy imagined to date. It means that we each truly do make a difference, and that personal choice by strength of will is a product of individual identity.

Perhaps what I've written here is closer to poetry than to methodical explanation, but I hope it serves to give you some sense of my thoughts on this topic.

Tracy Bruce Harms tbh@tesser.com
Boulder, Colorado caveat lector!

Date: Mon, 23 Dec 1996 01:42:29 -0700
From: Bill Powers <powers_w@FRONTIER.NET>
Subject: Re: Fw: Driving blind; wearing coats

[From Bill Powers (961223.0100 MST)]

Bruce Gregory (961222.1615 EST)--

>Having a direct experience of my ability to set reference
>levels, I am compelled to admit that I _am_ autonomous.

Autonomy isn't that easily handled. If you are compelled by evidence to admit that you are autonomous, you're demonstrating that your view is a logical conclusion, and thus forced by your premises and your experiences -- not by "you."

This is a peculiarity of rational argument. When you try to demonstrate rationally that something is true, you're saying, in effect, that anyone presented with the same facts would have to agree with you. But if that is so, you're saying that no rational person has a choice about what to believe: once the observations and the logic have been understood, the conclusion is foregone and no longer a matter of free choice. So where is your autonomy then? It's simply the computer in your head that reasons as it must, and its conclusions are out of your control.

Autonomy can't be understood in terms of just one or two levels of perception. In fact I don't think it can be understood until you've identified your own levels of perception in some kind of detail, so you can see that the logical, reasoning part of your mind is not the highest level that there is.

You say "I am compelled to admit that I am autonomous." To see that this is not your highest level of organization speaking, all you have to do is ask yourself what you think about this conclusion. Is being compelled to admit that you're autonomous OK with you? Would it be less acceptable if reason compelled you to go along with Bruce Abbott when he says we are NOT autonomous?

The question then becomes, "What is satisfied, or not satisfied, by concluding that human beings are autonomous?" The conclusion itself now becomes unimportant; whether I'm speaking to you or Tracy or Bruce Abbott (I'm addressing all of you), the question is the same. Wherever your line of reasoning leads you with respect to autonomy, you have to decide whether this reasoning is valid or invalid, and whether the conclusion is satisfactory or unsatisfactory. And in making that decision, you have to

consider something ABOUT the reasoning process, something that is a different kind of reason.

There's no way to answer the kind of question I'm posing without basically changing the subject. You must, in fact, go up a level. What happens when you look at it that way?

Best,

Bill P.

Date: Mon, 23 Dec 1996 06:46:58 -0500
From: Bruce Abbott <abbott@CVAX.IPFW.INDIANA.EDU>
Subject: Re: Autonomy

[From Bruce Abbott (961223.0645 EST)]

>Tracy Harms 1996;12,22.22:00 --

Re Autonomy: thanks. I have a much better appreciation of your position now.

>It appears that you hold a presumption that genuine autonomy implies a
>source or manner of change somehow outside the bounds of physical
>contingency. I do not share this supposition. My initial guess is that
>this reflects a difference in conceptualizing the nature of nature.

Quite possibly true, although it is also possible that we hold somewhat different definitions of "autonomy."

>The scientific traditions have tended to teach causality in terms of
>positive laws, i.e. in terms of processions of specific transformations.
>In contrast with this I think of things in terms of the metaphor of
>prohibitory laws, and thus a nature of limits which constrain what occurs
>from a manifold of impossible states. But within these limits, what occurs
>is spontaneous -- and local in its spontaneity. For me there is no
>exception to the status of quantum mechanics, nor does indeterminacy at
>larger scales occur only by means of a ripple-effect from sub-atomic
>quantum states. Rather, quantum indeterminacy is indicative of universally
>pervasive indeterminacy which has unique properties at every scale.

What empirical evidence supports this belief in a "universally pervasive indeterminacy which has unique properties at every scale"? I am fairly well versed in the current view of the universe as seen at the atomic level and below, with its virtual particles and quantum fluctuations and all, but it seems to require very special conditions to observe quantum effects at the macroscopic level. I would grant you that indeterminacy at the subatomic level rules out predestiny, but that to my mind is not the same as ruling in autonomy. When I try to adopt your viewpoint, I am left wondering why autonomy shouldn't be granted to any complex system -- the atmosphere, for example.

>At the scale of our bodies, in the context of our nervous systems and our
>literate culture, one property is human autonomy as we know it: People are
>genuinely self-directed.

To my mind, self-directed is not the same as autonomous. The cruise control on my car is self-directed. Once I switch it on and set the speed, it

decides from moment to moment what position to hold the throttle at, not me. But perhaps this is what you mean by "autonomous."

>The choices we make within our personhood are,
>like all changes, constrained by the historical limits of physical reality.
>But within those limits, what we become is an artifact of our unique,
>independent, immediate being.

You'll get no argument from me on that point. What we become is a product of uncountable accidents which have given us our unique genetic structures, developmental histories, and experiences. But I have the feeling you mean something more than that.

>The autonomy this involves is as rich,
>satisfying, and frightening as any vision of human autonomy imagined to
>date. It means that we each truly do make a difference, and that personal
>choice by strength of will is a product of individual identity.

I agree that we each truly do make a difference, but I would argue that strength of will is itself a product of brain organization as given by those same genetic structures, developmental histories, and experiences. Why have you chosen to adopt this particular viewpoint? The very question suggests that this choice was not truly free.

>Perhaps what I've written here is closer to poetry than to methodical
>explanation, but I hope it serves to give you some sense of my thoughts on
>this topic.

Yes, thank you again. I appreciate the poetry.

Regards,

Bruce

Date: Mon, 23 Dec 1996 06:39:03 -0500
From: Bruce Gregory <gregory@CF4.HARVARD.EDU>
Subject: Re: Autonomy

[From Bruce Gregory (961223.0640 EST)]

Bruce Abbott (961222.2110 EST)
>
> I'm sure I feel just as autonomous as you do.

I'm relieved ;-)

> But when you decide to set
> some reference level, why (not to mention how) do you do it? According to
> HPCT, you do it because your history of experience has resulted in the
> building of certain control systems which in the past have been reasonably
> successful in stabilizing your "intrinsic" variables. You may have learned
> many alternative ways to keep your various controlled variables near their
> reference levels, and it may be little more than what is more convenient, or
> what is possible at the time, or even "noise" in the neural circuitry, that
> results in a given high-level control system setting one lower-level
> reference over another at any given moment. The system has a lot of
> choices; determinism does not imply that there is one and only one course of

> action open to a system at any given time, but only that the choices made
> are themselves the products of physical interactions. I can feel that those
> choices were made autonomously, simply because I have no other conscious
> experience that informs me of the physical interactions whereby those
> choices were determined, in the same way that I have no idea how I manage to
> fetch up a memory. Both the decision and the memory just seem to happen
> because I will them, but that does not prove that they happen without
> physical cause.

I'm sorry that I left the impression that I think that autonomous actions have no causes. I prefer to reserve causes for attributes of models, however. When you ask me for the cause of something, you are asking me for a model whose mechanism can explain the phenomenon in question. Lacking such a model, I demur when it comes to giving causal explanations. I think it was Popper who said somethwhere, "There are no promotions for coast guarders who cry out, 'Ships in distress exist!'" Details are needed. Since learning a little PCT I am increasing aware of this requirement.

Bruce Gregory

Date: Mon, 23 Dec 1996 06:51:36 -0500
From: Bruce Gregory <gregory@CF4.HARVARD.EDU>
Subject: Re: Fw: Driving blind; wearing coats

[From Bruce Gregory (961223.0650 EST)]

Bill Powers (961223.0100 MST)]

> Bruce Gregory (961222.1615 EST)--
>
> >Having a direct experience of my ability to set reference
> >levels, I am compelled to admit that I _am_ autonomous.
>
> Autonomy isn't that easily handled. If you are compelled by evidence to
> admit that you are autonomous, you're demonstrating that your view is a
> logical conclusion, and thus forced by your premises and your experiences --
> not by "you."

In meant that I am compelled by my experience, not by logic. You are doubtless equally compelled to acknowledge that you can send messages via the Net. If you said that you were uncertain about this, I would have no choice but to assume that your experience looks much different from mine.

> This is a peculiarity of rational argument. When you try to demonstrate
> rationally that something is true, you're saying, in effect, that anyone
> presented with the same facts would have to agree with you. But if that is
> so, you're saying that no rational person has a choice about what to
> believe: once the observations and the logic have been understood, the
> conclusion is foregone and no longer a matter of free choice. So where is
> your autonomy then? It's simply the computer in your head that reasons as it
> must, and its conclusions are out of your control.
>
> Autonomy can't be understood in terms of just one or two levels of
> perception. In fact I don't think it can be understood until you've
> identified your own levels of perception in some kind of detail, so you can

> see that the logical, reasoning part of your mind is not the highest level
> that there is.

I agree. In fact, I don't understand autonomy at all!

> You say "I am compelled to admit that I am autonomous." To see that this is
> not your highest level of organization speaking, all you have to do is ask
> yourself what you think about this conclusion. Is being compelled to admit
> that you're autonomous OK with you? Would it be less acceptable if reason
> compelled you to go along with Bruce Abbott when he says we are NOT
autonomous?

If reason compelled me to the conclusion that I was _not_ autonomous,
I would question my reasoning, not my autonomy. Not because I am
unwilling to question autonomy, but simply because I get nowhere doing so.
Like solipsism (did I get it right that time?) imaging that everything you
do has a cause over which you ultimately lack "control" seems to solve
a problem that no one outside an institution has.

> The question then becomes, "What is satisfied, or not satisfied, by
> concluding that human beings are autonomous?" The conclusion itself now
> becomes unimportant; whether I'm speaking to you or Tracy or Bruce Abbott
> (I'm addressing all of you), the question is the same. Wherever your line of
> reasoning leads you with respect to autonomy, you have to decide whether
> this reasoning is valid or invalid, and whether the conclusion is
> satisfactory or unsatisfactory. And in making that decision, you have to
> consider something ABOUT the reasoning process, something that is a
> different kind of reason.

Yes, indeed.

> There's no way to answer the kind of question I'm posing without basically
> changing the subject. You must, in fact, go up a level. What happens when
> you look at it that way?

Eventually, the monkey chatter stops. But it always picks up again!

Bruce Gregory

Date: Mon, 23 Dec 1996 07:02:16 -0600
From: Bruce Abbott <abbott@CVAX.IPFW.INDIANA.EDU>
Subject: Re: Autonomy

[From Bruce Abbott (961223.0800 EST)]

>Bruce Gregory (961223.0640 EST) --

>I'm sorry that I left the impression that I think that autonomous actions
>have no causes. I prefer to reserve causes for attributes of models, however.
>When you ask me for the cause of something, you are asking me for a model
>whose mechanism can explain the phenomenon in question. Lacking such
>a model, I demur when it comes to giving causal explanations. I think it was
>Popper who said somethwhere, "There are no promotions for coast guarders
>who cry out, 'Ships in distress exist!'" Details are needed. Since learning a
>little PCT I am increasing aware of this requirement.

I can accept that. (:-> I do have such a model; it's called PCT.

Regards,

Bruce

Date: Mon, 23 Dec 1996 08:04:44 -0500
From: SjoshH@AOL.COM
Subject: PCT

Dear Mary Powers,
Thank you for your letter of Dec. 10. It was forwarded to me at my new address, 14 Babson St., Gloucester,MA 01930.
You write about the work of William Powers, presumably a relative of yours, who is now 70. That is approximately my age as well.
I might find "Perceptual Control Theory" of interest but cannot tell from your letter. Frankly I am kept busy with my own projects and current interests (mostly not related to cybernetics). I am not connected to any university with a big library. If you want to send me a reprint of one of W. Powers' writings in which his basic ideas are developed, I will read it. If it interests me sufficiently, I will contact William Powers.
I can be reached by letter or email -- but have not signed up for the Internet otherwise.
Sincerely, Steve J. Heims

Date: Mon, 23 Dec 1996 07:34:42 -0600
From: Bruce Abbott <abbott@CVAX.IPFW.INDIANA.EDU>
Subject: Re: Lecture and demo

[From Bruce Abbott (961223.0835 EST)]

>Rick Marken (961222.1030) --

>OK. The "Control of Perception" Demo is completed
>(http://www.leonardo.net/Marken/ControlDemo/ControlP.html) so here is my
>take on why we say "Behavior: The control of perception" rather
>than "Behavior: The control of the environment".

By a happy coincidence I just managed to get a new computer up and running with Windows 95, and was able to run through the JAVA demos. Great stuff, Rick! You'll have to show me how to do that.

>I think that we can see that behavior is the control of perception (not
>the environment) because we can see organisms controlling _different_
>perceptual representations of the _same_ environmental state of affairs.

Now hold on. _I_ was arguing that perception is controlled; _you_ were arguing that the environment is controlled. Let's not get confused about that! Which aspect of the environment you select for control is up to you. The fact that you are able to select different aspects for control at different times has no bearing on the question of whether it is your perception that is under control or some aspect of external reality.

>In my demo, the environment is a quadrilateral arrangement of four
>lines. . . .

>In this demo, the subject can control any one of three _perceptual_
>representations of an environmental state of affairs (the quadrilateral
>arrangement of x and y).

I would say that these are aspects of the objective environment. How you perceive them will depend on the nature of your perceptual mechanisms; what you attempt to control in the demo is one or another of those perceptual variables.

You were arguing that what you control are the objective aspects of the environment that give rise to those perceptions. I was arguing that this is true only as long as there is a predictable linkage between the perception and the environmental aspect of which the perception is supposedly a function. Your demo does not allow us to decide which is correct.

>The demo illustrates a point that Bill Powers has made in many
>contexts: we don't control things (like cars, houses, people etc.);
>we control variable perceptual _aspects_ of things; the color or
>speed of the car; the location or shape of the house, the
>politeness or ideas expressed by people. Behavior is the control
>of perception. But the environment is in the control loop; and
>the environmental reality that corresponds to computed perceptions
>is also being controlled.

That's what the operators at Three Mile Island thought. Because of a stuck valve (of which they were unaware), they were wrong. Your last sentence is simply an unsupported assertion of what you were supposed to be establishing in your argument.

Thanks for the lecture and the demo. Unfortunately, neither were relevant to the issue at hand.

Regards,

Bruce

End of CSGNET Digest - 22 Dec 1996 to 23 Dec 1996
****

Date: Tue, 24 Dec 1996 08:00:13 +1800
Subject: CSGNET Digest - 23 Dec 1996 to 24 Dec 1996

There are 20 messages totalling 1301 lines in this issue.

Topics of the day:

Date: Mon, 23 Dec 1996 09:15:07 -0600
From: Gary Cziko <g-cziko@UIUC.EDU>
Subject: *SPECIAL HOLIDAY STORY* "The Fable of the Radio"

[from Gary Cziko 961223.1509 GMT]

As I have done a few times in the past during this time of year, I am posting below Greg Williams's story "The Fable of the Radio."

It may not be "A Christmas Carol," but it is one of my favorite short stories and for some reason it seems to me quite appppropriate for this time of year.

Happy Holidays to all!--Gary

P.S. Greg's new e-mail address is rareideas@juno.com. His still current road address is 460 Black Lick Road, Gravel Switch, KY 40328 (606/332-7606). He is still very much interested in receiving any papers, book chapters or books related to PCT to add to his archives of PCT-related materials that he keeps on the banks of Black Lick Creek in Kentucky.

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THE FABLE OF THE RADIO

By Greg Williams, Rt. 1, Box 302, Gravel Switch, KY 40328

Distributed at the Control Systems Group Meeting, Durango, CO, August 1991

Note: This is a work of fiction. Any similarity between characters in this story and real individuals, living or dead, is purely contingent, whatever that means. [Insiders realize, of course, that Fred refers to B.F. Skinner and Bill to William T. Powers].

Once upon a time, a well-off little boy named Fred bought a radio with money his parents gave him (usually, but not always) when he took out the trash. Why Fred wanted a radio isn't important. What's important is that it didn't take him very long to figure out how to work the radio. When he turned the knob marked "ON- VOLUME" clockwise until there was a click, he sometimes could hear soft sounds from the radio. He discovered that the sounds got louder if he kept turning the "ON- VOLUME" knob clockwise, as far as it would go without a lot of force. If he then turned the knob counterclockwise, the sounds got softer. He also discovered that whether the radio made sounds at all (other than a hissing which also got louder when he moved the "ON-VOLUME" knob clockwise, and softer when he moved the knob counterclockwise) depended on the position of another knob, marked "TUNING."

Fred was happy with his radio--at least he stayed near it much of the time, moved both knobs occasionally (according to a schedule which his parents decided was sometimes "essentially random" and sometimes correlated with the sounds produced by the radio before and/or after the knobs were moved), and evidenced facial expressions and limb movements correlated with the intensity and frequency patternings of the sounds coming from the radio. Soon Fred could get his radio to make sounds that wouldn't result in his parents leaving the room; this occurred more frequently as allowance-paying day approached.

By and by, Fred's friends got radios, too, and Fred discovered that he could work those radios in the "same" (Fred's word) way as he worked his own; in truth, there were some differences, which Fred said were "of no consequence," between working his radio and working his friends' radio--for example, some of the latter had "ON" knobs, rather than "ON- VOLUME" knobs, and some made louder sounds when their "ON- VOLUME" knobs were turned counterclockwise, rather than clockwise. Still, it took Fred only a little while (with a bit of screaming from his friends' parents) to be able to work all of the radios equally well. Fred exclaimed to his parents, "I really know how to work radios well!" His parents agreed with him, not because of his claim, but because they saw him working the various radios in ways which, to them, could be classified, if not as "good," then at least as "correct."

Alas, one day about six weeks after he got his radio, Fred could not work it. He moved the knobs as he had before, but the radio made no sounds at all. Fred moved them again. No sounds. And again. Still no sounds. Gradually, Fred moved the knobs less and less frequently. (However, whenever he came home from working a friend's radio, he moved the knobs on his own radio quite frequently for awhile, even though the radio produced no sounds.) But eventually, none of his friends' radios could be worked, either, and Fred didn't move the knobs on his radio at all--the radio just sat silently in his room. Exactly 83 days after the last time he moved the knobs (on a day not noticeably different to his parents than those before or after it), Fred threw the radio out with the trash, muttering obscenities. (His parents, hearing the cursing, washed Fred's mouth out with soap. Fred kept on cursing, and his parents kept on using soap "to deal with his inappropriate verbal behavior." Fred confided to his closest pals, but not to his parents, that he had discovered he "liked" the taste of soap.)

Now, it happened that Fred's radio was found at the local dump by Bill, a street- smart kid who appreciated gadgets--the more complicated, the better. Bill had never seen the insides of a radio before, and he was delighted when he pried off the back and gazed upon the maze of wires and little objects interconnected inside. "Wow!" he exclaimed. "I wonder how it works?" Bill took the radio home, where he hid it from his father, who would probably try to prevent Bill from "breaking it by messing with its guts" enough to find out how it worked. Excitedly, Bill went to the public library and began looking for books on radios. He found a book with pictures of a radio being taken apart and put back together, in steps, but the radio in the book wasn't the same kind as the radio he had found, and he wanted to know how radios work in general, not just how the one he found or the one in the book could be repaired (which generally involved replacing "defective" parts with little understanding of how those parts worked). Then he came across a book titled BASIC ELECTRONICS: RADIO CIRCUITRY, VOLUME 1 and perked up; even though he didn't know what "electronics" meant, the words "basic" and "radio" so close together seemed encouraging. To make a long story short, Bill read that book (and also VOLUME 2 and VOLUME 3), spent hours looking at the construction of the radio he had found, and finally announced to his father that he had learned how radios work. (To which his father replied "so what?" but Bill didn't let that bother him.) Bill was so happy about knowing how radios work, he told his father about the radio he had found, currently hidden under a heap of broken concrete blocks. "Give it to me!" demanded his father. Bill got the radio and meekly gave it to his father. Of course, when his father tried to work it, he found that it was no use - - Bill hadn't changed anything inside the radio, and it still wouldn't make a sound. "Bah! What good is it?" Bill's father shouted, as he threw the radio down. As his

father walked away, Bill calmly walked over to the radio and took off its back. He speculated about why the radio didn't work. "It probably needs a new battery," he thought, and then he saw that one wire to the RF-coil was loose, probably due to his father's anger.

Bill took $3 from under his father's mattress, wrote out an I.O.U. (with interest) and stuffed it under the mattress, and headed for the local Radio Shack store with the radio in hand. On the way, as fate would have it, his path crossed that of Fred, who warily approached Bill, eyeing his non-designer jeans as if in disgust (or so Bill supposed). For a reason which Fred himself, to this day, says he "cannot explain--apparently, random variability," Fred began yelling "That's my radio! That's my radio! That's my radio!" over and over. Bill dropped the radio and took off running. When Fred recovered his composure, he did not stoop to pick up the radio; rather, he kept on walking and never so much as looked back at the radio lying on the sidewalk. But he emitted, almost inaudibly, "I'll fix that peon!" and his course changed slightly from its direction prior to encountering Bill.

>From a hidden vantage point up the block, Bill saw Fred walk away--strangely, without the radio. As soon Fred disappeared in the distance, Bill hurried back to the radio, grabbed it, and hurried to Radio Shack. On the way, he got to thinking about Fred's claim that the radio was Fred's. Well, maybe it was... maybe it had ended up at the dump by some crazy mistake or weird misunderstanding... and maybe getting it back in good working order would make Fred feel better... and maybe Fred, who looked pretty upscale, might thank Bill and introduce him to one of those fancy uptown girls. It would be worth a try, and $3.

The Radio Shack clerk didn't seem to know an RF- coil from his rear end, but he did show Bill how to use the soldering gun which he kept hot for repairing speaker leads and such. Bill paid for a new (overpriced) battery and moved the radio's knobs gingerly. The resulting chorus of "Louie, Louie" resounded throughout the store! The clerk yelled, "Get outside with that noise!" Bill complied. And just then, Fred appeared! With two cops!! Before Bill had a chance to run, one of the policemen grabbed him around the waist and threw him to the ground. Bill, who had never been in such a situation before, knew exactly what to do: he tried not to move a muscle (which was only partly successful; his left eye started to twitch uncontrollably). The radio continued to produce loud sounds (something about a "very last chance sale" at Harry's Carpet Barn) while the other policeman searched Bill for crack and Fred, with a strange look (at least it seemed strange to Bill), grabbed the radio.

It all turned out better than Bill expected. No, he didn't get introduced by Fred to any uptown girls; he never even spoke to Fred. Fred was immediately and rather mysteriously influenced by the (sound of the? sight of the? sound and sight of the?) again- working radio, and the again-working radio was immediately and rather mysteriously influenced by Fred (or Fred's muscles?). The upshot was that Fred scampered away with the radio, rapturously twiddling its knobs in an incredibly sophisticated way (or so it seemed to Bill). With nobody around to press charges, the policemen had to release Bill with a warning against "fooling around with somebody else's property in ways they wouldn't approve of."

Bill thought that, as they parted company, Fred had seemed very happy. Bill was happy that he had helped Fred become so happy. And Bill was also happy because he knew how radios work, even if he didn't know as well as Fred how to work radios (although he suspected that he could figure out how to work

radios as well as, and maybe even better than Fred, if he wanted). In fact, for the rest of his life, Bill remained very happy. (He even married an uptown girl, but that's another story).

For six weeks after the fateful meeting of Fred and Bill, Fred told his parents (with an annoyingly high frequency) that he was "very happy." They believed Fred, not because he said so, but because he took out the trash so regularly, even on the day after allowance-paying day, during every one of the six weeks. (And after those six weeks? Well, that's another story, too.)

Date: Mon, 23 Dec 1996 10:58:14 -0500
From: Martin Taylor <mmt@DCIEM.DND.CA>
Subject: Re: Lies, damned lies and non-control

[Martin Taylor 961223 11:00]
> Bruce Gregory (961220.1530 EST)

>
> Martin Taylor 961220 14:30
>
> > If we can keep straight that _both_ perceptual control _and_ model-based
> > output can, under different curcumstances, be useful in stabilizing
> > important environmental variables, then perhaps we can deal with them
> > not as conflicting propositions but as complementary possible strategies
> > that could support each other. A control loop in a model-based system
> > could reduce the "unmodelled deviation" (not "disturbance") to the
> > stabilized variable; a good model (such as the Artificial Cerebellum)
somewhere
> > in a control loop could reduce the effect of the disturbance and enhance
> > control, if the environmental feedback loop or the disturbance had
> > partly predictable characteristics.
> >
> > Perhaps it would be a good idea if we were to use "control" for perceptual
> > control, and "stabilization" for what happens to an environmental variable
> > whose perceptual correlate is controlled. Stabilization, but not control,
> > also happens when a good outflow model opposes a well-predicted disturbance.
>
> Could it not be that, as far as living control systems are
> concerned, the time scale for the results of outflow models
> such as the thickness of turtle shells or the behavior of
> deciduous trees is much much longer than the time scales for
> perceptual control? In which case might the former be of limited
> value in understanding the behavior of living systems?
>
Sorry to quote the whole message, but I'm afraid I see nothing in my message to which your comment relates.

In an earlier message I had mentioned turtle shells as a somewhat self-evident comment on a Marken statement that evolution didn't provide protective walls, but you quoted a quite different message.

Martin

Date: Mon, 23 Dec 1996 11:02:29 -0500

From: Martin Taylor <mmt@DCIEM.DND.CA>
Subject: Re: DIRECT Re: Lies, damned lies and non-control

Tracy,

Thank you for your encouragement. It's always nice when someone agrees, even though it can be fun when someone doesn't.

Martin

Date: Mon, 23 Dec 1996 11:11:18 -0500
From: Martin Taylor <mmt@DCIEM.DND.CA>
Subject: Re: Lies, damned lies and non-control

[Martin Taylor 961223 11:00]
> Rick Marken (961220.1330)]
>
> >Perhaps it would be a good idea if we were to use "control" for perceptual
> >control, and "stabilization" for what happens to an environmental variable
> >whose perceptual correlate is controlled.
>
> I don't think so. Martin. Nice try. I think we should call control "control".
>
> The environmental variable in a control loop is controlled. Try it and see
> for yourself. Do what I now call the "Nature of Control" tracking demo at my
> web site. I now provide two measures of control: RMS Error and Stability.
> Both of these measures are computed on the basis of the value of an
> environmental variable (cursor position in pixels) _not_ a perceptual
> variable (the neural correlate of cursor position or whatever the "real"
> controlled variable is in your brain). Lo and behold you will see that the
> cursor, an environmental variable -- the variable that you and Bruce Abbott
> seem to think is _not_ controlled by a control system -- is under control.

I'm afraid I don't understand this. You provide a demonstration that an environmental variable is stabilized by the process of perceptual control, and use this to assert that we should not use the word "stabilize" to refer to it. And furthermore, you deny the whole premise of PCT to assert that it is the environmental variable, and not the perceptual variable, that is controlled.

It's hard enough to argue with you when you twist and turn your own statements to mean different things three times per day, but when you say that "perceptual control" means "control of environmental variables", even I tend to get lost in the wilderness.

My proposal was based on the notion that "perceptual control" means "control of a perceptual variable." The result of perceptual control is the stabilization of an environmental variable. If there are other ways environmental variables can be stabilized, I don't think they should be called "control." That was my point. Now you say that if an environmental variable is stabilized, it must be "control," which is what you have been railing against Hans Blom for saying.

All I want for Christmas is some way of understanding Rick Marken:-)

Martin

Martin

Date: Mon, 23 Dec 1996 11:28:46 -0500
From: Martin Taylor <mmt@DCIEM.DND.CA>
Subject: Re: Driving blind; wearing coats

[Martin Taylor 961223 11:30]
> Bruce Gregory (961221.1815 EST)]
>
>
> Bruce Abbott (961221.1340 EST)
> >
> > >Bruce Gregory (961221.1310 EST) --
> >
> > >I would say that you use your internal map to set reference levels,
> >
> > And what is an internal map if not a model of some aspect of the external
> world?
>
> An internal map is certainly a model of the world. The debate _seems_
> to be about the role that such models play in control. My position is
> that we use them to help set reference levels.

Is there any other option, except using them to act on the world directly?
And if that were the case, how would it be done, if not by setting reference
levels for muscle tensions?

If one leaves any shred of PCT in one's theory of behaviour, one MUST believe
that the role of models is, in some way, to set reference levels. But there
may be issues as to whether models have _any_ role, and if they do, how they
go about affecting reference levels.

I don't think your statement of position refined very much anyone's (my,
anyway) understanding of what you think models do, except for the fact that
you do think they have a role.

Martin

Date: Mon, 23 Dec 1996 11:43:56 -0500
From: Martin Taylor <mmt@DCIEM.DND.CA>
Subject: Re: Lecture and demo

[Martin Taylor 961223 11:35]
> Rick Marken (961222.1030)]
>
> OK. The "Control of Perception" Demo is completed
> (http://www.leonardo.net/Marken/ControlDemo/ControlP.html) so here is my
> take on why we say "Behavior: The control of perception" rather
> than "Behavior: The control of the environment".
>
> I assume that we all know why we say that behavior is the control of
> _something_.
>....
> The demo illustrates a point that Bill Powers has made in many

> contexts: we don’t control things (like cars, houses, people etc.);
> we control variable perceptual _aspects_ of things; the color or
> speed of the car; the location or shape of the house, the
> politeness or ideas expressed by people. Behavior is the control
> of perception. But the environment is in the control loop; and
> the environmental reality that corresponds to computed perceptions
> is also being controlled.
>
I'm afraid your demo, nice though it may be, is far from addresing the point you hoped to address--whether to call the stabilization of an environmental variable "control" when there is no perceptual control process in action in respect of that variable..

(Rick, note the way your quote marks came out in this message, illustrated in the following quote)

>But all three perceptions can
>be controlled. You can see this in a graph of the state of the
>three perceptual variables over time. The controlled perceptual
>variable remains relatively stable while the uncontrolled
>variables vary all over the place.
>
>Note that all three perceptual representations are still “there”
>even when they are not controlled. The quadrilateral still has
>area (x*y) when the subject is controlling shape (x/y).

Whoops! At what point did you demonstrate that the three perceptual repreentations exist within the viewer's head when they are not being controlled?

Whoops again...Isn't it the geometrical analysis of the _environmental_ variable that says that the quadrilateral still has area when the subject is controlling shape?

Whoops again, again...Didn't someone called (I think it might have been) Rick Marken do some studies to check whether it was "really" (i.e. as analyzed by measuring the environmental variable) area that was controlled when the subject claimed to be controlling area in this situation?

Oh, whoopsy dooo!

Martin

Date: Mon, 23 Dec 1996 10:21:47 -0700
From: Bill Powers <powers_w@FRONTIER.NET>
Subject: Re: Autonomy

[From Bill Powers (961222.0830 MST)]

Bruce Gregory (961223.0650 EST) --

>Eventually, the monkey chatter stops. But it always picks up >again!

"I am compelled to admit that I _am_ autonomous" is part of the "monkey chatter", isn't it? Just as is Bruce Abbott's "What we become is a product of uncountable accidents which have given us our unique genetic structures, developmental histories, and experiences."

All such statements are products of the categorizing, sequencing, and logical/programming parts of our minds. So says the PCT model to which we have all lent, in various degrees, provisional acceptance. It seems to be a fact of experience that we can be aware of the products of these mental processes (one of which is a statement that mental processes result from brain operations).

But we can also change these mental processes: Bruce Abbott can think "I am Bruce Gregory" and Bruce Gregory can think "I am Eleanor Roosevelt." If that can be done, there's a higher level operating. I claim it's the level where we fit our logic into a set of principles. And the principles, it seems to me, fit into and are adjusted to fit various system concepts, world-views.

So, why do we have world-views? I can't see any explanation other than that we want to make sense of experience, to see it all fit into a system of some kind where all the parts make sense in terms of each other. People who say we are autonomous and people who say we are not all all trying to do the same thing: to get a picture of the whole system that is made of all their experiences and thoughts about experience. They will think whatever it takes to generate this overall sense of consistency or harmony.

To me, this need for consistency or harmony is a dominating requirement of life. It seems to function just like an intrinsic reference signal. I can see it in all sorts of people, from street gangs to academics. How does it fit into evolution, learning, and all that? I don't know. To me, it seems to be an observation that still lacks an explanation.

Best,

Bill P.

Date: Mon, 23 Dec 1996 11:34:29 -0700
From: "T. B. Harms" <tbh@TESSER.COM>
Subject: Re: *SPECIAL HOLIDAY STORY* "The Fable of the Radio"

*from T. Harms 1996;12,23.11:

Gary Cziko 961223.1509 GMT

I read Greg Williams's story aloud to my eight-year old son, and he thinks the title should be "The Fable of the Stupid Radio, A Special Radio Story". He says it has absolutely nothing to do with Christmas.

We had fun with it, getting very silly.

Tracy Bruce Harms tbh@tesser.com
Boulder, Colorado caveat lector!

Date: Mon, 23 Dec 1996 12:50:27 +0000
From: Richard marken <marken@AERO.ORG>
Subject: Re: Lecture and demo

[From Rick Marken (961223.1140 PST)]

Me:

>I think that we can see that behavior is the control of
>perception (not the environment) because we can see
>organisms controlling _different_ perceptual representations
>of the _same_ environmental state of affairs.

Bruce Abbott (961223.0835 EST) --

>Now hold on. _I_ was arguing that perception is controlled;
>_you_ were arguing that the environment is controlled. Let's
>not get confused about that!

My statement above explained _why_ we might suspect that it is perception that is controlled. Also, my point it not that it is the environment _rather than_ perception that is controlled. My point is that the environment is part of the perceptual control loop; when you control a perceptual function of the environment you are also controlling the aspect of the environment that is represented by the function. When you control your perception of width you are also controlling the aspect of the environment (x in my demo) of which your perception is a function. When you control your perception of shape you also control the aspect of the environment (x/y in my demo) of which your perception is a function.

You seemed to understand _control of perception_ to mean that the environmental variable (CV), of which the controlled perceptual variable, p, is a function, is not controlled; you seemed to agree with Martin, who said that that the CV was "stabilized" by output, but not controlled. My demo shows that this is not the case. The CV (the environmental correlate of the controlled perception) is controlled when the perception is controlled. The fact that x/y (a measure of an aspect of the environment) remains under control when the subject controls his perception of shape, shows that the CV is controlled when the perception of the CV is controlled.

The fact that controlled aspects of the environment correspond to controlled _perceptual_ variables is the reason why we are able to use The Test (disturbing variable aspects of the environment and determining whether these disturbances change the state of these variables) to determine what variables are under control.

>The fact that you are able to select different aspects for
>control at different times has no bearing on the question of
>whether it is your perception that is under control or some
>aspect of external reality.

That's correct. What has a bearing on this question is the fact that you can control _different_ perceptual aspects of the _same_ objective state of affairs. When you do this, you are controlling different perceptions _and_ you are controlling the different aspects of the environment that _correspond_ to these perceptions.

>You were arguing that what you control are the objective
>aspects of the environment that give rise to those

>perceptions.

Not quite. I am arguing that you are controlling perceptions and, as a result, that you are also controlling the measurable aspects of the objective environment that correspond to those perceptions. When you control a perception of x/y (where p = f(x/y)) you are also controlling x/y (the ratio of the lines in the environment that make up the quadrilateral).

> I was arguing that this is true only as long as there is a
> predictable linkage between the perception and the
> environmental aspect of which the perception is supposedly
> a function.

I guess my reply to this is that we always control _some_ objective aspect of the environment; the aspect that corresponds to the controlled perception. Your argument seems to be that we are not controlling an objective aspect of the environment (like x/y) if the perception we are controlling (say, x) does not correspond to the aspect of the environment that we suppose ourselves to be controlling (x/y, perhaps). But I argue that even if we are controlling, say, a perception of x under the assumption that we are really controlling the environmental variable, x/y, we are still controlling an objective aspect of the environment, viz. the environmental correlate of the perception of x, which is x.

>Thanks for the lecture and the demo. Unfortunately, neither
>were relevant to the issue at hand.

I can only provide the _opportunity_ for learning.

Martin Taylor (961223 11:35) --

>I'm afraid your demo, nice though it may be, is far from
>addresing the point you hoped to address--whether to call
>the stabilization of an environmental variable "control"
>when there is no perceptual control process in action in
>respect of that variable.

See my reply to Bruce. I think the demo does address this point rather clearly and shows (in the graph) that the environmental correlates of controlled perceptual variables are under control.

>(Rick, note the way your quote marks came out in this
>message, illustrated in the following quote)

It’s those damn smart quotes in Word, I think. I’ll try to fix that up.

Me:

>Note that all three perceptual representations are still
>there even when they are not controlled. The quadrilateral
>still has area (x*y) when the subject is controlling shape
>(x/y).

Ye:

>Whoops! At what point did you demonstrate that the three
>perceptual representations exist within the viewer's head
>when they are not being more controlled?

I agree that my statement above was not clear. What I should have said is that the _environmental correlates_ of the three perceptual representations (width, area and shape) are measurable aspects of the environment that exist even when they are not being perceived and controlled.

>Isn't it the geometrical analysis of the _environmental_
>variable that says that the quadrilateral still has area
>when the subject is controlling shape?

Yes. The point is that aspects of the environment (like shape, humidity, the taste of lemonade, the color of a leaf) are _available_ to be measured even when people are _not_ there to perceive them. In this sense I consider them objective. The physics model of the environment, for example, tells me that color doesn’t exist; just different wavelengths of electromagnetic energy. But I can monitor what corresponds to color (and Test to determine whether color is being controlled by a person) when I have a reasonably good idea of the perceptual (weighting) function that produces a color perception as a function of wavelength.

When Leonardo controlled his perception of the color at each point on the Last Supper he was also controlling the objective correlate of these perceptions -- the relative reflectance of different wavelengths from the fresco pigments.

Me:

> The environmental variable in a control loop is controlled.
>Try it and see for yourself.

Martin Taylor (961223 11:00) --

>I'm afraid I don't understand this. You provide a
>demonstration that an environmental variable is stabilized
>by the process of perceptual control, and use this to assert
>that we should not use the word " stabilize" to refer to it.

The stability measure in this demo shows that the environmental variable is protected from the effects of disturbance; it is controlled.

>And furthermore, you deny the whole premise of PCT to assert
>that it is the environmental variable, and not the >perceptual variable, that is controlled.

Not at all. I have never "denied" that it is perception that is controlled. All I am saying is that, when a perception is controlled, the aspect of the environment (CV) that corresponds to that perception is _also_ controlled. If this were not true, then an objective study of control (purposeful behavior) would be impossible (something that possibly wouldn’t trouble you but would put me out of business).

>All I want for Christmas is some way of understanding Rick

>Marken:-)

With all due respect, I think it might help if you were able to
get out of the mode of teaching PCT for a while and get into the
model of trying to learn it.

Best

Rick

Date: Mon, 23 Dec 1996 12:57:52 -0700
From: "T. B. Harms" <tbh@TESSER.COM>
Subject: somehow seems relevant...

*from T. Harms 1996;12,23.13

I was inspired to forward the following msg to the CSGnet because it struck
me as instructive regarding the fact that even fights involve a mutuality
of interests. Bill wrote directly to that in B:CP. In regard to hockey,
rugby, and boxing this is of course no surprise. Still, it is easy to
overlook, and kinda fun to be reminded.

Date: Mon, 23 Dec 1996 05:50:02 -0700
From: qotd-request@ensu.ucalgary.ca (Quote of the day)
To: qotd@ensu.ucalgary.ca (Quote of the day mailing list)
Subject: Quote of the day
X-Qotd-Incoming: 1514

Ever wonder what [hockey] players say during a fight? Here's a
blow-by-blow account of the conversation between Dallas' Todd Harvey
and Colorado's Mike Keane late in the fight last week.

Keane: "You finished?"

Harvey: "Yeah."

Keane: "Good fight, man."

Harvey: "Thanks."

- From ESPNET SportsZone, 12 October 1996

Submitted by: "William Wisner" <wisner@gryphon.com>
Oct. 12, 1996

Send quotation submissions to qotd@ensu.ucalgary.ca
Send list changes or requests to qotd-request@ensu.ucalgary.ca

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Date: Mon, 23 Dec 1996 15:26:09 -0500
From: Jeff Vancouver <jeffv@PSYCH.NYU.EDU>
Subject: the Vancouver experiment, etc.

[from Jeff Vancouver 961223.15:00 EST]

I clearly need to reorganize something. I just delete 1800 messages from my e-mail in-basket. Most I never read. Any with the subject "Vancouver experiment" I printed. The Fall term just ended last week and I have been completely swamped with my job search, teaching, research, etc. I am leaving for break in a few hours and will not be back until Jan 6. So there is sure to be a hugh list when I return.

One of my planned (but unlikely to occur) activities for the break is to summarize where we are on the model-based control issue. Unfortunately I may have deleted some key posts on the topic, but I am at my information overload end.

Let me tell you all the perception I am seeking to control. I have not given up on the larger academic community. For me, the goal is to develop a paper that can be submitted and published in APA acredited, peer-reviewed journals. That means that the antagonists in this debate (or some subset of them) must cooperate enough to create a coherent set of questions and data that address the questions. I have ideas of how this might look, but I need to read the more recent posts (that I printed) to see where we are.

I am not sure anyone else will be controlling for the same perception as me. If that is true, I will move on. But much in the way of good theory, data, and simulations are not making their way to a larger audience. Given that I must please this larger audience to control the variables of central interest to me, I want to work toward translating some of this good work from here to there. Of course, my pace is much slower than most on this net (for reasons I cannot quite understand). Please bare (bear?) with me.

Happy holidays

Jeff

_____

Jeffrey B. Vancouver

Assistant Professor Phone: (212)998-7816
Department of Psychology Fax: (212)995-4018
New York University e-mail: jeffv@psych.nyu.edu
6 Washington Pl., Rm 578
New York, NY 10003

Date: Mon, 23 Dec 1996 15:59:22 -0500
From: Bruce Gregory <gregory@CF4.HARVARD.EDU>
Subject: Re: Autonomy

[From Bruce Gregory (961223.1600 EST)]

(Bill Powers (961222.0830 MST)

> So, why do we have world-views? I can't see any explanation other than that
> we want to make sense of experience, to see it all fit into a system of some
> kind where all the parts make sense in terms of each other. People who say
> we are autonomous and people who say we are not all all trying to do the
> same thing: to get a picture of the whole system that is made of all their

> experiences and thoughts about experience. They will think whatever it takes
> to generate this overall sense of consistency or harmony.
>
> To me, this need for consistency or harmony is a dominating requirement of
> life. It seems to function just like an intrinsic reference signal. I can
> see it in all sorts of people, from street gangs to academics. How does it
> fit into evolution, learning, and all that? I don't know. To me, it seems to
> be an observation that still lacks an explanation.

A very interesting point. I suspect, however, that I have less invested
in my world view than you might infer. All I know is that I must decide
where to set the thermostat level, whereas the thermostat has no such
problem. Bruce Abbott's worldview doesn't provide much help in the
process. He may of course have a better model than I do, but I still
have no idea how to use it fruitfully. No doubt my limited imagination.

Bruce Gregory

Date: Mon, 23 Dec 1996 15:51:01 -0500
From: Bruce Gregory <gregory@CF4.HARVARD.EDU>
Subject: Re: Driving blind; wearing coats

[From Bruce Gregory (961223.1550 EST)]

Martin Taylor 961223 11:30

> > Bruce Gregory (961221.1815 EST)]

> > An internal map is certainly a model of the world. The debate _seems_
> > to be about the role that such models play in control. My position is
> > that we use them to help set reference levels.
>
> Is there any other option, except using them to act on the world directly?
> And if that were the case, how would it be done, if not by setting reference
> levels for muscle tensions?

My thoughts exactly. I'm delighted we all agree. Must be the season ;-)

> If one leaves any shred of PCT in one's theory of behaviour, one MUST believe
> that the role of models is, in some way, to set reference levels. But there
> may be issues as to whether models have _any_ role, and if they do, how they
> go about affecting reference levels.

Suppose I give you the following set of instructions, "Take the Mass.
Turnpike west. Leave at the Auburn exit, Exit 10. Turn south on Rout 395.
Cross the Massachusetts-Connecticut border and take exit 97 to
Putnam..." You now have a map or a model. Surely this is a model
of the world and surely you would make some use of it in setting
references levels for perceptions, such as the Ext 10 sign at Auburn.
This is all I was saying. _How_ you are able to follow my instructions
using this model is something I leave for those who understand these
things better than I.

> I don't think your statement of position refined very much anyone's (my,
> anyway) understanding of what you think models do, except for the fact that
> you do think they have a role.

Martin, few of us seem to refine your understanding of anything. Still, it's nice to agree from time to time...

Bruce Gregory

Date: Mon, 23 Dec 1996 16:46:45 -0700
From: Bill Powers <powers_w@FRONTIER.NET>
Subject: Re: Lecture and demo

[From Bill Powers (961222.1515 MST)]

Rick Marken (961223.1140 PST) --

>I have never "denied" that it is perception that is
>controlled. All I am saying is that, when a perception is
>controlled, the aspect of the environment (CV) that >corresponds to that
perception is _also_ controlled. If this >were not true, then an objective
study of control (purposeful >behavior) would be impossible (something that
possibly >wouldn't trouble you but would put me out of business).

I've meant for some years to write a program illustrating the figure in my '73 Science article that showed the organization of a control system. This diagram has implications that I hoped at the time (in vain) might arouse some discussion. Maybe you'd like to give it a try, Rick.

In that figure, there is no "CEV" shown in the environment. Instead, there is a collection of individual variables in a sort of cloud. This cloud is affected through multiple paths by the output of the control system, and the perceptual function is affected in multiple ways by the same cloud of variables, shown as "v's".

What I had in mind for the demo program (when I thought about it today) was to create a bunch of connections among the v's more or less at random, each v being affected (through weighting factors less than 1) by some subset of the other v's. For example,

```
v1 = 0.1*v2 + 0.3*v3 - 0.2*v5
v2 = 0.2*v4 - 0.3*v1
v3 = 0.2*v4 + 0.1*v1 + 0.7*v2 - 0.4*v1
v4 = ...
```

and so on. It's not supposed to make sense -- just a random bunch of effects, so changing any one variable would have effects on all the others.

Now you create an input function such that, for example,

p = 9 * v1 - 3 * v4 + 4 * v6,

and you create some effects of the output signal o such that, for example,

```
v1 = 0.1*v2 + 0.3*v3 - 0.2*v5 + 20* o
v3 = 0.2*v4 + 0.1*v1 + 0.7*v2 - 0.4*v1 - 12*o
...
```

Now you have to determine the net effect of o on p, and adjust the sign of

the (integrating) output function accordingly. You can do this quickly by varying o and determining which way p is affected (this is a linear system so there will only be either a positive or a negative effect). You adjust the effect of the error signal on the integrator to have the same sign, since there will be a sign inversion in the comparator.

This will give you a control system that controls some "aspect" of the collection of v's. To complete the demo we need a disturbance d, which also affects the cloud of v's: for example

```
v1 = 0.1*v2 + 0.3*v3 - 0.2*v5 + 20 * o - 0.3*d
v2 = 0.2*v4 - 0.3*v1 + 0.9*d
v3 = 0.2*v4 + 0.1*v1 + 0.7*v2 - 0.4*v1 - 12*o - 0.2*d
... etc.
```

In an even more general demo you could use multiple d's (d1,. d2, ... dn), each affecting several randomly-selected v's through different weighting factors.

You'll have to fiddle with the mutual effects of the v's so that when you iterate the whole environment system over and over with constant o and d, the states of the variables come to some steady state.

OK, so what's the relevance of this demo to the subject under discussion?

When you run this demo, you'll find that all the usual laws of control hold true. Varying the reference signal will make the perceptual signal follow it. Varying any disturbance affecting the v's will result in variations of the output that keep the perceptual signal from changing as much as it would change if there were no control; that is, the effect of the output on the perceptual signal will be proportional and opposite to the effects of variations in the disturbance, given a constant reference signal.

The only thing missing from this demo is any environmental variable that is the "correlate" of the perceptual signal. In order to do the test for the controlled variable, you would somehow have to come up with a perceptual function of your own, which would generate a signal p' such that

p' = 9 * v1 - 3 * v4 + 4 * v6,

the same function of the v's that was put into the control system's perceptual function.

Now you would find that the effect of o on p' is equal and opposite to the effect of d on p'. In other words, you would have to be perceiving the environment the same way that the control system perceives it: then and only then would you be able to see an "objective" correlate of what the control system is controlling.

Well, I shouldn't say "only" then. In fact there is probably a large number of forms for p' that would give an apparent model of what is being controlled. However, it seems to me that there is a theorem hidden in here somewhere, which perhaps some of our more sophisticated mathematicians could work out.

Suppose you have a set of n control systems, each controlling a different "aspect" of the set of v's. While the definition of the controlled variable for each system, found through the Test, might be ambiguous, when you have multiple control systems the definitions have to be mutually compatible. The

final states of the v's have to satisfy all the reference conditions at once, in terms of the way each system's p is derived from the set of v's. When you apply n Tests at the same time, the set of controlled variables you come up with must fit the behavior of the whole collection of v's under all possible combinations of disturbances, in proper relation to the set of o's, and over all possible values of the reference signals. It's assumed, of course, that you have a way to observe the state of each v.

The theorem I have vaguely in mind would say that the range of possible definitions of controlled variables becomes smaller as the number of parallel control systems (assumed orthogonal) becomes larger. I don't think that a unique set of definitions would ever become possible except in the most unlikely case where the number of v's was equal to the number of control systems. Since the number of v's must be assumed, in general, to be much larger than the number of control systems, the best we can hope for is to narrow the possibilities to families of possible controlled variables and associated control systems.

All this, I think, throws a different light on what we mean by "controlling the environment." When I first thought of this demo, quite a few years ago, I wondered what would happen if the environment were modeled as a completely random set of connections, connections that remain fixed but which have no particular ordering scheme behind them. Everything affects everything, although local effects are stronger than remote effects (because the coefficients are all less than 1). Could a set of control systems exist in which different aspects of this environment, literally created by the weightings assigned in the input functions, are brought under systematic control? Could a _hierarchy_ of control systems arise, in which there would be low-level and high-level controlled variables of the usual kind? Would such a hierarchy be able to derive theories about the environment, physical laws based on the regularities _created_ by each level of perceptual input functions? Could such a hierarchy, by controlling its own perceptions, impose order on an environment which, while regular, otherwise contains no ordering principles at all?

At the moment, my guess is "yes." While this guess might not be provable, perhaps it might be DISprovable. Anything that could be said about it mathematically would almost have to have profound implications regarding epistemology.

Best,

Bill P.

Date: Mon, 23 Dec 1996 16:55:35 -0700
From: Bill Powers <powers_w@FRONTIER.NET>
Subject: Re: Autonomy

[From Bill Powers (961222.1700 MST)]

Bruce Gregory (961223.1600 EST) --

>All I know is that I must decide
>where to set the thermostat level, whereas the thermostat has no such
>problem.

But you don't decide whether you feel too warm or too cold. Normally, you

turn the thermostat up or down so your own temperature feels just right. And you don't decide what feels just right. That reference signal (among many others) is not under your conscious control.

Best,

Bill P.

Date: Mon, 23 Dec 1996 20:19:26 +0000
From: Richard marken <marken@AERO.ORG>
Subject: Re: Lecture and demo

[From Rick Marken (961223.1910 PST)]

Bill Powers (961222.1515 MST)--

> What I had in mind for the demo program...was to create a bunch
> of connections among the v's more or less at random...Now you
> create an input function such that, for example,
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> variable that is the "correlate" of the perceptual signal.
> In order to do the test for the controlled variable, you
> would somehow have to come up with a perceptual function of
> your own, which would generate a signal p'... the same function
> of the v's that was put into the control system's perceptual
> function...All this, I think, throws a different light on what
> we mean by "controlling the environment."

It's not clear to me how the point you are trying to make with your very interesting suggestion for a demo differs from the point I was trying to make with my demo.

I think of x/y, x*y or x as "correlates" of controlled perceptual signals in my demo in the same sense that p' = 9 * v1 - 3 * v4 + 4 * v6 is a correlate of the controlled perceptual signal (p) in yours.

When I say "environmental variable" I am including _functions_ of what I think of as "real" environmental variables (the v's in your demo). I refer to x/y as an "environmental variable" because it is a function of what I am assuming to be the "real" environmental variables" in my demo.

I don't think that there are really shapes, widths, and areas "out there" in the environment, independent of systems that can construct these perceptions. What I am trying to do is verbally discriminate between a _perceptual variable_ (p) that is actually being controlled by a control system and a function (p') of variables in the control system's environment that seems to correlate with what the system is controlling.

So I am prepared to call p'-- a function of environmental variables -- an "environmental controlled variable" (CV) even though I know that there is no p' variable _in the environment_. This seems like reasonable terminology to me since p' is, from the modeler's perspective, a

representation of the controlled aspect of the _environmental component_
of the model of a control system.

Does this make sense to you?

Best

Rick

Date: Mon, 23 Dec 1996 20:34:40 -0700
From: Bill Powers <powers_w@FRONTIER.NET>
Subject: Re: Lecture and demo

[From Bill Powers (961222.2030 MST)]

Rick Marken (961223.1910 PST)--

>I think of x/y, x*y or x as "correlates" of controlled perceptual
>signals in my demo in the same sense that p' = 9 * v1 - 3 * v4 + 4 * v6
>is a correlate of the controlled perceptual signal (p) in yours.
>
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>what I think of as "real" environmental variables (the v's in your
>demo). I refer to x/y as an "environmental variable" because it is
>a function of what I am assuming to be the "real" environmental
>variables" in my demo.

This is a confusing issue, because we're trying to say that there is REALLY
a real environment out there, and at the same time say that it is not the
environment being represented by perceptual signals. The "real" environment,
one could say, contains the real ingredients of lemonade (quarks), but the
sensed environment (sweetness, tartness) is created by perceptual input
functions.

>I don't think that there are really shapes, widths, and areas
>"out there" in the environment, independent of systems that can
>construct these perceptions. What I am trying to do is verbally
>discriminate between a _perceptual variable_ (p) that is actually being
>controlled by a control system and a function (p') of variables in the
>control system's environment that seems to correlate with
>what the system is controlling.
>
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>an "environmental controlled variable" (CV) even though I know that
>there is no p' variable _in the environment_. This seems like reasonable
>terminology to me since p' is, from the modeler's perspective, a
>representation of the controlled aspect of the _environmental component_
>of the model of a control system.
>
>Does this make sense to you?

Every even-numbered day except Tuesdays.

Best,

Bill P.

Date: Tue, 24 Dec 1996 05:46:19 -0500
From: Bruce Gregory <gregory@CF4.HARVARD.EDU>
Subject: Re: Lecture and demo

[From Bruce Gregory (961224.0545 EST)]

Bill Powers (961222.2030 MST)
>
> Rick Marken (961223.1910 PST)--
>
> >I think of x/y, x*y or x as "correlates" of controlled perceptual
> >signals in my demo in the same sense that p' = 9 * v1 - 3 * v4 + 4 * v6
> >is a correlate of the controlled perceptual signal (p) in yours.
> >
> >When I say "environmental variable" I am including _functions_ of
> >what I think of as "real" environmental variables (the v's in your
> >demo). I refer to x/y as an "environmental variable" because it is
> >a function of what I am assuming to be the "real" environmental
> >variables" in my demo.
>
> This is a confusing issue, because we're trying to say that there is REALLY
> a real environment out there, and at the same time say that it is not the
> environment being represented by perceptual signals. The "real" environment,
> one could say, contains the real ingredients of lemonade (quarks), but the
> sensed environment (sweetness, tartness) is created by perceptual input
> functions.

The REAL environment is, of course, a component of our model.
That is, we are saying that we can account for our experience _if_ there is
a real environment with certain properties that interacts with our
perceptions in such a way. The REALLY REAL environment is the
one we cannot talk about, because talking about it makes it a
representation -- part of a model.

> >Does this make sense to you?
>
> Every even-numbered day except Tuesdays.

Damn. It's an even-numbered Tuesday....

Bruce Gregory

Date: Tue, 24 Dec 1996 05:48:06 -0500
From: Bruce Gregory <gregory@CF4.HARVARD.EDU>
Subject: Re: Lecture and demo

[From Bruce Gregory (961224.0545 EST)]

Rick Marken (961223.1910 PST)

The demos look great!

Bruce Gregory

Date: Tue, 24 Dec 1996 14:01:36 +0100
From: "J.A. Blom" <J.A.Blom@ELE.TUE.NL>
Subject: Re: reply to Martin: combining goals

[Hans Blom, 961224]

(Martin Taylor 961220 14:00)

>>... your construction seems to have an implication ("unintended
>>side effect"?) that you neither intended nor now wish to intend.
>>And therefore you cannot -- and do not want to -- see that
>>super-goal as something that _causes_ the behavior. And it doesn't,
>>of course; the 3 goals that you specified for and designed into the
>>controller cause its behavior. In that sense, the super-goal is not
>>causal; it is only mathematically equivalent to your 3 causal
>>parallel goals combined. Or does that make the super-goal causal by
>>implication?

>No it doesn't, because (by design), there is no representation
>anywhere except in the analyst's mind of the value of the
>super-goal. If the fact that the system is being observed and
>analyzed makes the variables in the analysis causal for the system,
>then you and I have very different views on the meaning of
>causality.

Causality plays, I think, a dual role. Top down: In the design of a process, we specify a goal (or more goals, if you want), which can be instrumented in a mechanism and thus can be considered the "cause" of the system's behavior. Bottom up: In the analysis of a process, we have to discover its goal(s) and the mechanism that realizes it; in this case, the goal(s) are in the eyes of the beholder; mental constructions or perceptions. Together, we may have the strange situation where _you_ construct a system with three goals, but _I_ "see" one goal at a higher hierarchical level, which is causally equivalent because it subsumes your three goals. We had this in our example: your system, for example, controls for x=1 AND for y=2 AND for z=3, whereas in my analysis it controls for (x,y,z) = (1,2,3). No difference in practice, just in words.

>>Beautiful! Here we have the raw contrast between "as if" and "in
>>fact" ;-). We see one and the same thing, the 3-goal controller
>>that you designed. You see that it has properties P1, P2, ..., PN.
>>I see an additional property PN+1. Is that property "in fact"
>>there, or is it only "as if" it is there, but not really? What kind
>>of test would you propose to make this question decidable?

>I would have to dissect the system and see whether I could find any
>representation of property PN+1 of the same kind as the
>representations that I will find of P1, P2, ... PN.

Is property PN+1 _really_ there or will you have to construct a perceptual input function to see it? If the latter, you may fail, or you may not want to construct one.

>Look, the issue wasn't whether one can determine whether a control

>hierarchy has a single top level goal in any particular case, but
>whether ALL control hierarchies must be BUILT so that they support
>one top-level goal.

In engineering design, that's a good strategy. If you build a controller with multiple (lower level) goals, their interactions will not be part of the design. Thus, the behavior of the overall system may still present some surprises. Surprises are what is to be excluded, as far as possible, in a design process.

>We know, for example, that all living control systems had as
>ancestors control systems that survived long enough to propagate
>their genes. We do NOT know that any of these ancestral control
>hierarchies were built so that they had a single top-level
>elementary control unit whose reference level for a perception of
>surviving had the value "long enough to propagate my genes."

This type of knowledge is by inference, by construction of perceptual input functions. I may "see" it but not you, or the other way around.

>But we do know that they ALL, over billions of years, without
>exception, behaved as if they did have such a goal.

Again the "as if". What would it mean to say that "it is as if a system has a goal, but in reality it doesn't"?

>> - does your controller have 3 goals whereas mine has one?

>Yours has four, mine has three. The referents for three of your
>goals are set by the control system for the top one.

Isn't that kind of funny? Exactly the same behavior in all respects, but a different number of goals? In that case, how "real" are goals?

>It's not the location of Rome that has extra degrees of freedom, but
>the ability to describe the behaviour used in getting to Rome--the
>behaviour you use to determine whether Rome is a "top-level goal" or
>simply the place arrived at because you wanted to get to longitude X
>and latitude Y for two irrelevant reasons. Your weights correspond
>to differences in the gain functions for X and Y control at the
>top-level of a hierarchy with no single (multidimensional) "Rome"
>("perceiving myself to be at x,y") goal.

Right: different weights will result in different gains for the subgoals, and thus in different "roads" toward the overall goal. For example, in

```
  minimize 1000000 * (x-1)^2 + (y-2)^2
```

the road towards the goal (x,y)=(1,2) will be pretty much constrained to where x is very close to 1, whereas in

```
  minimize (x-1)^2 + 1000000 * (y-2)^2
```

the road will run along y=2.

Thus, the weights translate into loop gains, and the single goal makes this explicit. How do you choose the three individual loop gains when you have three non-interacting goals?

Greetings,

Hans

End of CSGNET Digest - 23 Dec 1996 to 24 Dec 1996
****

Date: Wed, 25 Dec 1996 08:00:06 +1800
Subject: CSGNET Digest - 24 Dec 1996 to 25 Dec 1996

There are 19 messages totalling 796 lines in this issue.

Topics of the day:

1. Driving blind; wearing coats (2)
2. Autonomy (6)
3. Lecture and demo (6)
4. Demos
5. Test for the Stabilized Variable
6. thanks; schools
7. Lecture and demo, Autonomy (2)

Date: Tue, 24 Dec 1996 07:34:30 -0700
From: Bill Powers <powers_w@FRONTIER.NET>
Subject: Re: Driving blind; wearing coats

[From Bill Powers (961224.0530 MST)]

Bruce Gregory (961223.1550 EST)]

>Suppose I give you the following set of instructions, "Take the Mass.
>Turnpike west. Leave at the Auburn exit, Exit 10. Turn south on Rout 395.
>Cross the Massachusetts-Connecticut border and take exit 97 to
>Putnam..." You now have a map or a model. Surely this is a model
>of the world and surely you would make some use of it in setting
>references levels for perceptions, such as the Ext 10 sign at Auburn.
>This is all I was saying. _How_ you are able to follow my instructions
>using this model is something I leave for those who understand these
>things better than I.

I think it's important to recognize that these apparent prescriptions for action are actually descriptions of perceptions. "Take the Mass. Turnpike west" means "Do what is necessary to be on the Mass Turnpike travelling west," which is a description of an experience, not an action. The actions that will be necessary to create this perceptual result are not predictable. You can't tell the person "Slow down until the oncoming truck has cleared the merge lane." This means that the model is not the sort that converts an action into an outcome: it is not a model of the environment in the sense Hans Blom means.

You say " _How_ you are able to follow my instructions
using this model is something I leave for those who understand these
things better than I." You are as much an authority on this matter as anyone. The point of the HPCT model is exactly to explain HOW we can do

things like following instructions. What you have to do is pay attention the next time you're following instructions, to see how you translate the words into reference images. Try this:

1. Seat yourself in the usual place in front of your computer. Note that if you're already there, the actual perception matches the reference perception so there is no action necessary.

2. Place your right forefinger four inches above the keyboard and on a vertical line equidistant from the 'i' and the 'p' keys.

3. Keeping your forefinger on this line, lower it until it meets substantial resistance.

If you imagine doing this, you will turn the words into a series of reference images. At some point, either while you're setting up the appropriate series of reference images or when you create the corresponding perceptions, you will realize that these instructions result in typing an 'o' (on a standard keyboard). When you _feel_ "substantial resistance," you will _see_ your finger on the 'o' key -- and you will _see_ an 'o' (or a string of o's) appear on the screen if you're in a text editor.

In order to follow verbal instructions we have to convert the words and sentences into imagined perceptions. The words themselves don't cause anything to happen. "Turn south on Route 395" is not as clear as "When you see the sign for the Auburn exit, exit 10, get into the right lane and take the off ramp. When you reach Route 395 at the underpass turn left, which is south." The reason is that when the instruction starts out "turn south," you are not in a position to do that yet, and you have to re-order the images so turning south happens in the right place in the sequence. "South" means "left" if you're heading west (which you have to figure out), but the first thing you have to do, after seeing Exit 10 or the Auburn exit, is to bear right to take the off ramp.

To give good instructions you have to imagine the perceptions as they will occur and describe them in that order. To _follow_ instructions, you have to convert the descriptions into perceptions and set them up in sequence; when you have trouble doing this you know the instructions are poor and you have to ask questions to clarify them. You don't need to ask _how_ to create the perceptions -- that will normally become clear when you get to that point in the actual sequence and see what's going on.

If you understand that instructions are _descriptions of perceptions_ and not _prescriptions for actions_ I don't think you'll have any trouble figuring out how giving and following instructions work.

Best,

Bill P.

Date: Tue, 24 Dec 1996 10:56:44 -0600
From: Bruce Abbott <abbott@CVAX.IPFW.INDIANA.EDU>
Subject: Re: Autonomy

[From Bruce Abbott (961224.1200 EST)]

>>Tracy Harms 1996;12,22.22:00 --

>Bruce Abbott (961223.0645 EST)

>>The scientific traditions have tended to teach causality in terms of
>>positive laws, i.e. in terms of processions of specific transformations.
>>In contrast with this I think of things in terms of the metaphor of
>>prohibitory laws, and thus a nature of limits which constrain what occurs
>>from a manifold of impossible states. But within these limits, what occurs
>>is spontaneous -- and local in its spontaneity. For me there is no
>>exception to the status of quantum mechanics, nor does indeterminacy at
>>larger scales occur only by means of a ripple-effect from sub-atomic
>>quantum states. Rather, quantum indeterminacy is indicative of universally
>>pervasive indeterminacy which has unique properties at every scale.

>What empirical evidence supports this belief in a "universally pervasive
>indeterminacy which has unique properties at every scale"?

Having heard no reply, may I assume that the answer is "none"? (:->

Regards,

Bruce

Date: Tue, 24 Dec 1996 10:22:08 +0000
From: Richard marken <marken@AERO.ORG>
Subject: Re: Lecture and demo

[From Rick Marken (961224.0910 PST)]

Me:

>Does this make sense to you?

Bill Powers (961222.2030 MST) --

>Every even-numbered day except Tuesdays.

So what do you think of my "Control of perception" demo? (Please answer
the day after tommorrow;-))

Best

Rick

Date: Tue, 24 Dec 1996 10:25:14 +0000
From: Richard marken <marken@AERO.ORG>
Subject: Re: Lecture and demo

[From Rick Marken (961224.0915)]

Bruce Gregory (961224.0545 EST)--

> The demos look great!

Thank you. Thank you. I'm glad to see that someone's evaluation skills

are intact on even-numbered Tuesdays;-)

Season's Greetings

Rick

Date: Tue, 24 Dec 1996 12:07:31 -0600
From: Bruce Abbott <abbott@CVAX.IPFW.INDIANA.EDU>
Subject: Re: Lecture and demo

[From Bruce Abbott (961224.1310 EST)]

>Bill Powers (961222.1515 MST) --

>In that figure, there is no "CEV" shown in the environment. Instead, there
>is a collection of individual variables in a sort of cloud. This cloud is
>affected through multiple paths by the output of the control system, and the
>perceptual function is affected in multiple ways by the same cloud of
>variables, shown as "v's".

>When you run this demo, you'll find that all the usual laws of control hold
>true. Varying the reference signal will make the perceptual signal follow
>it. Varying any disturbance affecting the v's will result in variations of
>the output that keep the perceptual signal from changing as much as it would
>change if there were no control; that is, the effect of the output on the
>perceptual signal will be proportional and opposite to the effects of
>variations in the disturbance, given a constant reference signal.

>The only thing missing from this demo is any environmental variable that is
>the "correlate" of the perceptual signal.

I can see that this is true without having to run the demo. Perceptual variables may be constructed in various ways from the input, and the result may have no direct correspondence to any physical variable. However, the particular combination of physical variables represented in the perceptual input function is an objectively measurable quantity, given that one knows the formula. This is exactly what I expect based on my conception of how control systems work.

The original issue under discussion was whether a simulation-based system in which CVm matched CV (i.e., perfect modeling) could be considered to be controlling CV when running in blind mode. Martin's elegant contribution was to suggest that in this case the CV would be "stabilized" but not controlled. My suggestion was that, so long as CVm remained a pefect match to CV, it would not be possible to tell the difference, because mathematically the effects of system output and disturbance on CV would be identical to those on CVm.

Rick's response was to deny that the distinction Martin was making between "stabilized" and "controlled" was useful. CV, he said, either is controlled or is not controlled. I noted in response that CV (the environmental correlate of p in the "sighted" condition) is not what is actually controlled in a control system, p is. However I granted that if the function relating CV to p is stable, then CV will be controlled along with p. Rick apparently saw something wrong with this statement; after alluding for the umteenth time that I don't quite "get it," he offered to demonstrate why CV is "really" under control, and offered to provide a lecture and demo

that would show why Martin and I were wrong. The said demo and lecture were provided, and both Martin and I independently concluded that they are not relevant to the issue. While they show that the environmental correlate of p is controlled when there is a reliable function relating p to CV, they fail to demonstrate any difference between the case in which CV is directly controlled and the case in which CV is stabilized by model-based control when CVm is a perfect model of CV. Instead, the lecture and demo show only what I had granted in the first place: that when p is a reliable function of CV, that CV is controlled with p.

What I have heard since has only strengthened my conviction that Martin and I are correct in our analysis. I don't disagree with what Rick has asserted in his lecture, but then I never did. It is an entirely different assertion from the one we were actually debating.

Regards,

Bruce

Date: Tue, 24 Dec 1996 12:18:40 -0600
From: Bruce Abbott <abbott@CVAX.IPFW.INDIANA.EDU>
Subject: Re: Demos

[From Bruce Abbott (961224.1320)]

>>Me (961223.0835 EST):

>>By a happy coincidence I just managed to get a new computer up and running
>>with Windows 95, and was able to run through the JAVA demos. Great stuff,
>>Rick! You'll have to show me how to do that.

>Rick Marken

>

>>Bruce Gregory (961224.0545 EST)

>> The demos look great!

>Rick Marken (961224.0915)

>Thank you. Thank you. I'm glad to see that someone's evaluation skills
>are intact on even-numbered Tuesdays;-)

Bruce, I think you can see why I don't try to curry favor more often around here: when I try it, it doesn't seem to work! (:-(

(;->

Regards,

Bruce

Date: Tue, 24 Dec 1996 12:07:14 +0000
From: Richard marken <marken@AERO.ORG>

Subject: Test for the Stabilized Variable

[From Rick Marken (961224.1100 PST)]

Bruce Abbott (961224.1310 EST)--

> The original issue under discussion was whether a simulation-based
> system in which CVm matched CV (i.e., perfect modeling) could be
> considered to be controlling CV when running in blind mode...

> Rick's response was to deny that the distinction Martin was making
> between "stabilized" and "controlled" was useful...I noted in
> response that CV (the environmental correlate of p in the "sighted"
> condition) is not what is actually controlled in a control system,
> p is. However I granted that if the function relating CV to p is
> stable, then CV will be controlled along with p. Rick apparently
> saw something wrong with this statement;

Maybe I missed your (and Martin's) point. It seemed to me that you guys were saying that, in general, perception is controlled and the environmental correlate of perception (the CV) is stabilized. The stuff about the reliability of the function relating CV to p is irrelevant to the quesiton of whether CV is stabilized or controlled. If p is not a function of CV then CV is not the variable under control (CV is not the CV -- controlled variable); some other CV (the one that p is actually a function of) is the controlled variable (CV).

If you agree than, in a control loop, CV is controlled when when p (the function of CV) is controlled, then we have no problem. The term "stabilization" is only appropriatly applied to the (impossible) situation where the control system has _no_ perceptual representation of the CV but is able to generate outputs (based on a model of everything that is happening, will happen and can happen to that variable) that keep CV in a stable state. If systems were able to generate model based outputs that were this successful at stabilizing environmental variables then it would, indeed, be impossible to distinguish stabilized variables (SVs) from controlled variables (CVs). Fortunately, in the real world (where Martin spends so little of his time;-)) The Test makes it easy to distinguish SVs from CVs.

Best

Rick

Oh. And thanks for the nice words about the demos, Bruce. I should have ackowledged them but I was too busy thinking that you might actually believe that CVs are stabilized by computed output and not controlled;-)

Date: Tue, 24 Dec 1996 12:28:53 -0700
From: Bill Powers <powers_w@FRONTIER.NET>
Subject: Re: Autonomy

[From Bill Powers (961224.1120 MST)]

Bruce Abbott (961224.1200 EST)]

>>What empirical evidence supports this belief in a "universally pervasive
>>indeterminacy which has unique properties at every scale"?
>
>Having heard no reply, may I assume that the answer is "none"? (:->

No.

However, "universally pervasive indeterminacy" would be an overstatement of my view. It seems to imply that you can't predict anything, which isn't true. I would say that indeterminacies can be found on every scale. Some obvious ones are the phenomena of chaos, where an orderly causal system has states which are literally indeterminate.

Best,

Bill P.

Date: Tue, 24 Dec 1996 12:28:58 -0700
From: Bill Powers <powers_w@FRONTIER.NET>
Subject: Re: Lecture and demo

[From Bill Powers (961224.1145 MST)]

Bruce Abbott (961224.1310 EST)--

>Perceptual
>variables may be constructed in various ways from the input, and the
>result may have no direct correspondence to any physical variable.
>However, the particular combination of physical variables represented in
>the perceptual input function is an objectively measurable quantity, given
>that one knows the formula. This is exactly what I expect based on my
>conception of how control systems work.

The term "objective" is questionable here, because there's nothing in the environment that corresponds to the formula. As long as the control system and the observer use the same formula, they both perceive the same thing, but there is nothing _independent of the observer(s)_ that indicates what the right formula is. I've always thought of the word objective as meaning something independent of _any_ observer. How do you define it?

>The original issue under discussion was whether a simulation-based system
>in which CVm matched CV (i.e., perfect modeling) could be considered to be
>controlling CV when running in blind mode. Martin's elegant contribution
>was to suggest that in this case the CV would be "stabilized" but not
>controlled. My suggestion was that, so long as CVm remained a pefect
>match to CV, it would not be possible to tell the difference, because
>mathematically the effects of system output and disturbance on CV would be
>identical to those on CVm.

When I speak of control as resistance to disturbance, I have in mind an _arbitrary_ disturbance. By that I mean a disturbance which the system should be physically capable of resisting (neither too large nor too fast) but of an unpredictable waveform and time of occurrance. I want to be able to walk up to the system at any time, apply a disturbance of my choosing to the controlled variable, and see the action of the system change immediately to oppose the effect of my disturbance. A real control system can do this.

In thought-experiments, you have control of all the conditions both inside and outside the system. Your example might have used a disturbance of the form 10*sin(t/2)*(1 - exp(-0.01t)) + 20*t^-(2/7). If you say that the model inside the system contains exactly the same form of disturbance, then it will be true that CV and CVm will behave exactly the same way. But so what? It's a put-up job all the way. The critical question is how the model came to have exactly the same form of disturbance in it that the external world has.

Also, don't forget that the entire question may be moot: if people can't go on "stabilizing" CV in the absence of input, even with just a simple sine-wave disturbance, then obviously for them CV does not equal CVm, and the whole model is incorrect. I suspect that when the results come in from RunBlnd4, we will see that people can't even control or stabilize a controlled variable when the reference level is constant and the only disturbance is a simple, slow, completely predictable sine wave. If that proves to be true, my interest in a thought-experiment that assumes what seems to me an impossibility will wane very rapidly. It's already pretty low.

>Rick's response was to deny that the distinction Martin was making between
>"stabilized" and "controlled" was useful. CV, he said, either is
>controlled or is not controlled.

I would tend to agree with Rick, because of my definition of control given above. Remember that as far as the observer is concerned, what is controlled is ONLY the CV. The idea that this CV is represented by a perceptual signal inside the other system is theoretical. We can observe CV, but not p. When we apply a disturbance, we apply it to CV, not to p. The action that opposes the effect of the disturbance acts on CV, not p. The Test does not involve p at all. It involves only observables -- i.e., the observer's perceptions.

The observations have priority; the model comes second, and its only reason for existence is to explain the observations. When you fool around with thought-experiments too much, you tend to get the priorities reversed.

Best,

Bill P.

Date: Tue, 24 Dec 1996 12:28:55 -0700
From: Bill Powers <powers_w@FRONTIER.NET>
Subject: Re: Lecture and demo

[From Bill Powers (961224.1130 MST)]

Rick Marken (961224.0910 PST)]

>So what do you think of my "Control of perception" demo? (Please answer
>the day after tommorrow;-))

It's a nice idea, but I think you need to discriminate more sharply among the alternative perceptions. When I tried keeping width constant, the "shape" trace (y/x) varied less than my "width" trace, because I wasn't doing too well at controlling width. And I'm no good at all at controlling "perimeter." I'm best at keeping the figure square, because I can perceive deviations from squareness very clearly.

Suppose you started with an "H" in which the vertical scale, the horizontal scale, and the tilt of the sides of the H around the place where they're attached to the crossbar could be varied by three disturbances. A person could pick the height, the width, or the resemblance to either the letter H or the letter A as the controlled variable, with the handle affecting the same three variables that the disturbances affect.

In general, for this demo to work best, each of the possible controlled variables should be easily seen and controlled.

Best,

Bill P.

Date: Tue, 24 Dec 1996 12:56:03 -0700
From: "T. B. Harms" <tbh@TESSER.COM>
Subject: Re: Autonomy

*From Tracy Harms 1996;12,24.13:00

>Bruce Abbott (961224.1200 EST)
>>Bruce Abbott (961223.0645 EST)
>>What empirical evidence supports this belief in a "universally pervasive
>>indeterminacy which has unique properties at every scale"?
>
>Having heard no reply, may I assume that the answer is "none"? (:->

That way of asking the question tends to result in a loss of my interest. Because these are high-level metaphysical claims they are only susceptible to logical challenges, not empirical ones. (I can imagine no laboratory testing which would not be equally compatable with determinism and indeterminism both.) Furthermore, a request for empirical evidence strikes me as misplaced attention: Instead of asking for something useless we should be look for a history of clever and cogent attempts at exposing flaws and inadequacies.

The strength I see to indeterminism over determinism lies something like this: Rather than having two physical claims about the world (sub-atomic changes are indeterministic + larger changes are deterministic) we have a single claim. More importantly, if changes are rigidly deterministic we can expect that larger-scale changes will be able to be explained in terms of our understanding of the causality of small-scale change; but if change is inherently indeterminate at all scales, causal limits may differ for every subject matter such that distinct explanations must be forged for each. The intractability of explaining all phenomena by reductionist appeal to physics is more suggestive of indeterminism than determinism.

Tracy Bruce Harms tbh@tesser.com
Boulder, Colorado caveat lector!

Date: Tue, 24 Dec 1996 13:12:08 -0700
From: "T. B. Harms" <tbh@TESSER.COM>

Subject: Re: Autonomy

*From Tracy Harms 1996;12,24.13:11

Bill Powers (961224.1120 MST)

>However, "universally pervasive indeterminacy" would be an overstatement of
>my view. It seems to imply that you can't predict anything, which isn't
>true. I would say that indeterminacies can be found on every scale. Some
>obvious ones are the phenomena of chaos, where an orderly causal system has
>states which are literally indeterminate.

"Pervasive" is meant to indicate intrinsic or fundamental, not infinite.
(Note this: a world where indeterminacy is finite is indeterministic,
whereas a world where determinacy is finite is also indeterministic.)

The difference between determinist and non-determinist frameworks is
especially interesting in regard to the sort of explanation each requires
in the face of phenomena of chaos. Indeterminism need only explain the
emergence of indeterminacy in relation to the regularities which contrast
with it at the same scale where it is seen. (The recent mathematics of
"chaos" involve just this sort of explanation.) A full-fledged
determinism, on the other hand, needs to come up with an explanation which
shows a strict consequence somewhere else which would expose the apparent
indeterminacy as illusory -- and that's a far taller bill to fill.

Tracy Bruce Harms tbh@tesser.com
Boulder, Colorado caveat lector!

Date: Tue, 24 Dec 1996 15:12:15 -0500
From: Bruce Gregory <gregory@CF4.HARVARD.EDU>
Subject: Re: Lecture and demo

[From Bruce Gregory (961224.1510 EST)]

Bill Powers (961224.1145 MST)]

> The observations have priority; the model comes second, and its only reason
> for existence is to explain the observations. When you fool around with
> thought-experiments too much, you tend to get the priorities reversed.

I would like to add my fervent agreement. I suggest we adopt the
thought as a New Year's resolution!

Bruce Gregory

Date: Tue, 24 Dec 1996 15:20:36 -0500
From: Bruce Gregory <gregory@CF4.HARVARD.EDU>
Subject: Re: Driving blind; wearing coats

[From Bruce Gregory 9961224.1520 EST)]

Bill Powers (961224.0530 MST)]
>
> Bruce Gregory (961223.1550 EST)]
>
> >Suppose I give you the following set of instructions, "Take the Mass.
> >Turnpike west. Leave at the Auburn exit, Exit 10. Turn south on Rout 395.
> >Cross the Massachusetts-Connecticut border and take exit 97 to
> >Putnam..." You now have a map or a model. Surely this is a model
> >of the world and surely you would make some use of it in setting
> >references levels for perceptions, such as the Ext 10 sign at Auburn.
> >This is all I was saying. _How_ you are able to follow my instructions
> >using this model is something I leave for those who understand these
> >things better than I.
>
> I think it's important to recognize that these apparent prescriptions for
> action are actually descriptions of perceptions. "Take the Mass. Turnpike
> west" means "Do what is necessary to be on the Mass Turnpike travelling
> west," which is a description of an experience, not an action. The actions
> that will be necessary to create this perceptual result are not predictable.
> You can't tell the person "Slow down until the oncoming truck has cleared
> the merge lane." This means that the model is not the sort that converts an
> action into an outcome: it is not a model of the environment in the sense
> Hans Blom means.

I agree. That's why I said "setting reference levels for perceptions". Apparently
ifrom what I ubderstand of model-based control theories, the model is used to
set reference levels for perceptions of actions regardless of their effects on
the CV.

> You say " _How_ you are able to follow my instructions
> using this model is something I leave for those who understand these
> things better than I." You are as much an authority on this matter as
> anyone. The point of the HPCT model is exactly to explain HOW we can do
> things like following instructions. What you have to do is pay attention the
> next time you're following instructions, to see how you translate the words
> into reference images.
>
> To give good instructions you have to imagine the perceptions as they will
> occur and describe them in that order. To _follow_ instructions, you have to
> convert the descriptions into perceptions and set them up in sequence; when
> you have trouble doing this you know the instructions are poor and you have
> to ask questions to clarify them. You don't need to ask _how_ to create the
> perceptions -- that will normally become clear when you get to that point in
> the actual sequence and see what's going on.
>
> If you understand that instructions are _descriptions of perceptions_ and
> not _prescriptions for actions_ I don't think you'll have any trouble
> figuring out how giving and following instructions work.

Thanks. A very clear set of instructions. I had no trouble with the
perceptions ;-)

Bruce Gregory

Date: Tue, 24 Dec 1996 15:08:38 -0500

From: Bruce Gregory <gregory@CF4.HARVARD.EDU>
Subject: Re: Autonomy

[From Bruce Gregory (961224.1510 EST)]

Bill Powers (961224.1120 MST)]
>
> Bruce Abbott (961224.1200 EST)]
>
> >>What empirical evidence supports this belief in a "universally pervasive
> >>indeterminacy which has unique properties at every scale"?
> >
> >Having heard no reply, may I assume that the answer is "none"? (:->
>
> No.
>
> However, "universally pervasive indeterminacy" would be an overstatement of
> my view. It seems to imply that you can't predict anything, which isn't
> true. I would say that indeterminacies can be found on every scale. Some
> obvious ones are the phenomena of chaos, where an orderly causal system has
> states which are literally indeterminate.

I think the problem is "unique properties at every scale." Isn't chaos scale invariant?

Bruce Gregory

Date: Tue, 24 Dec 1996 13:38:59 -0700
From: "T. B. Harms" <tbh@TESSER.COM>
Subject: thanks; schools

*from Tracy Harms 1996;12,24.13:30

I'd just like to offer my general thanks to the participants in this group, and to cybernetic theorists more generally, for showing me a field where my interests in epistemology and engineering appear to come into closer union than I've found elsewhere. My love of philosophy has always been driven by my sense that general understanding is always relevant to specific situations, but there is something special about the way system theory ties epistemology with operating instances. I get a great deal out of the discussion which occurs here on the Control Systems Group Network.

I'm curious regarding the academic situation for control theory, cybernetics, and system science. Are there universities which stand as especially good in these fields of study? What trends or concentrations of scholarship can be found? Are there places where things are especially "happening," or are the hot studies happening with a scatteredness which elevates no school or foundation above others?

Tracy Bruce Harms tbh@tesser.com
Boulder, Colorado caveat lector!

Date: Tue, 24 Dec 1996 16:09:13 -0500
From: Bruce Gregory <gregory@CF4.HARVARD.EDU>
Subject: Re: Autonomy

[From Bruce Gregory (961224.1610 EST)]

Tracy Harms 1996;12,24.13:00

> The strength I see to indeterminism over determinism lies something like
> this: Rather than having two physical claims about the world (sub-atomic
> changes are indeterministic + larger changes are deterministic) we have a
> single claim. More importantly, if changes are rigidly deterministic we
> can expect that larger-scale changes will be able to be explained in terms
> of our understanding of the causality of small-scale change; but if change
> is inherently indeterminate at all scales, causal limits may differ for
> every subject matter such that distinct explanations must be forged for
> each. The intractability of explaining all phenomena by reductionist
> appeal to physics is more suggestive of indeterminism than determinism.

It may be a good idea to keep in mind that both determinism and
indeterminism are properties of models. Whenever we push models far
beyond the realms where they have been tested we are in danger of
going astray.

Bruce Gregory

Date: Tue, 24 Dec 1996 18:55:44 +0000
From: Richard marken <marken@AERO.ORG>
Subject: Re: Lecture and demo, Autonomy

[From Rick Marken (961224.1750 PST)]

Bill Powers (961224.1130 MST) re: Control of Perception demo

> It's a nice idea, but I think you need to discriminate more
> sharply among the alternative perceptions. When I tried keeping
> width constant, the "shape" trace (y/x) varied less than my
> "width" trace, because I wasn't doing too well at controlling
> width.... In general, for this demo to work best, each of the
> possible controlled variables should be easily seen and controlled.

I agree completely. Thanks for the very helpful ideas. I know
exactly what you mean and I will work on creating an improved
version of the demo based on your suggestions.

Tracy Harms (1996;12,24.13:00) --

> The strength I see to indeterminism over determinism lies
> something like this..

I think that casting the discussion of autonomy in the context of
an argument over determinism vs indeterminism misses one of the main
contributions that PCT might be able to make to the discussion. PCT
shows that the determinism that exists in closed loop systems, like
organisms, is quite different than the kind that exists in open loop
systems, such as inanimate objects.

Open-loop determinism is good old "cause-effect"; closed-loop
determinism, when the feedback in the loop is negative and the gain
is high, is "control".

The difference between objects that respond to causes and those that
control their inputs is illustrated in my "Detection of Purpose" demo.
The difference is that the cause-effect objects "go with the flow" (your
disturbance); the controlling objects "do their own thing" (despite your
disturbance). It seems to me that simple, closed-loop control systems
exhibits many of the charateristics that we call "autonomy", even though
such systems are not "indeterminate".

Best

Rick

Date: Tue, 24 Dec 1996 21:02:55 -0500
From: Bruce Gregory <gregory@CF4.HARVARD.EDU>
Subject: Re: Lecture and demo, Autonomy

[From Bruce Gregory (961224.2100 EST)]

Rick Marken (961224.1750 PST)

> I think that casting the discussion of autonomy in the context of
> an argument over determinism vs indeterminism misses one of the main
> contributions that PCT might be able to make to the discussion. PCT
> shows that the determinism that exists in closed loop systems, like
> organisms, is quite different than the kind that exists in open loop
> systems, such as inanimate objects.
>
> Open-loop determinism is good old "cause-effect"; closed-loop
> determinism, when the feedback in the loop is negative and the gain
> is high, is "control".
>
> The difference between objects that respond to causes and those that
> control their inputs is illustrated in my "Detection of Purpose" demo.
> The difference is that the cause-effect objects "go with the flow" (your
> disturbance); the controlling objects "do their own thing" (despite your
> disturbance). It seems to me that simple, closed-loop control systems
> exhibits many of the charateristics that we call "autonomy", even though
> such systems are not "indeterminate".

Nicely said. I feel exactly the same way. The autonomous nature of
living control systems is quite outside the determinism-indeterminism
dichotomy. Radioactive decay is indeterminate, but hardly a model I
want to emulate!

Bruce Gregory

End of CSGNET Digest - 24 Dec 1996 to 25 Dec 1996
****

Date: Thu, 26 Dec 1996 08:00:15 +1800
Subject: CSGNET Digest - 25 Dec 1996 to 26 Dec 1996

There are 2 messages totalling 187 lines in this issue.

Topics of the day:

1. Lecture and demo
2. Motivation on Trial

Date: Wed, 25 Dec 1996 15:38:17 -0500
From: Bruce Abbott <abbott@CVAX.IPFW.INDIANA.EDU>
Subject: Re: Lecture and demo

[From Bruce Abbott (961225.1540 EST)]

>Bill Powers (961224.1145 MST) --

>>Bruce Abbott (961224.1310 EST)

>>Perceptual
>>variables may be constructed in various ways from the input, and the
>>result may have no direct correspondence to any physical variable.
>>However, the particular combination of physical variables represented in
>>the perceptual input function is an objectively measurable quantity, given
>>that one knows the formula. This is exactly what I expect based on my
>>conception of how control systems work.

>The term "objective" is questionable here, because there's nothing in the
>environment that corresponds to the formula. As long as the control system
>and the observer use the same formula, they both perceive the same thing,
>but there is nothing _independent of the observer(s)_ that indicates what
>the right formula is. I've always thought of the word objective as meaning
>something independent of _any_ observer. How do you define it?

I define "objective" as public and reproducable. That is, if you and I both
conduct our observations under the same conditions, our observations will
agree, within the limits of experimental error. In this sense the
difference in the areas of two given rectangles are as objective their
lengths, widths, areas, or the temperature of the paper on which they are drawn.

>>The original issue under discussion was whether a simulation-based system
>>in which CVm matched CV (i.e., perfect modeling) could be considered to be
>>controlling CV when running in blind mode. Martin's elegant contribution
>>was to suggest that in this case the CV would be "stabilized" but not
>>controlled. My suggestion was that, so long as CVm remained a pefect
>>match to CV, it would not be possible to tell the difference, because
>>mathematically the effects of system output and disturbance on CV would be
>>identical to those on CVm.

>When I speak of control as resistance to disturbance, I have in mind an
>_arbitrary_ disturbance. By that I mean a disturbance which the system
>should be physically capable of resisting (neither too large nor too fast)
>but of an unpredictable waveform and time of occurrance. I want to be able
>to walk up to the system at any time, apply a disturbance of my choosing to
>the controlled variable, and see the action of the system change immediately
>to oppose the effect of my disturbance. A real control system can do this.

Yes. Hence the distinction to be made between controlled and stabilized. A variable may be stablized against some predictable disturbance waveform without being controlled.

>In thought-experiments, you have control of all the conditions both inside >and outside the system. Your example might have used a disturbance of the >form 10*sin(t/2)*(1 - exp(-0.01t)) + 20*t^-(2/7). If you say that the model >inside the system contains exactly the same form of disturbance, then it >will be true that CV and CVm will behave exactly the same way. But so what? >It's a put-up job all the way. The critical question is how the model came >to have exactly the same form of disturbance in it that the external world has.

This example was intended to provide a starting point for discussion -- an _optimal situation_ for model-based control. Thus far I have not been able to proceed beyond trying to get the participants in this debate to agree to what you just stated -- that CV and Cm will behave in exactly the same way, so that it would be impossible to tell, just by observing this "put up" system, whether CV is or is not under control. Lacking agreement on this point (which I thought was an obvious one that would meet no opposition), I have been unable to proceed further.

>Also, don't forget that the entire question may be moot: if people can't go >on "stabilizing" CV in the absence of input, even with just a simple >sine-wave disturbance, then obviously for them CV does not equal CVm, and >the whole model is incorrect. I suspect that when the results come in from >RunBlnd4, we will see that people can't even control or stabilize a >controlled variable when the reference level is constant and the only >disturbance is a simple, slow, completely predictable sine wave. If that >proves to be true, my interest in a thought-experiment that assumes what >seems to me an impossibility will wane very rapidly. It's already pretty low.

It would be my guess that people will tend to do poorly at this task, even with considerable practice at it, because it demands not only variation of the correct approximate form and amplitude (which I suspect people can learn to produce fairly well, although perhaps not via a mouse), but also (and critically) the correct phase (even relatively small phase errors can produce high rms errors), and this must be maintained over several cycles of the disturbance waveform. However, I've already noted biological systems in which internally-generated regular cycles -- the so-called biological clocks -- do produce excellent stabilization against some forms of predictable disturbance. Thus I think it would be a mistake to conclude that poor performance under the conditions imposed by RunBlnd4 would rule out simulation-based stabilization as an operative feature of biological systems. If people do poorly, all you will know is that they are not good at using simulation-based control in this situation.

>>Rick's response was to deny that the distinction Martin was making between >>"stabilized" and "controlled" was useful. CV, he said, either is >>controlled or is not controlled.

>I would tend to agree with Rick, because of my definition of control given >above. Remember that as far as the observer is concerned, what is controlled >is ONLY the CV. The idea that this CV is represented by a perceptual signal >inside the other system is theoretical. We can observe CV, but not p. When >we apply a disturbance, we apply it to CV, not to p. The action that opposes >the effect of the disturbance acts on CV, not p. The Test does not involve p >at all. It involves only observables -- i.e., the observer's perceptions.

But this is not an "either - or" proposition. A variable can be stable although not controlled. The question then be comes whether the observed stability is a result of control or of some other process. So I'm afraid that I'm going to have to disagree with both you _and_ Rick on this one, and continue to side with Martin. The distinction _is_ useful.

The Test can, of course, distinguish between a system stabilized by control or by some other means. This is important, but not relevant to the issue at hand, which concerns the degree to which an internal model of the predictable portion of the disturbance acting on a CV can be employed to reduce the variation in the CV, thus conferring upon it a degree of stability, although not control.

Regards,

Bruce

Date: Wed, 25 Dec 1996 15:48:42 EST
From: "Kenneth J. Kitzke" <71042.2733@COMPUSERVE.COM>
Subject: Motivation on Trial

We wish all PCT believers a very Happy, Prosperous and "Purposeful" New Year!

Although we mostly lurk and never get much of a chance to promote PCT, I thought you might enjoy a feeble attempt that Chris and I will be doing January 15, 1997. In fact, if any of you have PCT insights or group demos or stories regarding motivating others or ourselves, we would like to hear from you and we'll use it in this speech or "trial" in State College, Pennsylvania.

<this is paraphrased from a flyer of the Society of Human Resource Management, Chapter #265, Human Resource Association of Centre County (near Penn State University).

NEWS FLASH!

MOTIVATION ON TRIAL

In one of the strangest cases to hit the State College area in years, a local manager has been charged with failing to motivate her employees. The manager is pleading innocent based on Perceptual Control Theory, a radical new scientific discovery in the field of human behavior that governs all relationships between people.

A special prosecutor, Christopher Carrot, has been named. The presiding Judge will be the Honorable Kenneth J. Stick. Courtroom seating is limited. Final word has not yet been received form CNN on their possible live coverage of this historic hearing.

Carrot and Stick have tried to downplay rumors that the verdict may reveal the truth about motivation and change the role of management for ever.

About the Speaker:

Judge Stick is actually Kenneth J. Kitzke, a real judge for the Pennsylvania Quality Leadership Awards and a member of the Control Systems Group. Helping him as Christopher Carrot is his son Chris, an associate in Quality Dynamics, a quality management system consulting firm in Pittsburgh. While this luncheon

program will be lighthearted, Ken takes quality, performance, management and motivation seriously.

Ken was an original examiner for the Malcolm Baldrige National Quality Award. The quality system he managed won a MBNQA along with Motorola in 1988, the first year it was bestowed. Founded in 1987, Quality Dynamics is the retained quality consultant for some of the most successful companies in Western Pennsylvania including Adtranz, Bricmont, Medrad, Respironics and Thrift Drug (now a unit of Eckert owned by J. C. Penney Company).>

BTW, I'll be doing something new for New Year's Eve. It's called First Night and we understand many cites are controlling their collective perceptions this way. Our Pittsburgh cultural district will be offering numerous musical entertainment events and we'll be down there eating and enjoying it all with good friends to welcome 1997. What will you be doing for New Year's Eve?

Blessings,

Ken Kitzke and Chris

End of CSGNET Digest - 25 Dec 1996 to 26 Dec 1996
****

Date: Fri, 27 Dec 1996 08:00:17 +1800
Subject: CSGNET Digest - 26 Dec 1996 to 27 Dec 1996

There are 10 messages totalling 716 lines in this issue.

Topics of the day:

1. Lecture and demo (4)
2. Autonomy
3. Observations come first (2)
4. Lecture and demo. motivation
5. Webchat Broadcasting System
6. Lecture and demo, Autonomy

Date: Thu, 26 Dec 1996 10:15:29 -0600
From: Bruce Abbott <abbott@CVAX.IPFW.INDIANA.EDU>
Subject: Re: Lecture and demo

[From Bruce Abbott (961226.1115 EST)]

To understand why Rick's lecture and demo are irrelevant to the issue they were recently offered to address, it helps first to understand what issue Rick's lecture and demo does address. The demo is a JAVA recode of an earlier version Rick wrote in Hypercard for the Macintosh (and which I had seen over a year ago, if memory serves). It was written to demonstrate that the person controls the environment, rather than the other way around. The same environmental "stimulus" (the rectangle) is always present, yet the participant can choose to control different perceptions related to that stimulus. It is thus evident that the "stimulus" does not control behavior: the behavior is not "stimulus bound," since different behaviors can occur in "response" to the same stimulus, and for reasons that lie inside the organism.

But this is not the issue presently at hand. No one is arguing here that the environment controls behavior. What is being argued is that there are conditions under which an environmental variable can be stabilized even though it is not being controlled, by actually controlling another perception based on an internal model of the CV, which we have been calling CVm. The conditions are that the controller has an accurate model of the EFF (environmental feedback function) relating controller output to change in CV, that the disturbance to CV is predictable, and that the system has an accurate model of the disturbance.

To the extent that these conditions hold or fail, simulation-based stabilization will succeed or fail. Even where the disturbance waveform is only partially predictable, if the model of this predictable component is adequate, CV will be partially stabilized (i.e., will vary less than it otherwise would).

No one is claiming that this form of stabilization will work against unpredictable disturbances, or that it is as good as control (it isn't) when the latter is possible.

So Rick's lecture and demo is a bit like the following:

Question: Why does a fireman wear red suspenders?
Rick's reply: To get to the other side.

It's a great answer to another question (Why does the chicken cross the road?), but bears no relation to the question that was actually asked.

Regards,

Bruce

Date: Thu, 26 Dec 1996 10:50:40 -0600
From: Bruce Abbott <abbott@CVAX.IPFW.INDIANA.EDU>
Subject: Re: Autonomy

[From Bruce Abbott (961226.1150 EST)]

>Tracy Harms 1996;12,24.13:00

>>Bruce Abbott (961224.1200 EST)
>>>Bruce Abbott (961223.0645 EST)
>>>What empirical evidence supports this belief in a "universally pervasive
>>>indeterminacy which has unique properties at every scale"?

>>Having heard no reply, may I assume that the answer is "none"? (:->

>That way of asking the question tends to result in a loss of my interest.
>Because these are high-level metaphysical claims they are only susceptible
>to logical challenges, not empirical ones. (I can imagine no laboratory
>testing which would not be equally compatable with determinism and
>indeterminism both.)

That way of _answering_ the question tends to result in a loss of _my_ interest. If I understand correctly, the indeterminism found at the quantum level is revealed by empirical demonstration; that is why it is of such interest to scientists. If your claim is not susceptible to any

conceivable empirical test, then it is not a scientific theory. In fact, I might claim that it is an anti-scientific theory in that it appears to presuppose limits to what science will be able to discover about the workings of the human mind, and thus tends to remove the motivation to do the research.

>Furthermore, a request for empirical evidence strikes
>me as misplaced attention: Instead of asking for something useless we
>should be look for a history of clever and cogent attempts at exposing
>flaws and inadequacies.

I am all in favor doing the latter whether the theory under scrutiny is scientific or purely philosophical. But I was under the impression that you were offering a view with testable empirical consequences. If it has none, then as a scientist I find it uninteresting.

Regards,

Bruce

Date: Thu, 26 Dec 1996 11:13:47 -0600
From: Bruce Abbott <abbott@CVAX.IPFW.INDIANA.EDU>
Subject: Re: Observations come first

[From Bruce Abbott (961226.1215 EST)]

>Bruce Gregory (961224.1510 EST) --

>>Bill Powers (961224.1145 MST)

>> The observations have priority; the model comes second, and its only reason
>> for existence is to explain the observations.

I've already noted observations in which simulation-based stabilization may be at work. Generally speaking, if it _can_ be done to advantage, then there will probably be some organism that does it. This makes it worth exploring whether such a tactic can be made to work and, if so, what its requirements are.

>>When you fool around with
>> thought-experiments too much, you tend to get the priorities reversed.

>I would like to add my fervent agreement. I suggest we adopt the
>thought as a New Year's resolution!

Meaning what? That one should not examine the implications of one's models? What is "fooling around with thought experiments too much"? Tell that to Einstein.

I think that there is quite a bit to be learned from examining the logical (and mathematical) implications of one's assumptions, to see where they lead. In this case I think it might save quite a bit of empirical work, because what can be shown to be logically true (or false) need not be demonstrated empirically. Furthermore, you are not going to be able to empirically demonstrate that simulation-based stabilization is _not_ employed by living organisms for _any_ purpose merely by showing that human beings are not able to use it effectively to counteract the influence of a

sine-wave disturbance over several cycles.

Regards,

Bruce

Date: Thu, 26 Dec 1996 10:39:03 +0000
From: Richard marken <marken@AERO.ORG>
Subject: Re: Lecture and demo. motivation

[From Rick Marken (961226.0930)]

Bruce Abbott (961226.1115 EST)]

> It [Marken's demo] was written to demonstrate that the person
> controls the environment, rather than the other way around. The
> same environmental "stimulus" (the rectangle) is always present,
> yet the participant can choose to control different perceptions
> related to that stimulus. It is thus evident that the "stimulus"
> does not control behavior: the behavior is not "stimulus bound,"
> since different behaviors can occur in "response" to the same
> stimulus, and for reasons that lie inside the organism.

Nothing personal (I know the system concept for which you control)
but this has to be the worst restatement of the aim of my demo that
I have ever heard. The aim was not to show that "the person controls the
environment, rather than the other way around". Nor was the aim
to show that "behavior is not stimulus bound" (whatever that means). The
aim was what I said it was -- to show that control systems
control perceptual representations of objective states of affairs.
It did this by showing that a person (control system) can control
different representations of the _same_ objective state of affairs.

> But this is not the issue presently at hand...What is being argued
> is that there are conditions under which an environmental variable
> can be stabilized even though it is not being controlled, by
> actually controlling another perception based on an internal
> model of the CV, which we have been calling CVm.

I have never disputed this. Here's one for your PCT scrapbook:

In Bruce Abbott's ridiculously unrealistic model-based controller,
CV is _stabilized_ by computed outputs; it is not
controlled.
Richard S. Marken

You may copy the above sentence and keep it close to your heart if
it will help you move on to the next phase of development in your
understanding of PCT.

What I have disputed is the idea that the CV in a _normal_ control loop
is _stabilized_. Under normal cirsumstances, the CV is controlled, not
stabilized.

I will also note that if your model based controller were able to
predict every arbitrary disturbance that were being applied to the CV
(that is, if your model based controlled lived in the Twilight Zone)

then it would be impossible for an observer to tell that CV is being stabilized rather than controlled. This is possibly the most useless observation that could possibly be made about model-based control.

> No one is claiming that this form of stabilization will work
> against unpredictable disturbances, or that it is as good as
> control (it isn't) when the latter is possible.

Now this is actually a useful statement. The question, then, is "to what degree do organisms use model-based control when they lose input". Since you [Bruce Abbott (961225.1540 EST)] have declared that this cannot, in general, be determined by experiment, I guess you are in the cat-bird seat on this issue, eh?;-)

Kenneth J. Kitzke (961225) --

> a local manager has been charged with failing to motivate her
> employees. The manager is pleading innocent based on Perceptual
> Control Theory

The verdict has to be that the manager is innocent, right? PCT shows that you can't really "motivate people" (set their references for them), right? I find for the defendant;-)

Best

Rick

Date: Thu, 26 Dec 1996 19:52:31 -0500
From: Bruce Abbott <abbott@CVAX.IPFW.INDIANA.EDU>
Subject: Re: Lecture and demo

[From Bruce Abbott (961226.1955 EST)]

>Rick Marken (961226.0930)]

>>Bruce Abbott (961226.1115 EST)]

>> It [Marken's demo] was written to demonstrate that the person
>> controls the environment, rather than the other way around. The
>> same environmental "stimulus" (the rectangle) is always present,
>> yet the participant can choose to control different perceptions
>> related to that stimulus. It is thus evident that the "stimulus"
>> does not control behavior: the behavior is not "stimulus bound,"
>> since different behaviors can occur in "response" to the same
>> stimulus, and for reasons that lie inside the organism.

>Nothing personal (I know the system concept for which you control)
>but this has to be the worst restatement of the aim of my demo that
>I have ever heard. The aim was not to show that "the person controls the
>environment, rather than the other way around". Nor was the aim
>to show that "behavior is not stimulus bound" (whatever that means).
Well, that's how you described it when you _first_ suggested that I take a look at it, back when it was available only on hypercard. It came up in the context of differentiating between behavior: the control of perception and behavior as response to stimulus. [The term "stimulus bound" is contained within my conclusion, not my description of what your demo shows; it's

obviously not a term you would use.]

What is the "system concept" you think I control for? Rather than engaging in innuendo, how about stating it directly?

>The
>aim was what I said it was -- to show that control systems
>control perceptual representations of objective states of affairs.
>It did this by showing that a person (control system) can control
>_different_ representations of the _same_ objective state of affairs.

That's what you are using it for _now_, but that principle is self evident to me, and as I said before, irrelevant to this discussion.

>> But this is not the issue presently at hand...What is being argued
>> is that there are conditions under which an environmental variable
>> can be stabilized even though it is not being controlled, by
>> actually controlling another perception based on an internal
>> model of the CV, which we have been calling CVm.

>I have never disputed this.

Oh, get off it. You went so far as to deny the usefulness of the distinction between controlling a variable and stabilizing it.

Here's one for your PCT scrapbook:

>In Bruce Abbott's ridiculously unrealistic model-based controller,
>CV is _stabilized_ by computed outputs; it is not
>_controlled_.
You've missed the point entirely. The model is _intended_ to be ridiculously unrealistic! Now, how do the "computed outputs" of this controller differ from those of the usual type? That is, in what sense are the outputs of a control system _not_ "computed"? In both cases, o = f(r - p). Does one of these formulas have the label "computed" attached to it, and the other not?

>What I have disputed is the idea that the CV in a _normal_ control loop
>is _stabilized_. Under normal cirsumstances, the CV is controlled, not
>stabilized.

Semantics. I noted earlier that control is simply a method of stabilization. They are not opposites; control is a type of stabilization. The CV in a normal control loop is stabilized against random disturbances that are within the control system's capacity to oppose (amplitude, bandwidth).

>I will also note that if your model based controller were able to
>predict every arbitrary disturbance that were being applied to the CV
>(that is, if your model based controlled lived in the Twilight Zone)
>then it would be impossible for an observer to tell that CV is being
>stabilized rather than controlled.

Glad you agree. This agreement was to be my starting point for a further analysis and comparison between simulation-based stabilization and control; instead, you take it as the end-point. I have taken the physicists "assume a frictionless pendulum" and have then had to spend the next five days arguing about whether one _can_ assume a frictionless pendlum, there being no such thing in the real world and so on. By the time we're done with that, we've forgotten what the original debate was about. By now no one is

even in a frame of mind to hear the rest.

>> No one is claiming that this form of stabilization will work
>> against unpredictable disturbances, or that it is as good as
>> control (it isn't) when the latter is possible.

>Now this is actually a useful statement. The question, then, is "to what
>degree do organisms use model-based control when they lose input". Since
>you [Bruce Abbott (961225.1540 EST)] have declared
>that this cannot, in general, be determined by experiment, I guess you
>are in the cat-bird seat on this issue, eh?;-)

Now wait a minute, I never said that the question "_to what degree_ do organisms use model-based control when they lose input" is incapable of empirical test! My claim is that such a question cannot be answered with one experiment, on one type of CV, with one method of acting on the CV. If I claimed that "some fowl are white," you couldn't prove me wrong by showing me a black duck, could you? That would only prove that _this_ fowl is not white. I'm sorry, Rick, but that's just plain old-fashioned logic; I didn't make it up.

Regards,

Bruce

Date: Thu, 26 Dec 1996 21:01:04 -0500
From: "David M. Goldstein" <davidmg@JERSEY.NET>
Subject: Webchat Broadcasting System

From: David Goldstein\
To: All
Date: 12/26/96

I just discovered this website. It provides an opportunity to chat online. There is no charge. Is anyone familiar with it? It seems to open up some interesting possibilities for real time conversations. I imagine that two or more CSGnet people could agree to meet in a certain room at a certain time and pursue a particular line of conversation. Much of the problem with the present format is that people monologue at each other, for example Rick and Bruce, instead of talking with each other. I expect that a more conversational format would result in more efficient communication and less aggressive talk. I would be interested in hearing from people who have more experience with websites such as the one I mentioned. My inexperience with this kind of format could make me be more optimistic than is justified.

Date: Thu, 26 Dec 1996 19:57:42 +0000
From: Richard marken <marken@AERO.ORG>
Subject: Re: Lecture and demo

[From Rick Marken (961226.1850 PST)]

An imporved version of the "Control of Perception" demo is now available at:

http://www.leonardo.net/Marken/ControlDemo/ControlP.html

Comments will be greatly appreciated.

Bruce Abbott (961226.1955 EST)--

> What is the "system concept" you think I control for?

I would call it "science". Your "science" system concept requires that theories be alternative exlanations of existing facts. So PCT, from your point of view, can only be a new way of explaining the existing facts of behavior. The notion that PCT explains behavioral phenomena that were never before considered facts (and ignores most that are) is a disturbance to this concept.

Me:

> The aim [of the "Control of Perception" demo] was what I said it was > -- to show that control systems control perceptual representations
> of objective states of affairs.

Bruce:

> That's what you are using it for _now_, but that principle is self
> evident to me, and as I said before, irrelevant to this discussion.

Here's the discussion, as I remember it. You presented your model- based controller that controls CV when it controls CVm. You then argued that the CV that corresponds to a controlled perception in a regular control loop is _stabilized_ in the same way as the CV is stabilized in your model-based control system. I said that was wrong; the CV _is_ controlled in a control loop. You said that in saying this I was denying that behavior is the control of perception; that I was saying that it is the environment (CV) and not perception that is controlled. I said "_no_, perception is controlled", but so is the CV. You then asked "well, then why does PCT say that behavior is the control of perception"? I developed and posted the "Control of Perception" demo to answer _that_ question. Behavior is the control of perception and (from the observer's perspective) control of the environmental correlate of the controlled perception (the CV).

Bruce:

> What is being argued is that there are conditions under which
> an environmental variable can be stabilized even though it is
> not being controlled

Me:

>I have never disputed this.

Bruce:

> Oh, get off it. You went so far as to deny the usefulness of the
> distinction between controlling a variable and stabilizing it.

The distinction I was objecting to was calling perception "controlled" and the CV "stabilized" in a _normal_ control loop. I thought (and still

think, based on what you say below) that you thought that the CV is stabilized (not controlled) and that only the perception is controlled in a normal control loop. This is not only a useless distinction; it is an incorrect one. In your model-based system, the perception _and_ the CVm are controlled; the CV is stabilized (coincidentally kept in some reference state by the outputs that control CVm), _not_ controlled.

>how do the "computed outputs" of this [model-based] controller
>differ from those of the usual type?

It's not "how the outputs are computed" that differentiates your model-based from the usual type of controller. It's whether the outputs are computed "in the loop" with the controlled variable (CV). In model-based control, CV is _not in the loop_ with the outputs that are added to it. So the computed value of the outputs have nothing to do with the value of CV. That's why the outputs of a model-based controller don't _control_ CV (it's not even appropriate to call the CV in a model-based controller a "CV"). The outputs that are part of the closed loop in the "usual controller" _do_ control CV -- that's why it's called the CV (controlled variable).

Best

Rick

Date: Fri, 27 Dec 1996 07:01:44 -0500
From: Bruce Gregory <gregory@CF4.HARVARD.EDU>
Subject: Re: Observations come first

[From Bruce Gregory (961227.0700 EST)]

Bruce Abbott (961226.1215 EST)
>
> >Bruce Gregory (961224.1510 EST) --
>
> >>Bill Powers (961224.1145 MST)
>
> >>When you fool around with
> >> thought-experiments too much, you tend to get the priorities reversed.
>
> >I would like to add my fervent agreement. I suggest we adopt the
> >thought as a New Year's resolution!
>
> Meaning what? That one should not examine the implications of one's models?
> What is "fooling around with thought experiments too much"? Tell that to
> Einstein.

I knew Einstein, Bruce. And believe me, you're no Einstein. (Sorry, I just couldn't resist. Especially since I _did_ know Einstein, although admitedly I was only twelve at the time...) The critical element in a thought experiment is that its outcome must be uncontestable. To the extent that it is questioned, it fails in its purpose. Einstein was a master at envisioning thought experiments whose outcomes were not an issue.

> I think that there is quite a bit to be learned from examining the logical

> (and mathematical) implications of one's assumptions, to see where they
> lead. In this case I think it might save quite a bit of empirical work,
> because what can be shown to be logically true (or false) need not be
> demonstrated empirically.

Not so clear. Many of the "pardoxes" of quantum mechanics arise
because they seemed logically impossible, at least to Einstein, who
usedthought experiments in an attempt to show the limits of QM.

> Furthermore, you are not going to be able to
> empirically demonstrate that simulation-based stabilization is _not_
> employed by living organisms for _any_ purpose merely by showing that human
> beings are not able to use it effectively to counteract the influence of a
> sine-wave disturbance over several cycles.

I would have thought that the existence of circadian rhythms would
be reason enough to accept something like simulation-based
stabilization, which we all agree is unrelated to control. Martin
Moore-Ede (if I spelled his name correctly) has gather lots of data
on this phenomenon. Perhaps you might want to model some of it.

Bruce Gregory

Date: Fri, 27 Dec 1996 06:50:54 -0700
From: Bill Powers <powers_w@FRONTIER.NET>
Subject: Re: Lecture and demo, Autonomy

[From Bill Powers (961226.2020 MST)]

Rick Marken (961224.1750 PST)--

>The difference between objects that respond to causes and those that
>control their inputs is illustrated in my "Detection of Purpose" demo.
>The difference is that the cause-effect objects "go with the flow" (your
>disturbance); the controlling objects "do their own thing" (despite your
>disturbance). It seems to me that simple, closed-loop control systems
>exhibits many of the charateristics that we call "autonomy", even though
>such systems are not "indeterminate".

There's something else here that needs to be brought out. There is a class
of systems that create behavior without anything else making them behave. A
simple example is an oscillator. Acquire a couple of capacitors, a couple of
resistors, and a little package with eight legs. Solder up a few
connections. Attach a battery -- and out comes regular spontaneous behavior,
a sine wave of voltage that will go on and on all by itself until you cut
the power off. This is behavior with a material cause but no efficient cause
or final cause. What creates the behavior is the physical construction of
the components and the way they are physically connected, plus an
unpatterned source of power. There are no behavioral events per se; the
behavior simply proceeds endlessly. And because of that there are no
antecedent causes of the behavior. It's simply the nature of certain
organizations of matter to behave spontaneously, turning a steady supply of
energy into organized, patterned actions.

A control system is a special case of this type of spontaneously-acting
system. Wire up a control system and connect it to a source of power, and it

will spontaneously begin trying to make its input match whatever reference input it is given (or an equivalent reference input of zero, if there is no external reference signal and no internal offset). It will keep trying to do this in any environment, although it can succeed only in some environments. Where it does succeed, no external agency needs to tell it what actions to produce or when to start and stop behaving. It is the physical nature of a control system to control.

We're talking, therefore, about a type of mechanism that the mechanists of old would have considered magical. It was precisely this sort of mechanism that was rejected by those who thought that labeling a proposed kind of behavior spontaneous or self-generated was sufficient to prove its impossibility.

Best,

Bill P.

Date: Fri, 27 Dec 1996 06:51:10 -0700
From: Bill Powers <powers_w@FRONTIER.NET>
Subject: Re: Lecture and demo

[From Bill Powers (961226.2100)]

Bruce Abbott (961225.1540 EST) --

>I define "objective" as public and reproducable. That is, if you >and I both conduct our observations under the same conditions, our >observations will agree, within the limits of experimental error. >In this sense the difference in the areas of two given rectangles >are as objective their lengths, widths, areas, or the temperature >of the paper on which they are >drawn.

"The same conditions" means, of course, "conditions perceived by all to be the same." So "objective" observations still require observers who apply the same perceptual input functions to inputs from their environments. In some fields, observers have little difficulty in agreeing that they have established similar-enough conditions, and that they have made similar measurements. Unfortunately, psychology is not one of those fields.

>> I want to be able
>>to walk up to the system at any time, apply a disturbance of my >>choosing to the controlled variable, and see the action of the >>system change immediately to oppose the effect of my disturbance. >>A real control system >can do this.
>
>Yes. Hence the distinction to be made between controlled and >stabilized. A variable may be stablized against some predictable >disturbance waveform without being controlled.

Yes. But such examples are trivial, because they require the presence of a designer who can supply the necessary prediction. To say that CV and CVm will be the same when they are the outcomes of identical processes with identical inputs is not to say very much. The big problem is that of making sure that the processes and inputs are identical. And the slightest change of conditions will invalidate the model.

>This example was intended to provide a starting point for >discussion -- an_optimal situation_ for model-based control. Thus >far I have not been able to proceed beyond trying to get the >participants in this debate to agree to what you just stated -- >that CV and Cm will behave in exactly the >same way, so that it >would be impossible to tell, just by observing this "put up" >system, whether CV is or is not under control. Lacking agreement >on this point (which I thought was an obvious one that would meet >no opposition), I have been unable to proceed further.

Well, I agree with it, except that because of the way you have set up the system, I don't think of it as a control process. This is probably why I am a poor mathematician: I may say that I'll go along with a hypothetical situation for the sake of the argument, but if I don't believe in the proposed conditions, I will only be pretending to pay attention to the rest of the argument. You're forbidding me to apply my own disturbance to the CV, and then claiming it would be impossible to tell if the CV was or was not under control. I have to agree with that, but by making this condition you've simply made the determination impossible. If the moon were made of green cheese, and I had a spaceship and a loaf of bread, I could have a cheese sandwich. Under those conditions I'd look somewhere else for lunch.

>>Also, don't forget that the entire question may be moot: if people >>can't go on "stabilizing" CV in the absence of input, even with >>just a simple sine-wave disturbance, then obviously for them CV >>does not equal CVm, and the whole model is incorrect. I suspect >>that when the results come in from RunBlnd4, we will see that >>people can't even control OR stabilize a controlled variable when >>the reference level is constant and the only disturbance is a >>simple, slow, completely predictable sine wave. If that >>proves to be true, my interest in a thought-experiment that >>assumes what seems to me an impossibility will wane very rapidly. >>It's already pretty low.
>
>It would be my guess that people will tend to do poorly at this >task, even with considerable practice at it, because it demands not >only variation of the correct approximate form and amplitude (which >I suspect people can learn to produce fairly well, although perhaps >not via a mouse), but also (and critically) the correct phase (even >relatively small phase errors can produce high rms errors), and >this must be maintained over several cycles of the disturbance >waveform.

This is what I observed in me, and expect others to observe, although the data are not in yet. I can't even produce a decent sine-wave. If the data do not support the possibility of model-based control in this experiment, why spend any more effort on it? Can you think of another experiment we could try where it might work better?

>However, I've already noted biological systems in >which internally-generated regular cycles -- the so-called >biological clocks-- do produce excellent stabilization against some >forms of predictable disturbance.

I think you're exaggerating the "stability" of these clocks. In fact, most of them tend to run too slowly, and they are so little stabilized that they can easily be bought into synchronism with diurnal driving events of a distinctly shorter period. If they were really stable, it would be hard to change their frequencies.

When you say "excellent stabilization," I think you're making the wrong comparison. Whatever you're thinking of as being stabilized may be discernibly more stable compared with the case of no control at all, but compared to the multitude of variables that are stabilized by negative

feedback control they're hardly stabilized at all. There's a big obvious important phenomenon right in front of us, but we take it so much for granted that we don't even see it as a phenomenon.

>Thus I think it would be a mistake to conclude that poor
>performance under the conditions imposed by RunBlnd4 would rule out
>simulation-based stabilization as an operative feature of >biological systems. If people do poorly, all you will know is that >they are not good at using simulation-based control in this >situation.

You haven't mentioned any evidence that would convince me it's a mistake. A vague reference to biological clocks doesn't do the trick. An experiment where it works would serve a lot better.

Don't forget to look at the first half of the data from RunBlnd4. When you CAN see the controlled variable, you control it very well indeed, compared with how you do without a visible cursor.

>But this is not an "either - or" proposition. A variable can be >stable although not controlled. The question then becomes whether >the observed stability is a result of control or of some other >process.

OK, I'll go along with that. A marble in a bowl is not a control system, although it has a stable state. But one of the points of PCT is that stable processes in organisms are NOT stabilized that way.

>So I'm afraid
>that I'm going to have to disagree with both you _and_ Rick on this one,
>and continue to side with Martin. The distinction _is_ >useful.

>The Test can, of course, distinguish between a system stabilized by
>control or by some other means. This is important, but not >relevant to
>the issue at hand, which concerns the degree to which >an internal model of the predictable portion of the disturbance >acting on a CV can be employed to reduce the variation in the CV, >thus conferring upon it a degree of stability, although not >control.

Bruce, old pal, I am not going to buy that parrot. It's dead. I can see that its feet are nailed to the perch. I am not aware of any means of stabilizing a variable against arbitrary disturbances other than control. I am extremely unconvinced that an arrangement of the sort you propose for "stabilization" can be made to work at all in any practical sense, in an organism. Show me one little spontaneous move on the part of that parrot (and kindly get your thumb off the cage), and I might look at it again.

Best,

Bill P.

End of CSGNET Digest - 26 Dec 1996 to 27 Dec 1996
****

Date: Sat, 28 Dec 1996 08:00:16 +1800
Subject: CSGNET Digest - 27 Dec 1996 to 28 Dec 1996

There are 6 messages totalling 243 lines in this issue.

Topics of the day:

1. Autonomy
2. Webchat Broadcasting System
3. thanks; schools
4. Observations come first
5. Re.: Webchat Broadcasting
6. Searchable Archives for CSGnet

Date: Fri, 27 Dec 1996 10:44:29 -0600
From: Bruce Abbott <abbott@CVAX.IPFW.INDIANA.EDU>
Subject: Re: Autonomy

[From Bruce Abbott (961227.1145 EST)]

>Bruce Gregory (961224.1610 EST) --

>>Tracy Harms 1996;12,24.13:00

>> The strength I see to indeterminism over determinism lies something like
>> this: Rather than having two physical claims about the world (sub-atomic
>> changes are indeterministic + larger changes are deterministic) we have a
>> single claim. More importantly, if changes are rigidly deterministic we
>> can expect that larger-scale changes will be able to be explained in terms
>> of our understanding of the causality of small-scale change; but if change
>> is inherently indeterminate at all scales, causal limits may differ for
>> every subject matter such that distinct explanations must be forged for
>> each. The intractability of explaining all phenomena by reductionist
>> appeal to physics is more suggestive of indeterminism than determinism.

>It may be a good idea to keep in mind that both determinism and
>indeterminism are properties of models. Whenever we push models far
>beyond the realms where they have been tested we are in danger of
>going astray.

I agree with the sentiment, but keep in mind that Tracy's model is,
according to him, untestable.

Regards,

Bruce

Date: Fri, 27 Dec 1996 08:45:07 -0800
From: Richard Marken <marken@AEROSPACE.AERO.ORG>
Subject: Re: Webchat Broadcasting System

[From Rick Marken (961227.0840)]

David Goldstein (12/26/96) --

>I just discovered this website [Webchat Broadcasting System].

I think the URL is http://www.webchat.net/

>Much of the problem with the present format is that people monologue at

>each other, for example Rick and Bruce, instead of talking with each
>other. I expect that a more conversational format would result in more
>efficient communication and less aggressive talk.

Could you explain why you think this would be the case, from a PCT perspective? I keep getting hung up at the point where I realize that everyone involved in the "conversational format" would still be a control system.

Best

Rick

Date: Fri, 27 Dec 1996 12:54:38 -0800
From: Richard Marken <marken@AEROSPACE.AERO.ORG>
Subject: Re: thanks; schools

[From Rick Marken (961227.1300)]

Tracy Harms (1996;12,24.13:30) --

>I'm curious regarding the academic situation for control theory,
>cybernetics, and system science. Are there universities which stand as
>especially good in these fields of study? What trends or concentrations of
>scholarship can be found? Are there places where things are especially
>"happening," or are the hot studies happening with a scatteredness which
>elevates no school or foundation above others?

I know that the PCT approach to understanding living systems is not taught at _any_ university. There may be the occasional graduate seminar on PCT but that's about it.

The only place to study PCT is right here on CSGNet and/or at CSG meetings. They'll be turning out priests at the Yeshiva before they start turning out experts in PCT at Harvard;-).

Best

Rabbi John Paul II

Date: Fri, 27 Dec 1996 15:21:03 -0700
From: Bill Powers <powers_w@FRONTIER.NET>
Subject: Re: Observations come first

[From Bill Powers (961227.0700 MST)]

Bruce Abbott (961226.1215 EST)--

>I've already noted observations in which simulation-based stabilization
>may be at work. Generally speaking, if it _can_ be done to advantage,
>then there will probably be some organism that does it.

Unless there is another way that accomplishes the same end even better, in which case, I am told, evolution will ruthlessly favor the better solution, even if it's only 2% better.

>This makes it worth exploring whether such a tactic can be made to work
>and, if so, what its requirements are.

I'd like to see some evidence that any organism uses this method for anything. That would give me some interest in exploring it further.
>
>>>When you fool around with
>>> thought-experiments too much, you tend to get the priorities reversed.
>
>>I would like to add my fervent agreement. I suggest we adopt the
>>thought as a New Year's resolution!
>
>Meaning what? That one should not examine the implications of one's
>models? What is "fooling around with thought experiments too much"? Tell
>that to Einstein.

Einstein had a solid base of observational evidence to work with, including some anomalous but well-observed phenomena. We have nothing like that with regard to simulation-based control as a model of organisms.

>I think that there is quite a bit to be learned from examining the logical
>(and mathematical) implications of one's assumptions, to see where they
>lead. In this case I think it might save quite a bit of empirical work,
>because what can be shown to be logically true (or false) need not be
>demonstrated empirically.

That's probably the most dangerous assumption you could make. You never know all the premises on which a "logical demonstration" is based, nor can you ever be sure that you haven't played some subtle logical trick on yourself. Once you've established your premises as factual, and once you've demonstrated that your logic predicts observable events correctly, there may be a lot to be learned by following the implications out -- that's what we mean by making a prediction from a model. But there is no prediction that'so infallible (unless it's trivial) that it doesn't require experimental test.

>Furthermore, you are not going to be able to
>empirically demonstrate that simulation-based stabilization is _not_
>employed by living organisms for _any_ purpose merely by showing that
>human beings are not able to use it effectively to counteract the
>influence of a sine-wave disturbance over several cycles.

Quite right. Neither can I empirically demonstrate that it's _not_ produced by magic. However, one experiment at a time, I can determine whether it's used for one purpose, then for another purpose, and then for another. If this idea fails in all its applications, then I would feel justified in looking for a better explanation. You can save a lot of wasted mental effort by testing your ideas before you carry them so far that you become committed to them.

Best,

Bill P.

Date: Fri, 27 Dec 1996 19:25:59 -0500
From: "David M. Goldstein" <davidmg@JERSEY.NET>
Subject: Re.: Webchat Broadcasting

From: David Goldstein
Subject: Rick Marken 961227.0840
Date: 12/27/96

I said:
I expect that a more conversational format would result in more efficient communication and less aggressive talk.

Rick said:
Could you explain why you think this would be the case, from a PCT perspective? I keep getting hung up at the point where I realize that everyone involved in the "conversational format" would still be a control system.

I'll put on my PCT hat and try. Yes, each person remains a control system. But the variables a person is controlling for is likely to change in important ways.

What each person says is likely to be shorter. If too much time goes by before you answer, one is likely to see: Rick, are you still there? The shorter, simpler expressions are likely to help comprehension.

What each person says is likely to be more polite. If I don't like what you said, or the way you said it, I am more likely to let you know about it sooner before too many words have gone by. I could always decide to stop the conversation if I feel that you are being too rude, or too aggressive. Not wanting to end the conversation before a satisfactory point is reached seems more likely in a chat format than an email format.

I could go on but the point is that different formats are likely to result in different variables which a person wants to control. If God wanted us to communicate by email, he would have given us keyboards instead of mouths and ears.

" Could you explain why you think this would be the case, from a PCT perspective? I keep getting hung up at the point where I realize that everyone involved in the "conversational format" would still be a control system."

Date: Sat, 28 Dec 1996 08:25:44 -0500
From: "David M. Goldstein" <davidmg@JERSEY.NET>
Subject: Re: Searchable Archives for CSGnet

Gary Cziko wrote:
>
> [from Gary Cziko 962304.0120 GMT]
>
> I recently received the following notice about a searchable archive for CSGnet.
>
> I checked it out and it is pretty nifty. The catch is that you will also

> get some advertising as you search, but since you don't have to look at it,
> it doesn't seem too much of a price to pay.--Gary
>
> --
> - Reference.COM has begun archiving this list as of:
> Nov. 26, 1996
>
> - Searchable archives for the lists are available at:
>
> http://www.reference.com/cgi-bin/pn/listarch?list=CSGNET@postoffice.cso.uiuc
> .e
> du

I tried this and there was nothing there to search. Did anyone else try
it and have better luck?

End of CSGNET Digest - 27 Dec 1996 to 28 Dec 1996
****

Date: Sun, 29 Dec 1996 08:00:04 +1800
Subject: CSGNET Digest - 28 Dec 1996 to 29 Dec 1996

There are 6 messages totalling 394 lines in this issue.

Topics of the day:

1. Simulation-based stabilization vs. control (2)
2. Searchable Archives for CSGnet
3. archives
4. Re.: Webchat Broadcasting (2)

Date: Sat, 28 Dec 1996 08:35:50 -0600
From: Bruce Abbott <abbott@CVAX.IPFW.INDIANA.EDU>
Subject: Simulation-based stabilization vs. control

[From Bruce Abbott (961228.0940 EST)]

>Bill Powers (961227.0700 MST) --

>Bruce Abbott (961226.1215 EST)

>>I've already noted observations in which simulation-based stabilization
>>may be at work. Generally speaking, if it _can_ be done to advantage,
>>then there will probably be some organism that does it.

>Unless there is another way that accomplishes the same end even better, in
>which case, I am told, evolution will ruthlessly favor the better solution,
>even if it's only 2% better.

Control is not always best. For anti-predator defense, turtles have strong
shells (passive stabilization) rather than the ability to run fast (active
stabilization, i.e., control). Presumably evolution favored this solution
for the turtle, if not for the hare.

When birds fly south for the winter, do they wait for their beaks to freeze

before taking wing? If they did, would they be able to fly to safety before they froze to death? Is this more efficient than responding to an internally generated biological change of reference for location?

>>This makes it worth exploring whether such a tactic can be made to work
>>and, if so, what its requirements are.

>I'd like to see some evidence that any organism uses this method for
>anything. That would give me some interest in exploring it further.

I keep providing possible examples, and you keep dismissing them, a priori. One might almost think that you don't _want_ to see any evidence.

>>I think that there is quite a bit to be learned from examining the logical
>>(and mathematical) implications of one's assumptions, to see where they
>>lead. In this case I think it might save quite a bit of empirical work,
>>because what can be shown to be logically true (or false) need not be
>>demonstrated empirically.

>That's probably the most dangerous assumption you could make. You never know
>all the premises on which a "logical demonstration" is based, nor can you
>ever be sure that you haven't played some subtle logical trick on yourself.
>Once you've established your premises as factual, and once you've
>demonstrated that your logic predicts observable events correctly, there may
>be a lot to be learned by following the implications out -- that's what we
>mean by making a prediction from a model. But there is no prediction that'so
>infallible (unless it's trivial) that it doesn't require experimental test.

Bill, I'm only talking about a simple analysis. Here it is:

Assume a disturbance waveform having both predictable and unpredicable components. The changes in CV due to this disturbance can be resolved into orthogonal variance components, call them Vp (predictable variance) and Ve (error variance). If the variance in CV is Vcv, then

  Vcv = Vp + Ve.

A good control system will nearly cancel Vcv by means of an action that opposes the effect of the entire disturbance (assuming that the system can react strongly enough and quickly enough), leaving Vcv nearly equal to zero.

A good simulation-based stabilization system will nearly cancel Vp, leaving Vcv nearly equal to Ve.

Certain conclusions follow:

1. If Ve is negligible, then the simulation-based system will work about as well as the control system.

2. As Ve increases relative to Vp, the advantage of control over simulation-based stabilization increases proportionately.

3. However, simulation-based stabilization will be better than _no_ stabilization so long as Vp exists and can be modeled.

4. If Vp is negligible, or if the simulation-based system is unable to accurately model the predictable disturbance component, then the simulation-based system will fail or nearly fail to stabilize CV.

[However, the system may be able to do well in the short run, so this evaluation generally will depend on the time-frame over which the system must maintain stability.]

5. To the extent that Vp is not accurately modeled, Vp will not be reduced to nearly zero; the effect is the same as if a larger proportion of the variance in CV were due to Ve.

I'm sure that someone better versed in mathematics than I am could demonstrate these points rigorously, i.e., with proofs, but I think they are obvious enough as presented.

>>Furthermore, you are not going to be able to
>>empirically demonstrate that simulation-based stabilization is _not_
>>employed by living organisms for _any_ purpose merely by showing that
>>human beings are not able to use it effectively to counteract the
>>influence of a sine-wave disturbance over several cycles.

>Quite right. Neither can I empirically demonstrate that it's _not_ produced
>by magic. However, one experiment at a time, I can determine whether it's
>used for one purpose, then for another purpose, and then for another. If
>this idea fails in all its applications, then I would feel justified in
>looking for a better explanation.

I said the same thing in response to Rick [Bruce Abbott (961226.1955 EST)], to wit:

+>Now wait a minute, I never said that the question "_to what degree_ do
+>organisms use model-based control when they lose input" is incapable of
+>empirical test! My claim is that such a question cannot be answered with
+>one experiment, on one type of CV, with one method of acting on the CV.

>You can save a lot of wasted mental effort
>by testing your ideas before you carry them so far that you become committed
>to them.

My interest was in evaluating model-based control from a theoretical perspective, to determine its feasibility and limitations. I've done that, and suggested a few cases in which such a system _might_ play a role in biological systems. That's as far as I care to take it.

Regards,

Bruce

Date: Sat, 28 Dec 1996 11:41:18 -0500
From: "John E. Anderson" <jander@OSPREY.UNF.EDU>
Subject: Re: Searchable Archives for CSGnet

[From John E. Anderson (961228.1145 EST)]

David M. Goldstein wrote:
>
> Gary Cziko wrote:
> >
> > [from Gary Cziko 962304.0120 GMT]
> >

> > - Searchable archives for the lists are available at:
> >
> > http://www.reference.com/cgi-bin/pn/listarch?list=CSGNET@postoffice.cso.uiuc
> > .e
> > du
>
> I tried this and there was nothing there to search. Did anyone else try
> it and have better luck?

The problem is that the .edu domain name was left off the end of the URL
in Gary's message. Try the following one instead. It works.

http://www.reference.com/cgi-bin/pn/listarch?list=CSGNET@postoffice.cso.uiuc.edu

John
--
John E. Anderson
jander@unf.edu

Date: Sat, 28 Dec 1996 11:58:58 -0700
From: Bill Powers <powers_w@FRONTIER.NET>
Subject: Re: Simulation-based stabilization vs. control

[From Bill Powers (961228.1025 MST)]

Bruce Abbott (961228.0940 EST)]

>>Bill Powers (961227.0700 MST) --

>Control is not always best. For anti-predator defense, turtles have
>strong shells (passive stabilization) ...

Stabilization of _what_? You're using this word in a very strange way.

>When birds fly south for the winter, do they wait for their beaks to
>freeze before taking wing? If they did, would they be able to fly to
>safety before they froze to death? Is this more efficient than responding
>to an internally generated biological change of reference for location?

What does that have to do with simulation-based control? And why on earth
would birds wait for their beaks to freeze before flying to warmer climates?
Aren't there any other perceptual variables they might be controlling for?

>>I'd like to see some evidence that any organism uses this method for
>>anything. That would give me some interest in exploring it further.
>
>I keep providing possible examples, and you keep dismissing them, a >>priori.

But you're not giving any examples of simulation-based control, which is
what I thought the subject was. And none of the examples you give is
testable. Untestable examples are of no use.

>One might almost think that you don't _want_ to see any evidence.

On the contrary, that's what I keep asking you for, and you keep coming back
with non-examples. Where in the turtle's shell is a simulation of the way
the turtle's actions affect the environment? Where in the birds' migration

is a simulation of the environment? You keep SAYING that these phenomena are examples of simulation-based control, but you haven't said what is a simulation of what and how any such simulation fits into a model of the behaving system. In the case of the turtle's shell, how does the shell amount to a simulation that makes an imagined perception depend directly on the system's output in a way that mimics the perceived response of the environment to the same output? That, in case you've forgotten, is what "simulation-based control system" means. If you think your examples are relevant to simulation-based control you must be using some entirely different meaning for the term.

>Bill, I'm only talking about a simple analysis. Here it is:
>
>Assume a disturbance waveform having both predictable and unpredicable >components. The changes in CV due to this disturbance can be resolved >into orthogonal variance components, call them Vp (predictable variance) >and Ve (error variance). If the variance in CV is Vcv, then
>
> Vcv = Vp + Ve.
>
>A good control system will nearly cancel Vcv by means of an action that >opposes the effect of the entire disturbance (assuming that the system can >react strongly enough and quickly enough), leaving Vcv nearly equal to >zero.

>A good simulation-based stabilization system will nearly cancel Vp, >leaving Vcv nearly equal to Ve.
>
>Certain conclusions follow:
>
>1. If Ve is negligible, then the simulation-based system will work about > as well as the control system.

>2. As Ve increases relative to Vp, the advantage of control over
> simulation-based stabilization increases proportionately.
>
>3. However, simulation-based stabilization will be better than _no_
> stabilization so long as Vp exists and can be modeled.

>
>4. If Vp is negligible, or if the simulation-based system is unable to
> accurately model the predictable disturbance component, then the
> simulation-based system will fail or nearly fail to stabilize CV.
>
> [However, the system may be able to do well in the short run, so this
> evaluation generally will depend on the time-frame over which the
> system must maintain stability.]
>
>5. To the extent that Vp is not accurately modeled, Vp will not be
> reduced to nearly zero; the effect is the same as if a larger
> proportion of the variance in CV were due to Ve.
>
> I'm sure that someone better versed in mathematics than I am could
> demonstrate these points rigorously, i.e., with proofs, but I think >
they are obvious enough as presented.

OK, there's your logic all laid out and acknowledged. It's all quite air-tight -- except for the parts you have to assume are true in order to make the conclusions come out the way you want them to.

Is it really true that a little control is better than no control at all? What if a bird can control its position relative to a flying bug pretty well -- say, within ten inches. Is that good enough to get by on? Or could it be that a swallow needs _real control_ to avoid starvation?

Is it really true that an organism can predict a "predictable" component of a disturbance? If so, how would it go about doing that? Some kind of prediction mechanism is needed -- how would it work, especially with disturbances that can't be directly sensed? Are you seriously proposing a model, or are you just fooling around?

You brought up an example in which the only disturbance was a completely predictable sine-wave, yet when I tried that out experimentally, I found that my own predictions of the sine wave were useless. You complain that that doesn't prove there is no prediction for other kinds of disturbances, but if you want to assume there are disturbances that human beings can actually predict, it's up to you to come up with another example that we can test. Before I did the test, you ASSUMED that a sine wave was so predictable that OF COURSE people could predict it. After a test raised doubts about this example, you complained that it wasn't a fair test of the general idea. You can't keep moving the goal-posts, Bruce. You could wiggle out of any experimental disproof that way. If you'll propose testable examples, I'll keep testing them. But we have to have some sort of agreement that when experiment disproves an assertion, we will count the assertion as false, at least in the case tested.

Are you willing yet to agree that human beings can't predict totally predictable sine-wave disturbances in any way that is useful for stabilizing a cursor against disturbances? Or do you want more data from others? How did your own trials of RunBlnd4 come out? If you're going to take counterexamples seriously, we can talk about this. Otherwise, what's the point of doing any tests at all?

Best,

Bill P.

Date: Sat, 28 Dec 1996 17:30:37 -0500
From: "David M. Goldstein" <davidmg@JERSEY.NET>
Subject: archives

From: David Goldstein
Date: 12/28/96
Subject: Anderson 12/28/96

John Anderson said: The problem is that the .edu domain name was left off the end of the URL in Gary's message. Try the following one instead. It works.

http://www.reference.com/cgi-bin/pn/listarch?list=CSGNET@postoffice.cso.uiuc.edu

Yes it does. Thanks very much. Thought others might want to know.

Date: Sat, 28 Dec 1996 16:41:39 +0000
From: Richard marken <marken@AERO.ORG>

Subject: Re: Re.: Webchat Broadcasting

[From Rick Marken (961228.1530)]

David Goldstein (12/27/96) on why "Webchat" might be better than CSGNet:

> I'll put on my PCT hat and try. Yes, each person remains a control
> system. But the variables a person is controlling for is likely to
> change in important ways.

You mean that if we go to the Webchat format I might stop
controlling for the variables that are disturbed by so much
of what Bruce Abbott says;-)

> What each person says is likely to be shorter...The shorter,
> simpler expressions are likely to help comprehension.

So Bruce will comprehend what I am saying when I say short things
like "Controlled variables are controlled" and I'll comprehend what
Bruce is saying when he says short things like "No. They are
stabilized"? What if we comprehend but don't agree?

> What each person says is likely to be more polite. If I don't
> like what you said, or the way you said it, I am more likely to
> let you know about it sooner before too many words have gone by.

You mean people are impolite on CSGNet because they are not told
soon enough that people don't like what they said?

> I could always decide to stop the conversation if I feel that you
> are being too rude, or too aggressive.

And people can't do that on CSGNet?

> I could go on but the point is that different formats are likely
> to result in different variables which a person wants to control.

Well, I think it would be a very interesting experiment. My guess
is that the tone of the Webchat conversations would be exactly
the same as the tone of the CSGNet conversations.

> If God wanted us to communicate by email, he would have given us
> keyboards instead of mouths and ears.

If God wanted us to do _anything_, we're doing it;-)

Best

Rick

Date: Sat, 28 Dec 1996 20:24:29 -0500
From: "David M. Goldstein" <davidmg@JERSEY.NET>
Subject: Re.: Webchat Broadcasting

From: David Goldstein
Subject: Marken 12/28/96
Date: 12/28/96

Rick said: Well, I think it would be a very interesting experiment. My guess is that the tone of the Webchat conversations would be exactly the same as the tone of the CSGNet conversations.

So who else is going to accept do this " very interesting experiment "?

The steps needed: One person(for example, Rick) has to say--Do you want to step outside and say that? Meet me at the corner of WBS on X date at X time. The name of the private room is PCT (or whatever). People have to remember about the time differences across the USA. The other person (for example, Bruce) has to agree to the date/time or propose a different one. Each person can bring their posse.

End of CSGNET Digest - 28 Dec 1996 to 29 Dec 1996
****

Date: Mon, 30 Dec 1996 08:00:21 +1800
Subject: CSGNET Digest - 29 Dec 1996 to 30 Dec 1996

There are 5 messages totalling 1654 lines in this issue.

Topics of the day:

1. Simulation-based stabilization vs. control (3)
2. Rat in Skinner Box Experiments
3. TrkBlnd1.pas

Date: Sun, 29 Dec 1996 10:21:04 -0600
From: Bruce Abbott <abbott@CVAX.IPFW.INDIANA.EDU>
Subject: Re: Simulation-based stabilization vs. control

[From Bruce Abbott (961229.1120 EST)]

>Bill Powers (961228.1025 MST) --

>>Bruce Abbott (961228.0940 EST)

>>Control is not always best. For anti-predator defense, turtles have
>>strong shells (passive stabilization) ...

>Stabilization of _what_? You're using this word in a very strange way.

Force exerted against the soft tissues, such as is produced when the predator grabs you between its jaws and squeezes down. The turtle's hard shell tends to prevent those forces from reaching a level internally that can do serious damage to the turtle. If the effects of a disturbance to a variable are minimized, does that not confer a degree of stability on that variable? I would hardly call this a strange use of the term.

>>When birds fly south for the winter, do they wait for their beaks to
>>freeze before taking wing? If they did, would they be able to fly to
>>safety before they froze to death? Is this more efficient than responding
>>to an internally generated biological change of reference for location?

>What does that have to do with simulation-based control?

A reference for location is keyed to an internally generated annual rhythm. The action taken (flying south for the Winter) opposes a predictable disturbance to the bird's body temperature and food supply.

>And why on earth
>would birds wait for their beaks to freeze before flying to warmer climates?

Why _wouldn't_ they? Until they get cold, or have trouble finding food, there is no disturbance.

>Aren't there any other perceptual variables they might be controlling for?

Such as? I would hold that perceptual variables such as length of daylight would serve only to phase-lock the annual rhythm-generator.

>But you're not giving any examples of simulation-based control, which is
>what I thought the subject was. And none of the examples you give is
>testable. Untestable examples are of no use.

That's an unsupported assertion. Explain why you think they are untestable.

>>One might almost think that you don't _want_ to see any evidence.

>On the contrary, that's what I keep asking you for, and you keep coming back
>with non-examples. Where in the turtle's shell is a simulation of the way
>the turtle's actions affect the environment? Where in the birds' migration
>is a simulation of the environment? You keep SAYING that these phenomena are
>examples of simulation-based control, but you haven't said what is a
>simulation of what and how any such simulation fits into a model of the
>behaving system. In the case of the turtle's shell, how does the shell
>amount to a simulation that makes an imagined perception depend directly on
>the system's output in a way that mimics the perceived response of the
>environment to the same output? That, in case you've forgotten, is what
>"simulation-based control system" means. If you think your examples are
>relevant to simulation-based control you must be using some entirely
>different meaning for the term.

The turtle's shell was not provided an an example of simulation-based stabilization, but to support my claim that control is not the only way to stabilize a variable against disturbances.

As for a test of simulation-based stabilization (NOT control), I did work on a program I thought might do the job. It involved moving a "gun" that could fire a "projectile" at an "aircraft" flying overhead. My idea was to use various forms of regular or irregular motion of the aircraft and practice hitting the aircraft as it moved across the screen. (Owing to the relative speeds of aircraft and projectile, this requires "leading" the aircraft a bit, as in real gunnery.) The next step was to have the aircraft disappear into the clouds for various durations; the question was, would I still be able to hit it, by estimating where the aircraft "should be" according to previous experience.

Unfortunately, I found out that the aircraft was nearly impossible for me to hit even when in plain view! The mouse was just too jerky for precise aiming. So if we're going to give this sort of test a whirl, we're going to have to use something a bit more stable than a mouse -- a joystick, perhaps. I strongly suspect that this is a serious problem for the current RunBlind

experiment as well, providing an extremely challenging situation even for closed-loop control. The mouse introduces a fairly significant random disturbance component all its own.

>> [my comparison of simulation-based stabilization and control quoted]

>> I'm sure that someone better versed in mathematics than I am could
>> demonstrate these points rigorously, i.e., with proofs, but I think
>> they are obvious enough as presented.

>OK, there's your logic all laid out and acknowledged. It's all quite
>air-tight -- except for the parts you have to assume are true in order to
>make the conclusions come out the way you want them to.

What does "acknowledged" mean -- that you read it or that you agree with it?

As for the second statement, for clarity we need to separate two ways of defining the "truth" of the conclusions. In the first way, the conclusions are "true" if they follow from the assumptions. This is a statement about the validity of the reasoning. I believe that my reasoning is correct, so that, in this sense of "truth," my conclusions are true.

The second way of defining "truth" is that the conclusions apply to reality. This is a statement about the validity of the assumptions. You suggest that I adopted certain assumptions in order to make the conclusions come out the way I want them to. In fact, I have only laid out the _conditions_ under which simulation-based stabilization will be of some benefit. Whether those conditions hold in any given situation is an empirical matter, but at least as a result of this analysis we now know what those conditions are.

>You brought up an example in which the only disturbance was a completely
>predictable sine-wave, yet when I tried that out experimentally, I found
>that my own predictions of the sine wave were useless. You complain that
>that doesn't prove there is no prediction for other kinds of disturbances,
>but if you want to assume there are disturbances that human beings can
>actually predict, it's up to you to come up with another example that we can
>test.

I used the sine-wave disturbance to illustrate the properties of a system that used simulation-based stabilization of CV when the disturbance was only partly modeled. That demonstration showed that simulation-based stabilization would be of benefit (reducing the effect of the disturbance on CV) during the "blind" phase _if_ the EFF and predictable component of the disturbance were accurately modeled. I noted that in this case the requirements for modeling would be stringent in that the model would have to preserve not only the correct shape and amplitude of the disturbance, but its phase as well. I never claimed that people would be good at this.

>After a test raised doubts about
>this example, you complained that it wasn't a fair test of the general idea.
>You can't keep moving the goal-posts, Bruce.

See above. In fact, in an earlier post, I explicitly addressed this problem, stating that if simulation-based stabilization can be found at work in biological organisms at all, it probably will be at higher levels at which disturbances are likely to be much more regular (and slower) than at the motor level. It's an empirical question, but I'm not surprised to find that people have difficulty tracking sine waves "blind" over several cycles with a mouse.

I should like to emphasize, once again, that I make no claim that simulation-based stabilization applies to any variable in any organism; I am not _advocating_ this model. Rather, as a scientist I am interested in the possibilities. My only intention when I stepped into this quicksand was to explore the conditions under which simulation-based, if it existed in biological systems, would actually work. That I have done.

I do have an idea for a somewhat different version of my "gunner" program that might work sufficiently well with a mouse; when I find some time I'll give it a try. Rather than trying to hit an aircraft, I'm going to switch to aerial bombing. That way all I'll have to do is move the "sights" to the position I expect a moving "tank" to be and drop the bomb.

>Are you willing yet to agree that human beings can't predict totally
>predictable sine-wave disturbances in any way that is useful for stabilizing
>a cursor against disturbances?

Try a much lower frequency sine wave, so that vision is lost through only a portion of the cycle. If human beings can't do well then, I'll agree to your conclusion. And Bill, either conclusion is acceptable to me. My interest is in the _testing_, not in any particular outcome. I have no theoretical axe to grind.

Regards,

Bruce

Date: Sun, 29 Dec 1996 11:18:15 +0000
From: Richard Marken <marken@AERO.ORG>
Subject: Re: Simulation-based stabilization vs. control

[From Rick Marken (961229.1000 PST)]

Bruce Abbott (961229.1120 EST) --

>I have no theoretical axe to grind.

I love it!

As we enter the New Year, I would like to take this opportunity to thank Bruce for his consistently entertaining posts. You've made 1996 a wonderful year on CSGNet for me. If you ever become a full-tilt PCTer, Bruce, then conventional psychology beware;-)

Best

Rick

PS. To Bill Powers. Could you please re-send the Runblnd program. I seem to have saved your comments but not the attached Pascal source.

Date: Sun, 29 Dec 1996 14:27:54 -0500
From: "David M. Goldstein" <davidmg@JERSEY.NET>

Subject: Rat in Skinner Box Experiments

From: David Goldstein
Subject: Rat in Skinner Box Experiments
Date: 12/29/96
To: all

I was trying to summarize the findings which appeared on CSGnet to my daughter who is a Psychology major. My recall was that looking at these experiments from a PCT perspective has resulted in some surprising findings but I wasn't able to remember them in enough detail to be useful.

Can someone provide a brief summary of these studies so far? Thanks.

Date: Sun, 29 Dec 1996 18:53:53 -0700
From: Bill Powers <powers_w@FRONTIER.NET>
Subject: Re: Simulation-based stabilization vs. control

--=====================_851936033==_
Content-Type: text/plain; charset="us-ascii"

[From Bill Powers (961229.1445 MST)]

Bruce Abbott (961229.1120 EST) --
>
>>Stabilization of _what_? You're using this word in a very strange way.
>
>Force exerted against the soft tissues, such as is produced when the
>predator grabs you between its jaws and squeezes down. The turtle's hard
>shell tends to prevent those forces from reaching a level internally that
>can do serious damage to the turtle.

OK, so there's a control system that grows a shell when predators grab and squeeze, which "stabilizes" tissue damage near zero. Is this a simulation-based control system? If so, what is simulating what? And exactly where does this system reside? It's not likely to work in any individual turtle, is it?

>If the effects of a disturbance to a
>variable are minimized, does that not confer a degree of stability on that
>variable? I would hardly call this a strange use of the term.

Not every means of stabilizing something is interesting from a control-system point of view. For example, when a lake freezes to the bottom, it tends to stabilize the positions of any fish swimming in it. It's the _manner_ of stabilization that matters.

>>>When birds fly south for the winter, do they wait for their beaks to
>>>freeze before taking wing? If they did, would they be able to fly to
>>>safety before they froze to death? Is this more efficient than responding
>>>to an internally generated biological change of reference for location?
>
>>What does that have to do with simulation-based control?
>
>A reference for location is keyed to an internally generated annual
>rhythm. The action taken (flying south for the Winter) opposes a

>predictable disturbance to the bird's body temperature and food supply.

I see. An internal oscillator with a period of one year. Ingenious.

>>And why on earth
>>would birds wait for their beaks to freeze before flying to warmer climates?
>
>Why _wouldn't_ they? Until they get cold, or have trouble finding food,
>there is no disturbance.

>>Aren't there any other perceptual variables they might be controlling for?

>Such as?

Why not suppose that there are perceptions and reference signals concerning the length of day, the mean temperature, the minimum or maximum or mean altitude of the sun at noon, the colors of leaves and plants, the availability of summer food supplies, the constellations that appear at night, and other equally likely possibilities? In Colorado (and I suppose elsewhere too) we are admonished to remove humming-bird feeders in the fall, because if "nectar" continues to be available, the birds will fail to migrate in time to survive. There are lots of perceptual variables that birds might control by migrating to different latitudes, with survival being a side-effect of doing so. An annual clock seems like one of the least plausible explanations.

> I would hold that perceptual variables such as length of daylight
>would serve only to phase-lock the annual rhythm-generator.

But why do you need this rhythm generator if you can get the same effect by controlling the variables that change annually? The internal timer is superfluous, unless you can somehow directly demonstrate its existence.

>>But you're not giving any examples of simulation-based control, which is
>>what I thought the subject was. And none of the examples you give is
>>testable. Untestable examples are of no use.
>
>That's an unsupported assertion. Explain why you think they are >untestable.

Perhaps you are even more ingenious than I thought. How would you test to see if a turtle (species, I assume) grows shells as a way of stabilizing tissue damage due to predators? An alternative hypothesis would be that turtles which have grown shells have survived in environments where predators are a problem, while few without shells have survived. Testing either hypothesis would be, to say the least, difficult and time-consuming (more time than you and I --combined -- have left).

As to the bird migrations, I can forsee technical difficulties in measuring the behavior of an oscillator having a one-year period, particularly since measuring its period would require cutting it off from the environment while still allowing the migratory behavior to occur. In fact, all the experiments I have heard of concerning bird migration involve cutting off or distorting sensory input from the environment, and doing so seems to have a critical effect on time and direction of flight.

>>>One might almost think that you don't _want_ to see any evidence.

>>On the contrary, that's what I keep asking you for, and you keep coming
>>back with non-examples. Where in the turtle's shell is a simulation of

>>the way the turtle's actions affect the environment?

>The turtle's shell was not provided an an example of simulation-based
>stabilization, but to support my claim that control is not the only way to
>stabilize a variable against disturbances.

Oh. I must have missed that disclaimer. But why do you think you need to prove that control is not the only way to stabilize a variable against disturbances? Doesn't everybody know that? Those nails through the parrot's feet stabilize it against disturbances that might make it fall off the perch. But that doesn't have much to do with the PARROT'S acting in such a way as to stabilize some external variable.

>As for a test of simulation-based stabilization (NOT control), I did work
>on a program I thought might do the job. It involved moving a "gun" that
>could fire a "projectile" at an "aircraft" flying overhead. ...
>Unfortunately, I found out that the aircraft was nearly impossible for me
>to hit even when in plain view! The mouse was just too jerky for precise
>aiming.

Just reduce the mouse sensitivity, if "jerkiness" is really the problem. I suspect, however, that the problem is simply that you've set up a task that people aren't very good at. This control problem involves a time delay (a transport lag) which means that control movements have to be slow if the loop gain is to be high. If the angle of aim has to accelerate and decelerate to compensate for a changing distance to the target, that makes the task doubly difficult.

The best way to hit the target is to ignore the "gun" and the "projectiles" on their way to the target, and focus on the stream of projectiles where they are crossing ahead of or behind the target. You have to move that stream until it intersects the position of the target. Because of the delay you have to make your movements slow -- reducing mouse sensitivity will help you do this.

Remember the Gulf War telecasts, with airplanes flying serenely through the hail of anti-aircraft fire? Gunners in real combat situations (or good simulators) almost always miss the target.

>So if we're going to give this sort of test a whirl, we're going to
>have to use something a bit more stable than a mouse -- a joystick, perhaps.

I very much doubt that the mouse is the problem; if it is, it's easy to fix. We use mouses all the time in tracking experiments, and they pretty faithfully reflect what the participant's hand is doing. Human movements (especially mine) are pretty jerky in themselves, and when you're working blind, they don't repeat very accurately. This doesn't matter for closed-loop control because control is based on monitoring the _outcome_, not the movements. If your movement is a little off, it's corrected just as if an external disturbance had occurred.

>I strongly suspect that this is a serious problem for the current RunBlind
>experiment as well, providing an extremely challenging situation even for
>closed-loop control. The mouse introduces a fairly significant random
>disturbance component all its own.

Quit blaming the mouse. It's YOU. Your hand is wobbling. The mouse is just picking up those wobbles and showing them to you. In RunBlnd4, during the sighted portion, my tracking errors are about 10% RMS of the peak-to-peak

excursion of the disturbance. They are perfectly real tracking errors, introduced largely by my aged nervous system. At the frequency of movement used, my tracking errors contain an obvioyus systematic lag of about 90 degrees. The wobbles are not caused by the mouse: my mouse resolves about 600 counts for a six-inch movement, or 100 counts per inch. I estimage that my wobbles cover a range of about 60 pixels plus and minus, of which probably 59 are real. The little slippages of the mouse show up only as a long-term drift, a pixel or two at a time, at random intervals.

Anyway, the important thing about RunBlnd4 is the comparison between the tracking errors with and without sight. Without sight they are 20 times as large, AT LEAST.

>>> [my comparison of simulation-based stabilization and control quoted]
>
>>> I'm sure that someone better versed in mathematics than I am could
>>> demonstrate these points rigorously, i.e., with proofs, but I think
>>> they are obvious enough as presented.
>
>>OK, there's your logic all laid out and acknowledged. It's all quite
>>air-tight -- except for the parts you have to assume are true in order to
>>make the conclusions come out the way you want them to.
>
>What does "acknowledged" mean -- that you read it or that you agree with it?

I agree that given your premises, your conclusions follow logically. I claim that several of your premises are false to fact, so the conclusions are irrelevant in the real world.
>
>As for the second statement, for clarity we need to separate two ways of
>defining the "truth" of the conclusions. In the first way, the
>conclusions are "true" if they follow from the assumptions. This is a
>statement about the validity of the reasoning. I believe that my
>reasoning is correct, so that, in this sense of "truth," my conclusions
>are true.

Yes, I agree.

>The second way of defining "truth" is that the conclusions apply to
>reality. This is a statement about the validity of the assumptions. You
>suggest that I adopted certain assumptions in order to make the
>conclusions come out the way I want them to. In fact, I have only laid
>out the _conditions_ under which simulation-based stabilization will be of
>some benefit. Whether those conditions hold in any given situation is an
>empirical matter, but at least as a result of this analysis we now know
>what those conditions are.

Fine. I won't argue. Let's get on with the empirical testing.

>>You brought up an example in which the only disturbance was a completely
>>predictable sine-wave, yet when I tried that out experimentally, I found
>>that my own predictions of the sine wave were useless. You complain that
>>that doesn't prove there is no prediction for other kinds of
>>disturbances, but if you want to assume there are disturbances that human
>>beings can actually predict, it's up to you to come up with another
>>example that we can test.
>
>I used the sine-wave disturbance to illustrate the properties of a system
>that used simulation-based stabilization of CV when the disturbance was

>only partly modeled. That demonstration showed that simulation-based
>stabilization would be of benefit (reducing the effect of the disturbance
>on CV) during the "blind" phase _if_ the EFF and predictable component of
>the disturbance were accurately modeled. I noted that in this case the
>requirements for modeling would be stringent in that the model would have
>to preserve not only the correct shape and amplitude of the disturbance,
>but its phase as well. I never claimed that people would be good at this.

Well, then, what sort of simulation-based control _would_ they be good at?

>In fact, in an earlier post, I explicitly addressed this
>problem, stating that if simulation-based stabilization can be found at
>work in biological organisms at all, it probably will be at higher levels
>at which disturbances are likely to be much more regular (and slower) than
>at the motor level. It's an empirical question, but I'm not surprised to
>find that people have difficulty tracking sine waves "blind" over several
>cycles with a mouse.

I couldn't even track a quarter of a cycle accurately. How did you do?

>I should like to emphasize, once again, that I make no claim that
>simulation-based stabilization applies to any variable in any organism; I
>am not _advocating_ this model. Rather, as a scientist I am interested in
>the possibilities. My only intention when I stepped into this quicksand
>was to explore the conditions under which simulation-based, if it existed
>in biological systems, would actually work. That I have done.

Roger. Now can we forget all this belaboring of the obvious, and do some
actual testing that might tell us when people do and do not use
simulation-based control?

>I do have an idea for a somewhat different version of my "gunner" program
>that might work sufficiently well with a mouse; when I find some time I'll
>give it a try. Rather than trying to hit an aircraft, I'm going to switch
>to aerial bombing. That way all I'll have to do is move the "sights" to
>the position I expect a moving "tank" to be and drop the bomb.

You'll have the same problems. When you miss, you'll have to make a
correction. The correction has to be made slowly and in small increments
because of the delay. The best way to do this is to ignore the "sight" and
simply control the position where the successive bombs are dropping,
relative to the "tank." You'll probably do better without any sight to
distract you (unless you cheat and build a predictive capability into the
sight, so it shows where the bombs will actually hit).

>>Are you willing yet to agree that human beings can't predict totally
>>predictable sine-wave disturbances in any way that is useful for
>>stabilizing a cursor against disturbances?

>Try a much lower frequency sine wave, so that vision is lost through only
>a portion of the cycle.

I'll attach the code and you can try it out easily. I look forward to your
results.

>If human beings can't do well then, I'll agree to
>your conclusion.

OK.

Best,

Bill P.

--=====================_851936033==_
Content-Type: text/plain; charset="us-ascii"

```
program runblnd4;
{ Simulation to demonstrate a controller capable of continuing to control
  (to some degree) after its input has been lost.

  Compile in Turbo Pascal 7.0 or Borland Pascal 7.0 or above
  Written by Bruce Abbott
  Indiana - Purdue Fort Wayne
  (961218)

  Revised version. Graph mode fixed; speed of display adjustment
  added. WTP 961221

  Test of human controller with loss of input and simple sine-wave
  disturbance. WTP 961222

}

uses dos, crt, graph, mouse, grutils;

var
  ch: char;
  Stop, CanSee: boolean;
  MaxX, MaxY, MaxColor: integer;
  mousecal: real;
  delaystring: string;
  delayval,code: word;
  handle,dist: array[1..1800] of integer;

Procedure Runexpt;
var c,d: real;
    i,j,oldx,curscolor: integer;
begin
 oldx := maxx div 2;
 setcolor(white);
 clearviewport;
 outtextxy(maxx div 2 - 140, maxy div 2 + 70,'CENTER MOUSE ON PAD, SPACE TO
START');
 outtextxy(0, maxy div 2,
'    NOTE: CURSOR WILL DISAPPEAR HALFWAY THROUGH RUN. TRY TO ESTIMATE THE');
 outtextxy(0, maxy div 2 + 15,
'   SIDEWAYS MOUSE MOVEMENTS REQUIRED TO KEEP THE INVISIBLE MOUSE UNDER THE');
 outtextxy(0, maxy div 2 + 30,
'               TARGET UNTIL THE RUN AUTOMATICALLY ENDS');

 ch := readkey;
 clearviewport;
 setcolor(Lightred);
 line(maxx div 2, maxy div 2,maxx div 2, maxy div 2 - 20);
 curscolor := white;
 initmouse;
 for j := -120 to 1800 do
```

```
 begin
  i := abs(j);
  if j > 899 then curscolor := black;
  readmouse;
  if j >= 0 then
   d := 0.175*maxx*sin(2*pi*i/225.0)
  else d := 0.0;
  c := mousex*mousecal;
  handle[i] := round(c);
  dist[i] := round(d);
  delay(delayval);
  setcolor(black);
  line(oldx,maxy div 2,oldx,maxy div 2 + 20);
  oldx := handle[i] + dist[i] + maxx div 2;
  setcolor(curscolor);
  line(oldx,maxy div 2,oldx,maxy div 2 + 20);
 end;
end;

procedure showresults;
var i: integer;
begin
 clearviewport;
 setcolor(lightred);
 outtextxy(0,0,'HANDLE POSITION');
 setcolor(lightgreen);
 outtextxy(0,15,'DISTURBANCE');
 setcolor(white);
 outtextxy(0,30,'ERROR');
 for i := 1 to 1800 do
 begin
  putpixel(i div 3, handle[i] + maxy div 2,lightred);
  putpixel(i div 3, dist[i]  + maxy div 2,lightgreen);
  putpixel(i div 3, handle[i] + dist[i]  + maxy div 2,white);
  putpixel(i div 3, maxy div 2,lightgray);
 end;
 setcolor(lightgray);
 line (300,maxy,300,0);
 outtextxy(100,maxy - 50,'CURSOR VISIBLE');
 outtextxy(400,maxy - 50,'CURSOR INVISIBLE');
 ch := readkey;
end;

procedure calibratemousex;
var mousemax, mousemin: real;
begin
 clearviewport;
 outtextxy(maxx div 3, maxy div 2,'Set mouse to left edge of pad,');
 outtextxy(maxx div 3, maxy div 2 + 10,'Press space when done');
 ch := readkey;
 readmouse;
 mousemin := mousex;
 outtextxy(maxx div 3, maxy div 2 + 40,'SLIDE mouse to right edge of pad,');
 outtextxy(maxx div 3, maxy div 2 + 50,'Press space when done');
 ch := readkey;
 readmouse;
 mousemax := mousex;
 if mousemax = mousemin then mousecal := maxx/512.0
 else
```

```
 mousecal :=  maxx/(mousemax - mousemin);
end;

begin
  if not initmouse then halt;
  delaystring := paramstr(1);
  val(delaystring,delayval,code);
  ClrScr;
  InitGraphics;
  MaxX := GetMaxX; MaxY := GetMaxY;
  MaxColor := GetMaxColor;
  Stop := false;
  calibratemousex;
  clearviewport;
  Runexpt;
  Showresults;
  restorecrtmode;
  closegraph;
end.
```

--==========_851936033==_--

Date: Sun, 29 Dec 1996 20:51:48 -0700
From: Bill Powers <powers_w@FRONTIER.NET>
Subject: TrkBlnd1.pas

--==========_851943108==_
Content-Type: text/plain; charset="us-ascii"

[From Bill Powers (961229.1945 MST)]

Bruce Abbott (961229.1710 EST) --

I received and ran Trkblnd4. The mouse problem is as I thought: too sensitive. Go through it and replace "MouseX" by "MouseX div 6" (two places). Also, be sure your hand is not touching the mouse pad and that your arm is not touching the table: swing from the shoulder (or, I suppose, from the elbow -- I used the shoulder pivot). Don't press down on the mouse. You will get nice smooth traces. Most of the mouse problems I saw resulted from a combination of slip-stick friction of fingers against pad, and the tiny movements of the hand being magnified.

After making this change, I first ran the sighted version four times:

```
RMS   13.7  15.4  11.7  10.2
TOT   97.5  95.1  98.7  100.0
```

Having got 100 percent in Percent Time On Target and a 10 pixel RMS tracking error, I then did 20 runs in the blind condition (the following runs begin with the very first time I experienced the blind condition):

```
RMS   30.2  24.4  14.8  17.2  22.9  24.6  15.5  19.6  16.3  20.7
TOT   82.6  73.8  95.4  92.7  75.2  81.1  90.1  89.8  88.2  85.8

RMS   20.1  14.4  26.4  17.0  17.5  17.9  18.6  19.9  16.5  22.2
TOT   84.3  90.6  73.2  91.2  82.8  92.6  86.6  84.0  94.2  77.4
```

And finally, I did 10 more runs in the sighted condition:

```
RMS   11.6  13.0   9.6  11.8  10.8  10.6  10.2  10.4  10.6  10.2
TOT   99.1  97.0 100.0 100.0 100.0 100.0 100.0 100.0 100.0 100.0
```

The runs last 18 seconds on my machine. When I don't move the mouse at all during a run, the RMS error is 141.3 and the percent time on target is 9.5.

In the sighted condition, most of the error came in the start-up transient when the target suddenly began to move. I learned to synchronize my tapping of the space bar to start the run and the initial leftward movement of the mouse, so I could keep the initial transients smaller than the target size. Without those initial transients, the RMS errors might have been a little smaller.

In the blind condition, my best strategy seemed to be to try to generate a smooth sinusoidal movement of the cursor and mouse (the feel of moving the mouse was definitely part of it). When the target reappeared I was usually a bit off and had to make a quick correction before it disappeared again. I might have been able to do better with continued practice, but my problem seems to be that I can't generate a smooth regular sine wave -- it's always drifting in speed or amplitude or both. Maybe some of you slightly younger guys would do better.

When the target wasn't present, I didn't get any feeling of imagining it. I tried once or twice but quickly got off the target and went back to the other way, which was just to try to reproduce the remembered sight and feel of the cursor and mouse when I was tracking well (as nearly as I could).

In the blind condition, with the target invisible, I was tracking the cursor with my eyes.

For those who can't compile Pascal code, I attach the executable program with the modification mentioned above. It will be MIME encoded, unless your program (like Eudora Light) automatically decodes it.

Best,

Bill P.

```
--=====================_851943108==_
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Content-Transfer-Encoding: base64
Content-Disposition: attachment; filename="trkblnd1.exe"

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```

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End of CSGNET Digest - 29 Dec 1996 to 30 Dec 1996
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Date: Wed, 1 Jan 1997 08:00:00 -0600
Subject: CSGNET Digest - 31 Dec 1996 to 1 Jan 1997

There are 21 messages totalling 1477 lines in this issue.

Topics of the day:

1. Simulation-based stabilization vs. control (7)
2. evolution, etc. (5)
3. knowledge <--> adaptation (3)
4. Re.: TrkBlnd1.pas (2)
5. Dawkins on Variable Mutation Rate
6. Grinding Axes in '87
7. Fixed Action Patterns vs. Fixed Motor Patterns (2)

Date: Tue, 31 Dec 1996 07:06:21 -0700
From: Bill Powers <powers_w@FRONTIER.NET>
Subject: Re: Simulation-based stabilization vs. control

[From Bill Powers (961231.0615 MST)]

Rick Marken (961230.1830) --

You said some things awfully well in this post:

>Although the output is computed as part of a control of (imagined) input
>loop, I think it's still best to call it "computed output" because it is
>not being _used_ to influence a variable in the loop;
>it is simply being "tapped off" this loop and added to a non-loop
>variable (what you are calling the CV, although it is _not_ a CV because
>it isn't _controlled_). The closed loop is being used as the _means_ of
>computing the output that is added to a variable that will (because we
>know the disturbance to that variable) be stabilized by this output.

That is exactly it. The closed-loop part of this arrangment isn't being used for control of the CV; it's being used as an indirect way of computing the output needed to have a specific open-loop effect on the CV under the assumption that the disturbance and the environment are not going to change their natures.

Bruce A:
>> As for whether the CV is controlled in a control system, I'm
>> sticking by my original statement: yes, it is controlled, but
>> only for so long as the link between CV and p is constant.
>
Rick:
>If the link between CV and p changes then CV is no longer the CV. CV is
>the variable that p IS NOW a function of. Try thinking of it this way.
>Say that p = 3*(x+y) so CV = 3*(x+y). Now suppose the perceptual
>function changes so that p = x-y. Now CV = x-y. This new variable is now
>the controlled variable (CV); the environmental correlate of p that is
>controlled by the control system.

Again, right on target. You said what I've been trying to say but having trouble expressing. The CV is _defined by_ the perceptual function. It doesn't just exist out there in the environment while the organism tries to find a perceptual function that will represent it. The CV is WHATEVER CORRESPONDS TO THE PERCEPTUAL SIGNAL. There may be many different external circumstances that will yield the same perceptual signal; the control system doesn't care which one exists. Suppose the control task is to place a child's alphabet block on a table so one side faces north. Which side? It doesn't matter. There are six different physical orientations that will yield the perception "one side facing north." The object of control isn't to create a unique physical situation; it's to produce ANY physical situation that will result in the specified perception.

There are two very different perspectives on behavior involved in this discussion. One of them takes the externalized observer-centered point of view (that of the commercial control engineer, for example) in which there is a given environment full of things and processes that the organism has to learn to control. The other takes the organism-centered point of view in which the organism controls its perceptions by acting on a world of which it knows nothing _a priori_.

I don't need to elaborate on the first perspective; it's the one we assume when we are unaware that our own perceptions create the world we experience. In the second way of looking at behavior, the organism's task is to organize perceptions that (a) are controllable, and (b) produce benefits for the

organism when controlled in particular states. In this view, there is no one unique world; the organism tests different ways of perceiving until it finds ways that give it a controllable world. What the external observer sees being controlled follows from this process.

However, the observer-centered viewpoint tends to lead us to think that once we have found a controlled variable, we have found the _objective thing_ that the organism is controlling. We then unconsciously assume that this thing exists there in the environment (because, after all, we are perceiving it!), and it is ANY organism's task to learn to perceive this variable and learn to control it via that perception. This leads into errors such as assuming that because we see one organism controlling a particular variable, another organism will control the same variable. We discover that one organism controls z = 3x + 4y, and reify z so we begin looking for other organisms that also control z. The next organism may actually control 4x + 3y, but if 3x + 4y is stabilized against arbitrary disturbances, 4x + 3y will also be stabilized against many of them, although not as precisely. If we're used to our predictions not working very well, we'll just assume that there are variations among organisms, so some of them can control z better than others can. In fact, both organisms might be controlling quite precisely, but the external correlates of their perceptions are a little different.

Thanks for cutting through the haze, Rick.

Best,

Bill P.

Date: Tue, 31 Dec 1996 08:40:16 -0600
From: Bruce Abbott <abbott@CVAX.IPFW.INDIANA.EDU>
Subject: Re: Simulation-based stabilization vs. control

[From Bruce Abbott (961231.0940 EST)]

>Bill Powers (961230.1650 MST) --

> . . . when we consider
>the effects of a changing reference signal, it becomes obvious that the
>disturbance isn't the only thing that has to be simulated in such a system;
>the simulation must also include the effects that the system's output has on
>the environment through its effectors and eventually on perceptions even
>without any disturbances present.

Yes, I've already noted that: the simulation must include a model of the EFF.

>The simulation must include the properties
>of all the lower-level systems involved, the effects of the outputs of those
>systems on the external world, the effects of the external world on the
>lowest level of sensors, and the manner in which higher-order perceptions
>are derived from lower-order perceptions.

A given control system cannot distinguish between a change in its own reference signal and a disturbance to p, so I don't see how manipulating the reference changes the requirements of the system from what they are when the system is simply opposing disturbances to p. Either way, the system need know nothing about how its lower-level systems work their magic. A

simulation-based system would need only to model the predictable component of variation in its own CV _as it perceives it_ and the way in which variation in its outputs relate to changes in that CV. It need not know or care about how its lower-level systems bring about that relationship. That's how I see it; if you see it differently you'll have to explain to me what I'm overlooking.

>I am interested in the general problem of how we manage to fill in the
>perceptual gaps when we control under adverse conditions. But to come up
>with a workable model requires more than just saying that the brain can
>simulate disturbances of a simple known nature, or that by some unspecified
>means the brain can construct a working model of its own lower levels and
>their interactions with the external world. The essence of the problem is to
>find a way in which the _effect_ of doing all these complex things can be
>achieved by an arrangement that is _simple_, rather than just taking the
>straightest route from A to B without regard to all the implied
>complexities. What puts me off any proposals offered so far is their lack of
>elegance and simplicity, their assumption that whatever needs to be done can
>be done by the brain, as if it had infinite computing capacities and
>whatever unnamed abilities turn out to be necessary.

I feel like I'm battling the hydra -- cut off one head and two grow to take its place. The demo I provided purposely omitted any mechanism whereby CVm and the modeled EFF would come into being. I wanted to clarify the properties of the simulation-based system as compared to those of an ordinary control system, assuming that the unspecified mechanism had already done its job. If it turned out that the system performed poorly even under these rather ideal conditions, there would be no need to tackle the modeling problem. So now you're complaining that I haven't provided a mechanism.

You are quite right that any serious consideration of the problem must ultimately include concrete proposals as to mechanism. Perhaps someone will want to take on that job. I've already done what I set out to do, which was to offer what I hoped would be a clearer view of the systems under discussion.

>I can easily agree that IF the brain could simulate a simple sine-wave
>disturbance, and IF that disturbance could pass through a proper simulation
>of the environment and the perceptual system, THEN the simulated perception
>would behave just like the real one, and control of the simulated variable
>would be accompanied by control of the real one. It seems to me that this
>conclusion is built into the conditions that are assumed.

That is true of all deduction. The purpose of deductive logic is to reveal the implications that may be hidden within the assumptions.

>What bothers me is
>the lack of attention to explaining HOW THIS CAN HAPPEN. How does the brain
>find out that what it needs to simulate is a simple sine-wave, instead of,
>for example, a damped cosine wave or a waveform that is only somewhat like a
>sine wave? How is this simulated disturbance generated, how is the starting
>time determined, what adjusts the simulation until it matches the real
>thing? Hans Blom's model at least addresses these questions, although it
>doesn't explain where the basic form of the simulation comes from and
>assumes that no matter how complex the required simulation, the brain can
>handle it. None of the other proposals I have heard even mentions this problem.

See above. The omission was intentional.

>>Also, I think "computed input" would be a better term

>>than "computed output" to describe it; the output is determined exactly as
>>in an ordinary control system, what differs is that the simulation-based
>>system computes what _input_ it predicts should be present, moment by >moment.

>Yes, this is a major departure from Hans Blom's model and one that I
>proposed some months ago. In this model, the simulated perceptual signal is
>compared directly with the reference signal, with the error acting on the
>simulation in the usual (for PCT) way. This makes the computation of the
>inverse of the simulated environment unnecessary, or considerably
simplifies it.

Ya listenin', Rick?

>I think that when we try to apply this general concept to a modular
>hierarchical system, where each control system controls a simple
>one-dimensional variable, the "simulation" problem will reduce to a much
>simpler one, the problem of generating a single signal that is a simple
>function of the error signal -- essentially what I proposed as an
>imagination connection in B:CP. The need for such connections to explain
>phenomena like planning, thinking, imagining, and dreaming was recognized 23
>years ago, and I really don't think that any of us has progressed much
>beyond that by way of proposing an actual workable model.

In fact, in my looser constructions of the idea of simulation-based control
I've had the imagination connection very much in mind. In Trkblnd1, one of
the issues I think both of us attended to while doing the exercise was
whether we were attempting to imagine the invisible target moving along its
trajectory or trying to maintain the rhythm of the kinesthetic sensations
arising from mouse movement. I found that I could do both. That's an
unfortunate problem with this approach -- there are secondary perceptions
one can switch to controlling when the target disappears, other than the
imagined target position. However, doesn't switching to control of
mouse-movement rhythm still involve generating an internal, time-varying
reference keyed to a remembered rhythm? If so, then this playback acts as a
kind of surrogate perception to which the mouse movements are matched, i.e.,
simulation-based control.

>>As for whether the CV is controlled in a control system, I'm sticking by
>>my original statement: yes, it is controlled, but only for so long as the
>>link between CV and p is constant. During dark adaptation, a person may be
>>allowed to control the perceived brightness of a small, dim spot of light,
>>holding it at a particular level. As adaptation proceeds, the person will
>>be observed to gradually _decrease_ the objective intensity of the light,
>>but will _report_ that the _perceived_ intensity remains constant. In
>>this case control _stabilizes p while _destabilizing_ CV.

>Let's be careful about shifting the focus to the environment. In the case
>you describe, what matters to the organism isn't the objective intensity of
>the light, but the effect on the sensor signals. In fact the state of the
>objective correlate of the perception (assuming there is one) is important
>only through the route of intrinsic variables. Most of the time what matters
>is the mutual consistency of our perceptions; the objective situation is
>secondary. We adjust the lights until we can see clearly; the actual level
>of illumination needed for clear vision is almost irrelevant.

The only things the organism knows about are its perceptions. If your
hearing is failing, you may keep turning up the volume on your hearing aid
and eventually may begin to accelerate the loss rate by over-stimulating
(and thereby damaging) the remaining hair-cell receptors of the cochlea. As

for the importance of intrinsic variables, I have already described (to Rick) how reorganization would be expected to readjust the system so as to reduce the error, in those cases wherein a changed relationship betwee p and CV brought about serious error in the intrinsic variables.

>I'm concerned that another part of the PCT message is getting lost in this
>discussion of degrees of stabilization. The discussions going on now seem to
>imply that a little stablization is better than none, with the kind provided
>by true control simply being a little better than the other kinds. To me,
>this implies a failure to recognize just how universal true control is, how
>exceedingly accurate it is, and how very different the behavior of organisms
>would look if sensitive, accurate control didn't exist.

I certainly do not mean to imply that true control is simply a little better than other kinds of stabilization. As my analysis shows, simulation-based control is a rather brittle form, requiring the existence of highly favorable conditions if it is to confer any benefit at all. It simply cannot deal with random disturbances, and in many systems (particularly those at the lowest levels), rather large, unpredictable disturbances are the rule. The purpose of my analysis was to highlight these differences, not to sweep them under the rug.

>If we're going to talk about simulation-based control, I think it's a bad
>mistake to start out by assuming simple predictable disturbances acting in a
>reliable way. That's not the GENERAL case; it's a very special case, and any
>system that is organized only to deal with that special case is simply going
>to fail when put into a real environment. The PCT model handles the general
>case; it doesn't need disturbances to be predictable; it doesn't need
>single-valued input or output functions; it handles predictable environments
>exactly as it handles unpredictable ones, because controlling in the
>presence of unpredictable disturbances includes the case of controlling in
>the presence of predictable disturbances. Even though a disturbance is, in
>principle, predictable, it doesn't need to be predicted in order to be
>resisted -- unless you've set up the model so it _always_ needs to predict.

Again, I started by assuming a best-case senario for simulation-based control in order to evaluate its performance under those conditions, not because I assume that such conditions are typical. That's how analysis usually proceeds: we start with the simple case ("consider a frictionless spring") and then, when we understand that, move on to more realistic cases. I am well aware of the advantages of ordinary control in handling both predictable and unpredictable disturbances.

>Sorry to go on so long on this, but it really seems to me that in talking
>about simulation-based control in the way that's been going on is simply
>leading us back into the older view of behavior, in which behavior is seen
>as only approximate, imprecise, stochastic, and fundamentally variable.
>That's the legacy of a century and a half of trying to apply a wrong model,
>and blaming all the difficulties on nature instead of on the modeler.

I don't see it that way at all, but if you think discussing this issue tends to obscure the main message you wish to convey on CSGnet, I'm willing to drop it. In fact, I've already said about all I have to say on the matter.

Regards,

Bruce

Date: Tue, 31 Dec 1996 08:01:59 -0700
From: "T. B. Harms" <tbh@TESSER.COM>
Subject: Re: Simulation-based stabilization vs. control

*from Tracy Harms (961231.0751 MST)

Bill Powers (961231.0615 MST)

>There are two very different perspectives on behavior involved in this
>discussion. One of them takes the externalized observer-centered point of
>view (that of the commercial control engineer, for example) in which there
>is a given environment full of things and processes that the organism has to
>learn to control. The other takes the organism-centered point of view in
>which the organism controls its perceptions by acting on a world of which it
>knows nothing _a priori_.

I'd love to hear some elaboration on this final comment. I'll agree that
we cannot accept the Kantian picture of a priori qualities to knowledge,
but on the other hand it does seem to me that in terms of the origin and
development of individual organisms a great bulk of their knowledge is
prior-to-the-fact of their interaction with the world. For example, before
young land-animals are exposed to the open atmosphere they often grow
lungs. Here we encounter knowledge about the world which cannot be seen as
after-the-fact *for that individual*.

Tracy Bruce Harms tbh@tesser.com
Boulder, Colorado caveat lector!

Date: Tue, 31 Dec 1996 09:24:49 -0700
From: Bill Powers <powers_w@FRONTIER.NET>
Subject: Re: Simulation-based stabilization vs. control

[From Bill Powers (961231.0800 MST)]

Bruce Abbott (961231.0940 EST)]

>A given control system cannot distinguish between a change in its own
>reference signal and a disturbance to p, so I don't see how manipulating
>the reference changes the requirements of the system from what they are
>when the system is simply opposing disturbances to p. Either way, the
>system need know nothing about how its lower-level systems work their >magic.

There are always some dynamic effects involve in control; when you vary the
reference signal for a control system, its perceptual signal doesn't follow
it instantly and perfectly. The simulation must take that into account to be
stable. Also, higher-level control systems must in general adjust many
lower-level reference signals at the same time, leading to multiple effects
in the environment, any of which might involve a disturbance. The
environment, in general, will be nonlinear, so its response to action will
depend on the setting of the reference signal. A simulation-based control
process must take all these effects into account to achieve stable control
that mimics direct control of the external CV. Finally, the general case of
higher-level control includes perceptual inputs that are not themselves
under control; they represent aspects of the environment that are not in the

loop for lower systems. When a dog chases a cat, it can affect its own position relative to the environment, but not the cat's position. One element of the controlled relationship is not involved in any of the dog's lower-level control loops. Simulation-based chasing control would have to model where the cat is going to go.

>I feel like I'm battling the hydra -- cut off one head and two grow to
>take its place. The demo I provided purposely omitted any mechanism
>whereby CVm and the modeled EFF would come into being. I wanted to clarify
>the properties of the simulation-based system as compared to those of an
>ordinary control system, assuming that the unspecified mechanism had
>already done its job.

I understand what you were doing, but I still think that this approach begs the question. What this way of modeling does is to postulate WHATEVER PROPERTIES ARE NEEDED TO MAKE THE MODEL WORK EVEN IF THEY ARE IMPOSSIBLE TO STATE OR ACHIEVE. And of course those properties are not actually stated; what is assumed is that the _final effect_ is achieved by an undefined mechanism, never mind how.

What you end up with is a trivial conclusion. If the simulation were a perfect representation of the external EFF, and if the simulated disturbance were a perfect imitation of the actual disturbance, then the output of the simulation would be the same as the output of the real EFF. That's not even a deduction: it's just saying the same thing twice in different words. It's just saying that if the simulation is perfect, it is perfect (and of course it's also saying that to the extent that it's not perfect, it's not perfect). But we knew that to begin with.

> If it turned out that the system performed poorly even under
> these rather ideal conditions, there would be no need to tackle the
>modeling problem. So now you're complaining that I haven't provided a
>mechanism.

There is no way that the system could perform poorly unless you postulate that it performs poorly -- for example, by postulating that the modeled disturbance is different from the real one. If you could build a perfect stimulator, it would simulate perfectly. If it simulated poorly, it would simulate poorly. The whole problem is HOW SUCH A SIMULATION COULD EXIST.

>You are quite right that any serious consideration of the problem must
>ultimately include concrete proposals as to mechanism. Perhaps someone
>will want to take on that job. I've already done what I set out to do,
>which was to offer what I hoped would be a clearer view of the systems
>under discussion.

OK, granted. I'm probably being too hard-nosed about this.

>>Yes, this is a major departure from Hans Blom's model and one that I
>>proposed some months ago. In this model, the simulated perceptual signal
>>is compared directly with the reference signal, with the error acting on
>>the simulation in the usual (for PCT) way. This makes the computation of
>>the inverse of the simulated environment unnecessary, or considerably
>>simplifies it.
>
>Ya listenin', Rick?

It still requires the _forward_ simulation to exist, and that is still a major unsolved problem. I wish we could get off the subject of who is right,

you or Rick. That is a very boring subject. Are you guys in some sort of contest? What's the prize?

> .. an
>unfortunate problem with this approach -- there are secondary perceptions
>one can switch to controlling when the target disappears, other than the
>imagined target position. However, doesn't switching to control of
>mouse-movement rhythm still involve generating an internal, time-varying
>reference keyed to a remembered rhythm?

It could involve an oscillator in the output function that is variable with repect to amplitude, phase, and frequency. It obviously involves the ability to _perceive_ in those terms. A perception of a constant frequency of oscillation would be a _steady_ signal indicating frequency, not an oscillating signal. The reference signal would also be a steady signal if the frequency is to be held constant. I don't yet have a model for control of oscillatory variables.

>If so, then this playback acts as a
>kind of surrogate perception to which the mouse movements are matched,
>i.e., simulation-based control.

That's not what simulation-based control means. Simulation-based control involves a simulation that connects the error signal to the simulated perceptual signal; the simulation is of a property of the environment. You're just describing a reference signal.

> I have already described (to
>Rick) how reorganization would be expected to readjust the system so as to
>reduce the error, in those cases wherein a changed relationship between p
>and CV brought about serious error in the intrinsic variables.

See my earlier post today, agreeing with Rick's. The CV is not defined independently of p. It is simply the inverse input function of p. Reorganization changes, among other things, the form of the perceptual input function, thereby changing the definition of CV. There's no way that p can have a "changed relationship" with the _same_ CV.

>I certainly do not mean to imply that true control is simply a little
>better than other kinds of stabilization. As my analysis shows,
>simulation-based control is a rather brittle form, requiring the existence
>of highly favorable conditions if it is to confer any benefit at all. It
>simply cannot deal with random disturbances, and in many systems
>(particularly those at the lowest levels), rather large, unpredictable
>disturbances are the rule. The purpose of my analysis was to highlight
>these differences, not to sweep them under the rug.

OK, good. I agree with your purpose.

Best,

Bill P.

Date: Tue, 31 Dec 1996 11:20:39 -0500
From: Bruce Gregory <gregory@CF4.HARVARD.EDU>
Subject: Re: Simulation-based stabilization vs. control

[From Bruce Gregory (961231.1130 EST)]

Tracy Harms (961231.0751 MST)

> I'll agree that
> we cannot accept the Kantian picture of a priori qualities to knowledge,
> but on the other hand it does seem to me that in terms of the origin and
> development of individual organisms a great bulk of their knowledge is
> prior-to-the-fact of their interaction with the world. For example, before
> young land-animals are exposed to the open atmosphere they often grow
> lungs. Here we encounter knowledge about the world which cannot be seen as
> after-the-fact *for that individual*.

I fear we are in danger of using the word "knowledge" in such a
broad way that it is almost meaningless. Mach argued that inertia
is an effect of the total distribution of mass in the universe. I would
be hesitant to say that a rock has "knowledge" of this mass distribution
by virtue of its inertia.

Bruce Gregory

Date: Tue, 31 Dec 1996 08:58:31 -0800
From: Rick Marken <marken@LEONARDO.NET>
Subject: Re: Simulation-based stabilization vs. control

[From Rick Marken (961231.0900 PST)]

Bill Powers (961231.0615 MST)

>Thanks for cutting through the haze, Rick.

And thanks for pointing out the scenic view beyond.

Bruce Abbott (961231.0940 EST)

> doesn't switching to control of mouse-movement rhythm still involve
> generating an internal, time-varying reference keyed to a remembered
> rhythm?

Yes, as part of the process of controlling the _perception_ of
sinusoidal mouse movement.

>If so, then this playback acts as a kind of surrogate perception to
>which the mouse movements are matched, i.e., simulation-based control.

There is no "playback" in the PCT model. What happens is that a
time-varying lower level reference is generated as the output used to
produce the _perception_ of a time-varying mouse movement. The reference
for
the perception of mouse movement is probably selected from memory on
the basis of a match with the remembered perception of cursor movement.
But once the reference is selected (by whatever means) the process is
good old control of _perception_.

The PCT model of "blind" tracking differs substantially from the
simulation-based control model. The PCT model controls a _perception_

of mouse movement; the simulation - based control model, on the other hand, controls only its _imagination_ of cursor (or mouse) movement.

Best

Rick

Date: Tue, 31 Dec 1996 10:04:46 -0700
From: Bill Powers <powers_w@FRONTIER.NET>
Subject: Re: Simulation-based stabilization vs. control

[From Bill Powers (961231.0930 MST)

Tracy Harms (961231.0751 MST)

Sorry we couldn't get together on my trip to Boulder. There will be more chances.

>I'll agree that
>we cannot accept the Kantian picture of a priori qualities to knowledge,
>but on the other hand it does seem to me that in terms of the origin and
>development of individual organisms a great bulk of their knowledge is
>prior-to-the-fact of their interaction with the world. For example,
>before young land-animals are exposed to the open atmosphere they often
>grow lungs. Here we encounter knowledge about the world which cannot be
>seen as after-the-fact *for that individual*.

I think we have to maintain a distinction between knowledge acquired by an individual during one lifetime and knowledge passed on across generations. At the very least, different time-scales are involved, and certainly different mechanisms.

I also think that Lamarck should by no means be put prematurely to rest. The experiment with cutting the tails off mice was a silly way to test the thesis that acquired characteristics can be inherited. If you think of an acquired characteristic as a change that the organism has instituted for its own purposes, then clearly what the environment may do to the organism is not a "characteristic." If you think of characteristics as resulting from internal reorganizing processes (which can include mutations), then there is really no difference between natural selection (as it actually is carried out by the organism) and inheritance of acquired characteristics.

When you think of it this way, the need for a mechanism becomes even more obvious. How do organisms pass along some of the things that are obviously passed along, like lungs? The standard answer is "DNA," but a lot more is passed from parents to children than DNA. The entire contents of the mother's egg is passed along, including many complex proteins and protein structures which can easily include functioning biochemical control systems in full working order. This is true even in bacteria. Human parents pass to their children a predisposition to develop a hierarchy of control systems appropriate to the environment of a single lifetime. But they also pass along, at least in some species, some rather astonishing _behavioral_ characteristics, which are very hard to explain.

Lake Michigan, some tens of thousands of years ago, had a land bridge across its middle. Certain birds, whose names elude me, apparently established a migration path south along the lakeshore, then across the land bridge, then

on further south on the other side. They still follow that path, although the land bridge has been gone for thousands of years. The simplest-seeming explanation for why they cross just where the land bridge was is that somehow they remember the path as it used to be -- across hundreds or thousands of generations. This naturally creates certain technical problems for any model that hopes to explain how such things can happen.

If we look at evolutionary changes and learning processes of a single lifetime as examples of a general reorganizing capability, as Gary Cziko has suggested, then we can see all "knowledge", on all time-scales, as a process of creating perceptions (and corresponding CVs) that are useful for the purpose of maintaining life. We have to remember that all aspects of an organism are, first of all, _human perceptions_. A "lung" is a human perception. It is only secondarily and hypothetically a physical structure that can absorb oxygen and release CO2. If we see "lungness" as a controlled variable (on the evolutionary time scale), that is just how WE see it; the actual controlled variable which is served by growing lungs is something known only to the organism in question, and "known" only in the sense of biochemical processes.

Behind this discussion there is another one, concerning the way different and independent reorganizing processes seem to converge on outcomes that look very similar to us human observers. This is another hint that there is actually a real environment of some sort out there. But that's another subject.

Best,

Bill P.

Date: Tue, 31 Dec 1996 11:26:34 -0700
From: "T. B. Harms" <tbh@TESSER.COM>
Subject: evolution, etc.

*from Tracy Harms (961231.1130 MST)

Bill Powers (961231.0930 MST)

>If you think of an
>acquired characteristic as a change that the organism has instituted for its
>own purposes, then clearly what the environment may do to the organism is
>not a "characteristic." If you think of characteristics as resulting from
>internal reorganizing processes (which can include mutations), then there is
>really no difference between natural selection (as it actually is carried
>out by the organism) and inheritance of acquired characteristics.

I'm afraid I don't follow that proposal. How might mutation count as an internal reorganizing process?

>When you think of it this way, the need for a mechanism becomes even more
>obvious.

I don't understand what this talk of "need for a mechanism" accomplishes. It must have something to do with your reconsideration of Lamarckian theories of adaptation, right? That's as close as I can get, however.

>How do organisms pass along some of the things that are obviously

>passed along, like lungs? The standard answer is "DNA," but a lot more is
>passed from parents to children than DNA. [...]

Yes, and Gerald Edelman develops something like this point in _Topobiology: An Introduction to Molecular Embryology_. I don't see how this moves us at all away from neo-darwinian theory.

>If we look at evolutionary changes and learning processes of a single
>lifetime as examples of a general reorganizing capability, as Gary Cziko has
>suggested, then we can see all "knowledge", on all time-scales, as a process
>of creating perceptions (and corresponding CVs) that are useful for the
>purpose of maintaining life.

I'm not so sure of that. Yes, the PCT structure can be metaphorically extended, but you've been warning us against doing that and I think your warnings are correct. There was a feedback process in the production of the turtles' shells, but I agree with you that it was not a *control* system. I propose that we need to distinguish the differences between within-organism control, among-organism control, and something-or-other non-control, where at least one of the somethings-or-other is the evolutionary adaptation which produces speciation. I'm pretty sure it will not conform to a control-loop explanation, thus we'll need to become handy with the differences.

>We have to remember that all aspects of an
>organism are, first of all, _human perceptions_.

I cannot accept this way of dealing with things. It is entirely reasonable to presume that there are any diversity of aspects to things which are neither our perceptions nor dependent upon them.

>A "lung" is a human perception. It is only secondarily and hypothetically
>a physical structure that can absorb oxygen and release CO2.

"Lung" is a human *concept*, which may indicate, to some degree of adequacy, something about vertibrate physiology. A lung, however, is that thing which is meant by the word "lung," which is usually an organ, *not* a perception *nor* a concept.

>If we see "lungness" as a controlled
>variable (on the evolutionary time scale), that is just how WE see it; the
>actual controlled variable which is served by growing lungs is something
>known only to the organism in question, and "known" only in the sense of
>biochemical processes.

Again, I don't think the evolution of organisms will fit the control-system model. If not, "lungness" can't be a controlled variable.

>Behind this discussion there is another one, concerning the way different
>and independent reorganizing processes seem to converge on outcomes that
>look very similar to us human observers. This is another hint that there is
>actually a real environment of some sort out there. But that's another subject.

And one which has been touched on before. You are far too cautious in refering to the imperceptive continuum of stuff at large, a.k.a. reality. I recognize that some people take it far too much for granted that said reality is simply what appears to us: It is not. But you overcorrect as you shy from those errors, Bill.

Tracy Bruce Harms tbh@tesser.com
Boulder, Colorado caveat lector!

Date: Tue, 31 Dec 1996 11:44:52 -0700
From: "T. B. Harms" <tbh@TESSER.COM>
Subject: knowledge <--> adaptation

*from Tracy Harms (961231.1140 MST)

Bruce Gregory (961231.1130 EST)

>I fear we are in danger of using the word "knowledge" in such a
>broad way that it is almost meaningless. Mach argued that inertia
>is an effect of the total distribution of mass in the universe. I would
>be hesitant to say that a rock has "knowledge" of this mass distribution
>by virtue of its inertia.
>
>
>Bruce Gregory

I don't share your fear. It is clear to me that knowledge, even in the broadened sense I've pushed for, is *contingent.* That is, it is something extra that needs explanation beyond the more-or-less universal properties of things. Mass, friction, space-filling, heat -- such things are neatly explained without consideration of local conformance, or fit, between a fragile structure and its environment.

If you object to the work 'knowledge' as I use it, we could switch to speaking of adaptation, as the two concepts are more or less equivalent for me at this time.

Tracy Bruce Harms tbh@tesser.com
Boulder, Colorado caveat lector!

Date: Tue, 31 Dec 1996 15:22:09 -0500
From: Bruce Gregory <gregory@CF4.HARVARD.EDU>
Subject: Re: evolution, etc.

[From Bruce Gregory (961231.1525 EST)]

Tracy Harms (961231.1130 MST)
>
>
> Bill Powers (961231.0930 MST)
>
>
> >We have to remember that all aspects of an
> >organism are, first of all, _human perceptions_.

Sloppy speaking. You told us that a perception is
the existence of a neural signal in a perceptual pathway.
You _may_ mean that all aspects of an organism are

inferences based on perceptions.

> I cannot accept this way of dealing with things. It is entirely reasonable
> to presume that there are any diversity of aspects to things which are
> neither our perceptions nor dependent upon them.

I agree. Noting that these "things" are elements in our
models.

> >A "lung" is a human perception.

Same problem described above.

> >It is only secondarily and hypothetically
> >a physical structure that can absorb oxygen and release CO2.
>
> "Lung" is a human *concept*, which may indicate, to some degree of
> adequacy, something about vertibrate physiology. A lung, however, is that
> thing which is meant by the word "lung," which is usually an organ, *not* a
> perception *nor* a concept.

Lungs are elements of the models that we use to make
sense of the perceived world.

> >If we see "lungness" as a controlled
> >variable (on the evolutionary time scale), that is just how WE see it; the
> >actual controlled variable which is served by growing lungs is something
> >known only to the organism in question, and "known" only in the sense of
> >biochemical processes.
>
> Again, I don't think the evolution of organisms will fit the control-system
> model. If not, "lungness" can't be a controlled variable.

I'd love to see Bill's model. Who is controlling lungness? How?

> >Behind this discussion there is another one, concerning the way different
> >and independent reorganizing processes seem to converge on outcomes that
> >look very similar to us human observers. This is another hint that there is
> >actually a real environment of some sort out there. But that's another
subject.

Again, the existence of "a real environment of some sort out there"
is an element of the model thatvirtually everyone accepts. It is
hardly "hinted at". As we agreed, the existence of a "real world"
is not an inference, but something we cannot avoid. The nature
of this something is guessed at in our models.

Bruce Gregory

Date: Tue, 31 Dec 1996 15:26:17 -0500
From: Bruce Gregory <gregory@CF4.HARVARD.EDU>
Subject: Re: knowledge <--> adaptation

[From Bruce Gregory (961231.1525 EST)]

Tracy Harms (961231.1140 MST)

>
> I don't share your fear. It is clear to me that knowledge, even in the
> broadened sense I've pushed for, is *contingent.* That is, it is something
> extra that needs explanation beyond the more-or-less universal properties
> of things. Mass, friction, space-filling, heat -- such things are neatly
> explained without consideration of local conformance, or fit, between a
> fragile structure and its environment.

The more or less universal properties of things are properties of
our models, not of things independent of those models.

 > If you object to the work 'knowledge' as I use it, we could switch to
> speaking of adaptation, as the two concepts are more or less equivalent for
> me at this time.

I'd definitely prefer this, since the two concepts are _not_ equivalent
for me. Knowledge, for me, refers to our confidence in our models.

Bruce Gregory

Date: Tue, 31 Dec 1996 13:57:48 -0700
From: Bill Powers <powers_w@FRONTIER.NET>
Subject: Re: evolution, etc.

[From Bill Powers (961231.1300 MST)]

Tracy Harms (961231.1130 MST)--

>I'm afraid I don't follow that proposal. How might mutation count as an
>internal reorganizing process?

To understand this you have to understand what we call the E. coli type of
reorganization, named after E. coli's method of steering up and down
chemical gradients. E. coli can't actually steer; all it can do is alter its
direction of travel at random, by brief episodes of tumbling. Despite this
random mode of changing direction, it can travel up and down a a gradient 50
to 70 percent as fast as it could if it could really steer itself.

E. coli senses a _time rate of change of concentration_ of an attractant (or
repellent), generated by its swimming in a straight line through a chemical
gradient. If this rate of change is more positive than a certain amount, E.
coli delays its next tumble; if less positive, it tumbles sooner. The result
is that it spends much more time swimming up the gradient than down it.
Systematic control is achieved by adjusting the timing of a truly random
output (actually determined to be random by measurement).

This principle seems to be unknown to the biologists who are objecting to
the Cairnes et. al. studies of "directed mutation" in E. coli (by
coincidence, the same organism). They think that in order for mutations to
proceed in a systematic direction, there would have to be a change (which
doesn't actually occur) in the distribution of the mutations away from pure
randomness. But that is not so; all that has to change is the _interval
between_ mutations: the mutations themselves can still be completely random.
The way it would work is just the way E. coli steers:

If, after a mutation, the multiplying bacteria experience increasing stress,
the next mutation will occur sooner; if the stress begins to fall, it will

be postponed. This means that the bacteria will multiply for a longer time before another change when the mutation leads to a less stressful condition, with the result of a rapid shift of the population toward forms that reduce the stress. This relationship between level of stress and mutation rate is well known in several kinds of microorganisms, apparently.

To make this into a control system all we need is to postulate that there is some biochemical sensor that detects the stress level (or some chemical concentration related to it), compares it with a reference level, and converts the error into the length of delay before the next mutation. Of course we also have to imagine that random mutations are triggered by the organism itself, rather than by cosmic rays or other random processes alone. The mutation rate could be influenced by, for example, reducing or increasing the number of repair enzymes being manufactured, thus letting natural wear and tear have greater or lesser effects.

I think this gives us a reasonable basis for proposing that evolution is primarily an action by the species rather than a process in which the species participates only passively.

>>When you think of it this way, the need for a mechanism becomes even more
>>obvious.
>
>I don't understand what this talk of "need for a mechanism" accomplishes.
>It must have something to do with your reconsideration of Lamarckian
>theories of adaptation, right? That's as close as I can get, however.

In the above discussion I tried to illustrate the sort of mechanism that would be required.

>>How do organisms pass along some of the things that are obviously
>>passed along, like lungs? The standard answer is "DNA," but a lot more is
>>passed from parents to children than DNA. [...]
>
>Yes, and Gerald Edelman develops something like this point in
>_Topobiology: An Introduction to Molecular Embryology_. I don't see how
>this moves us at all away from neo-darwinian theory.

It says that working biochemical control systems can be passed along from one generation to the next without interruption, including the states of the variables and the functions relating them. It's pretty sure that DNA and RNA -- genes -- are actively involved in local biochemical control processes, and are not simply tape recordings which are played back without themselves being affected. I have proposed a rather fanciful view of evolution in which control processes are involved from the very beginning of life, with the stability of organization being actively maintained rather than simply resulting from a lack of effective disturbances. I don't know how this squares with neo-darwinian theory; can you elaborate?

> Yes, the PCT structure can be metaphorically
>extended, but you've been warning us against doing that and I think your
>warnings are correct.

I'm not intending my extension to be metaphorical. There are quite literal biochemical control systems identified _in vitro_ and _in vivo_, as well as _in computo_, and there are certainly negative feedback systems at the level of DNA, although they have not been formally identified as control systems yet (there are people working on that -- maybe the chief person involved, who listens in on this conversation, could be persuaded to say a few words

on that).

>>We have to remember that all aspects of an
>>organism are, first of all, _human perceptions_.
>
>I cannot accept this way of dealing with things. It is entirely
>reasonable to presume that there are any diversity of aspects to things
>which are neither our perceptions nor dependent upon them.

I think it's necessary to deal with things this way at least once in a while, to keep ourselves honest.

>>A "lung" is a human perception. It is only secondarily and hypothetically
>>a physical structure that can absorb oxygen and release CO2.
>
>"Lung" is a human *concept*, which may indicate, to some degree of
>adequacy, something about vertibrate physiology. A lung, however, is that
>thing which is meant by the word "lung," which is usually an organ, *not*
>a perception *nor* a concept.

How do we know about lungs, except through what our senses tell us? I don't distinguish among perceptions in the old categories, like "concrete" and "abstract" or "sensation -perception - conception". I just call everything a perception, and distinguish among levels by proposing specific classes of perceptions.

Other topics can wait for me to have a nap or two -- I'm sure they'll come up again.

Best,

Bill P.

Date: Tue, 31 Dec 1996 14:05:10 -0700
From: Bill Powers <powers_w@FRONTIER.NET>
Subject: Re: evolution, etc.

[From Bill Powers (961231.1400 MST)]

Bruce Gregory (961231.1525 EST) --
>> >We have to remember that all aspects of an
>> >organism are, first of all, _human perceptions_.
>
>Sloppy speaking. You told us that a perception is
>the existence of a neural signal in a perceptual pathway.
>You _may_ mean that all aspects of an organism are
>inferences based on perceptions.

Aw, it's not THAT sloppy. Inferences are neural signals in perceptual pathways, too, aren't they? How else would you know about them?

>Lungs are elements of the models that we use to make
>sense of the perceived world.

And we know about these models because they are signals in perceptual pathways. Everything we can experience is a signal in a perceptual pathway, it says here. Maybe that's wrong, but if so, I'd like to know how.

>Again, the existence of "a real environment of some sort out there"
>is an element of the model thatirtually everyone accepts. It is
>hardly "hinted at". As we agreed, the existence of a "real world"
>is not an inference, but something we cannot avoid. The nature
>of this something is guessed at in our models.

I'm simply emphasizing the guesswork. Maybe not all lungs are identical.

Best,

Bill P.

Date: Tue, 31 Dec 1996 13:10:43 -0800
From: Rick Marken <marken@LEONARDO.NET>
Subject: Re: evolution, etc.

[From Rick Marken (961231.1305 PST)]

Bill Powers (961231.0930 MST)--

> We have to remember that all aspects of an organism are,
> first of all, _human perceptions_.

Bruce Gregory (961231.1525 EST) --

> Sloppy speaking.

I don't think so.

> You told us that a perception is the existence of a neural signal
> in a perceptual pathway.

Right. And things like arms, legs and lungs are precisely that
(according to PCT): neural signals in perceptual pathways.

>You _may_ mean that all aspects of an organism are inferences based
> on perceptions.

No. I think he means what he said.

We can and do make inferences based on our perceptions. These
inferences are perceptions too, of course.

Perceptually yours

Rick

Date: Tue, 31 Dec 1996 18:50:42 -0700
From: "T. B. Harms" <tbh@TESSER.COM>
Subject: Re: knowledge <--> adaptation

*from Tracy Harms (961231.1844 MST)

Bruce Gregory (961231.1525 EST)

>The more or less universal properties of things are properties of
>our models, not of things independent of those models.

This reminds me of Korzybski's homily "the map is not the territory." True as that is, you are hip to the fact that it is the things (independent of our models) which change as they do. Insofar as those changes conform with what our envisioned models lead us to expect, I don't see why the regularity itself, in the stuff itself *not in our models*, might not be labelled a "property." Contrary to your assertion, that is.

>> If you object to the work 'knowledge' as I use it, we could switch to
>> speaking of adaptation, as the two concepts are more or less equivalent for
>> me at this time.
>
>I'd definitely prefer this, since the two concepts are _not_ equivalent
>for me. Knowledge, for me, refers to our confidence in our models.

Not an uncommon view, I'm sure. The word has come to us with denotations of truth, certainty, confidence, recognition, experience, awareness, skill, intimacy, and more. Not surprisingly in formal studies a lot of these meanings end up being discarded as various problems and solutions are clarified. There appears to be a major alternative between retaining concern with confidence at the expense of concern for truth, or retaining concern with truth at the expense of concern for confidence. I've chosen the latter route. Thus knowledge, for me, has nothing to do with our confidence in our models. Instead it has to do with the improvement of models. And whereas Bill Powers seems to like to think of knowedge in terms of perceptual experience, my emphasis is on relational structures which are not themselves perceptions.

Tracy Bruce Harms tbh@tesser.com
Boulder, Colorado caveat lector!

Date: Tue, 31 Dec 1996 22:38:52 -0500
From: "David M. Goldstein" <davidmg@JERSEY.NET>
Subject: Re.: TrkBlnd1.pas

From: David Goldstein
Subject: Powers 961229.1945 MST
Date: 12/31/96

When one looks at the design of this study, it sure looks like the standard way of doing research I was taught: There is an independent variable, sighted versus blinded. Each subject experiences all values of the variable ("within-S variable"). There is a second independent variable, replications. There are two different dependent variables, mean and standard deviation of performance in the tracking task.

One could apply the analysis of variance to determine whether the mean difference between the sighted versus blinded condition was more than one would expect by chance. One could estimate the magnitude of the effect.

This sure looks very traditional in terms of research design. I must be missing something. Can someone point out what I am missing here?

Date: Tue, 31 Dec 1996 21:47:50 -0600
From: Gary Cziko <g-cziko@UIUC.EDU>
Subject: Dawkins on Variable Mutation Rate

[from Gary Cziko 970101.0313 GMT]

Responding to Bill Powers (961231.1300 MST) who said:

>If, after a mutation, the multiplying bacteria experience increasing stress,
>the next mutation will occur sooner; if the stress begins to fall, it will
>be postponed. This means that the bacteria will multiply for a longer time
>before another change when the mutation leads to a less stressful condition,
>with the result of a rapid shift of the population toward forms that reduce
>the stress. This relationship between level of stress and mutation rate is
>well known in several kinds of microorganisms, apparently.
>
>To make this into a control system all we need is to postulate that there is
>some biochemical sensor that detects the stress level (or some chemical
>concentration related to it), compares it with a reference level, and
>converts the error into the length of delay before the next mutation. Of
>course we also have to imagine that random mutations are triggered by the
>organism itself, rather than by cosmic rays or other random processes alone.
>The mutation rate could be influenced by, for example, reducing or
>increasing the number of repair enzymes being manufactured, thus letting
>natural wear and tear have greater or lesser effects.
>
>I think this gives us a reasonable basis for proposing that evolution is
>primarily an action by the species rather than a process in which the
>species participates only passively.

Greg Williams recently pointed out to me that Richard Dawkins discusses the notion of variable mutation rates (although not from a control-theory perspective) in his latest book _Climbing Mount Improbable_ (1996, New York: Norton).

Dawkins suggests that this could be the case for asexually reproducing organisms but not for sexually reproducing ones. He argues that for sexual organisms specific genes could not "learn" to increase their mutation rate in response to certain environmental conditions since sexual reshuffling would separate the mutator gene from the structural one. But I don't think his argument holds against a general increase in mutation rate throughout the genome of the type I think Bill is arguing for. See Dawkins pp. 82 ff.

Greg also informed me that this idea was something actually entertained by Darwin as discussed in David Depew's _Darwinism Evolving_ (I haven't been able to get my hands a copy of this book yet).

Wishing all a Happy New Year and continued fun on CSGnet,
--Gary

Date: Tue, 31 Dec 1996 21:18:40 +0000
From: Richard Marken <marken@AERO.ORG>
Subject: Grinding Axes in '87

[From Rick Marken (961231.2010 PST)]

My last post of 1996 is inspired (appropriately enough) by a comment Bruce Abbott made a couple days ago. Bruce said something like “I have no theoretical ax to grind”. I don’t know if this is really true but if it is I think it’s too bad.

I guess it’s no secret that I _do_ have a theoretical ax to grind -- and I love it. My theoretical ax is PCT. I grind it on the whetstone of experimental test and I try to wield it to cut back the jungle of cause-effect thinking that has sealed the life sciences into a morass of futile and often cruel ignorance.

Here’s hoping that 1987 -- the 24th anniversary of the publication of _Behavior: The Control of Perception_ -- will be the beginning of the era in which the PCT ax can be used constructively rather than destructively; when it can be used to carve out an explanation of the phenomenon of control rather than to chop down the forest of excuses that continue to be give for continuing the study the cause-effect illusion; to build a science of purpose rather than to chop away at the rotting foundation of experimental psychology.
Here's to life. L'chaim.

A happy and purposeful New Year to all.

Best

Rick

Date: Tue, 31 Dec 1996 23:43:21 -0600
From: Gary Cziko <g-cziko@UIUC.EDU>
Subject: Fixed Action Patterns vs. Fixed Motor Patterns

[from Gary Cziko 970101.0530 GMT]

Here's an interesting passage from a book I've been looking through.

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"Neuroscientists distinguish between a FAP (fixed action pattern), the stereotypical production of some particular motor _result_--a limb movement, say--and an FMP (fixed motor pattern), the electrical activity in the motor neurons that brings about that result, FMPs are comparatively rare. Cricket songs come close; the neural activity involved in the production of these songs is tightly controlled in regard to the number and the {132.1} timing of motor impulses (Ewing and Hoyle 1965). Hoyle (1984, p. 405) describes an even more impressive case: The courtship behavior of a tiny male grasshopper produces stereotypic motor output accurate to within a millisecond of the firing of individual nerve impulses. However, most FAPs--even in insects--are not fixed with regard to the electrial activity in the motor neurons causing the movements. Careful measurements reveals that many thousands of visually identical stepping movements of a locust, for example no two steps exhibit the same underlying pattern of electrical activity in the motor neurons causing the movements (Hoyle, 1984, p. 405). And what is true of insects is even more obviously true of human beings--if not at the level of specific limb movements, then at the level of individual acts. (How many different ways are there of doint one

thing, such as waving to a friend, scratching ones nose, or eating an apple?)" (pp. 131-132)

Dretske, Fred. (1988). _Explaining behavior: Reasons in a world of causes_. Cambridge: MIT Press (A Bradford Book).

==========

The research by Hoyle (1984, p. 405) mentioned seems quite interesting. How do non-PCT neuroscientists explain "visually identical stepping movements" with "no two steps exhibit[ing] the same underlying pattern of electrical activity in the motor neurons causing the movements"?

Dretske makes a good try, but he never seems to realize that a "motor _result" is a perception and that is controlled by behavior.

I hadn't realized that research like this had been done. For those interested, the Hoyle reference is:

Hoyle, G. (1984). The scope of neuroethology. _Behavioral and Brain Sciences_, _7_(3), 367-412.

--Gary

P.S. Wouldn't it be interesting if the only FMPs found were for behaviors that did not normally need to overcome disturbances? Is this the case for a cricket's song?

Date: Wed, 1 Jan 1997 05:13:05 -0700
From: Bill Powers <powers_w@FRONTIER.NET>
Subject: Re: Fixed Action Patterns vs. Fixed Motor Patterns

[From Bill Powers (970101.0500 MST)]

Gary Cziko 970101.0530 GMT --

>Here's an interesting passage from a book I've been looking through.
>
>==========
>
>"Neuroscientists distinguish between a FAP (fixed action pattern), the
>stereotypical production of some particular motor _result_--a limb
>movement, say--and an FMP (fixed motor pattern), the electrical activity
>in the motor neurons that brings about that result, FMPs are comparatively
>rare.

This sort of thing has been noted throughout this century. It was either Charles Sherrington or Hughlings Jackson who said that the cortex "thinks in movements, not muscles." In my (very old) Ranson and Clark neuroanatomy text, there is this:

"Stimulation of a given point of the motor zone [of the cortex] controlling foreleg movements, for example, caused the contralateral foreleg to assume a 'final position,' making whatever preliminary adjustments that were necessary to attain it. Depending on the position prior to stimulation, the limb might begin its approach to the final position with protraction or retraction, movements exactly opposite in direction." (p. 304)

Sherrington called this common effect "the instability of the motor point." I take it as a possible indication that the stimulation was exciting a nerve cell that normally carried a position or joint-angle reference signal.

So even a "fixed action pattern" is not fixed with relation to stimulation of the brain. It all depends, of course, on what level in the hierarchy the investigations happened to have lighted upon. Not knowing about levels of organization, and not thinking in terms of perceptual signals, error signals, and output signals of a control system, these early researchers (and their descendants, apparently) didn't know what they were looking at.

Effects like these keep being discovered, forgotten, rediscovered, and forgotten again, because there has been no underlying model to make sense of them. In the first years of my work on control theory I enthusiastically planned to inhabit the library of the hospital where I worked, going through all the neurology literature there and finding out how the brain is organized, so I could look for control systems. The reality was a bitter disappointment. There were lots of fragments like the above, but they were scattered and disorganized; nothing was followed through. The general impression was sort of like reading one of those Ripley's Believe It Or Not books -- a collection of fascinating vignettes having nothing to do with each other.

>Cricket songs come close; the neural activity involved in the
>production of these songs is tightly controlled in regard to the number >and
>the {132.1} timing of motor impulses (Ewing and Hoyle 1965). Hoyle (1984,
>p. 405) describes an even more impressive case: The courtship behavior of
>a tiny male grasshopper produces stereotypic motor output accurate to
>within a millisecond of the firing of individual nerve impulses.

Isn't this what you'd expect when looking at the output function of a low-level, perhaps first-order, control system in a simple CNS? The real question isn't the relation of the observed result to the driving signals, but where the driving signals come from. Are they error signals? Outputs of higher systems? Spontaneous? Research done without any theoretical questions in mind is essentially random -- useless. Just when you get to something interesting, the researcher gets bored and goes on to some other topic. I call this "gee-whiz" research. "Gee whiz," says the researcher, "will you look at that! I wonder what else I can find that's even more fascinating." Its sort of like channel-surfing on satellite TV. I found a lot of that in neurology texts.

Gary Cziko (970101.0313) --

RE: variable mutation rates:

>Dawkins suggests that this could be the case for asexually reproducing
>organisms but not for sexually reproducing ones. He argues that for
>sexual organisms specific genes could not "learn" to increase their
>mutation rate in response to certain environmental conditions since sexual
>reshuffling would separate the mutator gene from the structural one. But
>I don't think his argument holds against a general increase in mutation
>rate throughout the genome of the type I think Bill is arguing for. See
>Dawkins pp. 82 ff.

Why should there be a "mutator gene?" If you think of all the genes as a _system_, it would be the organization of the whole system that produces these and other effects, just as it is the organization of the whole brain,

not a single neuron, that produces behavioral effects. I think that attempts to find one-to-one correspondences between genes and large organizational effects are terribly naive. It's like removing a transistor from a boom-box, and when the music stops announcing that you've found the music transistor.

After all, we know that genes are just an organism's way of making another organism, don't we?

Best,

Bill P.

Date: Wed, 1 Jan 1997 06:29:53 -0700
From: Bill Powers <powers_w@FRONTIER.NET>
Subject: Re: Re.: TrkBlnd1.pas

[From Bill Powers (970101.0500 MST)]

>From: David Goldstein
>Subject: Powers 961229.1945 MST
>Date: 12/31/96
>
>When one looks at the design of this study, it sure looks like the
>standard way of doing research I was taught: There is an independent
>variable, sighted versus blinded. Each subject experiences all values
>of the variable ("within-S variable"). There is a second independent
>variable, replications. There are two different dependent variables,
>mean and standard deviation of performance in the tracking task.
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>One could apply the analysis of variance to determine whether the mean
>difference between the sighted versus blinded condition was more than
>one would expect by chance. One could estimate the magnitude of the
>effect.
>
>This sure looks very traditional in terms of research design. I must be
>missing something. Can someone point out what I am missing here?

If you were to do that kind of study, you could probably publish it. Nobody would insist that you present the model you're testing or show how this result informs the process of choosing between two models. People can maintain a cursor closer to the target when the cursor is visible than when it is invisible. So what? That's just another statistical fact to be left in the archives for someone else to explain. It would take its place along with the fact that mothers tend to hold their babies on the left, and other such startling facts that still hold the attention of the scientific world to this day.

This result, however, would be different from the usual ones in the archives in one respect. The magnitude of the effect is probably five times the standard deviation of the blind/sighted tracking errors. The normal publishable difference, to achieve $p < 0.05$, is about two standard deviations. An effect that is five times the standard deviation has a probability of occurring by chance of about 6×10^{-7}. Considering that in 50 replications or so I got (what seems by visual inspection) about the same magnitude of effect each time, the probability that all 50 results occurred by chance is even more microscopic. It would seem that this fact is qualitatively different from the kind of fact established under the general

requirement that p < 0.05 (or even 0.001).

A couple of years ago I made the modest suggestion that experimental "facts" in psychology would tend to be more believable if we simply raised the requirement that a publishable effect be larger than 2 standard deviations to the only slightly more stringent requirement that it be larger than 4 to 6 standard deviations of the variables. If we picked 5 as the new number, we would have, as noted above, p < 0.0000006 as the minimum requirement.

This would have a number of salutory effects. First, vast forests would be preserved instead of being cut down to be used in publishing papers that are never cited by anyone but the authors. Second, vague and imprecise "findings" would be replaced by robust facts that can be verified by replication of the experiments. And third, it would be possible to use such facts in scientific discourses where reasoning depends on six or seven facts being true at the same time -- and perhaps even more. If the probability of truth of a fact is 0.95, then a conclusion that depends on seven such facts being true at once has a probability of 0.7 of being true: it would be incorrect 30% of the time. It's hard to create a science when your conclusions are false almost one time in three. On the other hand, if your facts have a 0.9999994 probability of being true, you can string together an argument that uses 1000 such facts and your conclusion will still have a probability of truth of 0.999. On that kind of fact you can build a REAL science.

When the amplitude of the signal is only 5 times the noise level, we are getting measurements that are at the lower end of the range considered useful in the physical sciences and engineering. Yet if psychology were to raise its standards only to that level, it would be transformed.

For some odd reason, however, I have yet to meet a life scientist who is willing to submit to even the modest raising of standards that I propose. The general response is, "But then I would never be able to publish anything!" By superhuman self-control, I am usually able to avoid saying "Splendid!"

In the control-system experiments that the handful of PCT researchers have been able to do (including yours and Dick Robertson's with self-concept control), we routinely get signals that are 10 times the noise level. The table I use from the Handbook of Chemistry and Physics stops at 7 standard deviations, where we find that p < 2.6*10^-10. So the facts that we are finding, although simple, are as certain as most facts of physics. We could reason every day from sets of 10 such facts and be mistaken in our conclusions only once in 1,000 70-year lifetimes. That's what it takes to build a real science.

You've sat through this lecture before, David, and so have others on the net. I'm still waiting for someone to take it seriously. Anyway, there are people on the net now who haven't heard it before, so maybe I'll find an ally yet.

Best,

Bill P.

End of CSGNET Digest - 31 Dec 1996 to 1 Jan 1997
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