Date: Wed Apr 01, 1992 4:17 am PST
Subject: Time out for chainsawing

From Greg Williams (920401)

>Bill Powers (920331)

Thanks for the compliment, but I hope you don't say it too loud -- somebody might want me to do yet another project! One thing I seem to be particularly good (but not unique) at is "influencing" you to reply with multi-kilobyte posts... I wish I weren't quite that good at that!!

Record cold here is April-fooling me: I've got to cut wood again! So a detailed reply on the cockroach stuff will have to wait. In the meantime, I want to clarify a point so I can better reply when I'm able.

Suppose for the sake of argument that the cockroach does in fact get "away" from an air-puff via pre-calibration based solely on the initial pattern of hair movements. (I did find some quite amazing papers by Camhi yesterday at the University of Ky. Med. Ctr. library. Would you believe microanemometers, mini wind-generator, recordings from sensory hair cells, neural circuit mapping, and high-speed movies? I'm impressed with the level of methodological detail! More on this later. (J. COMPAR. PHYSIOL. ca. 1980)) Are you saying that the larger (evolutionary/learning) negative-feedback loop which then keeps the calibration appropriate is NOT control? I was trying to say (maybe in agreement with you and Marcos) that the overall "escape" "response," even with pre-calibrated actions (used as the "output function"), is a CLOSED LOOP. But (not in agreement with you (?)) then I want to call that CONTROL. It is negative feedback, and, in the case of learning, presumably has a reference level. It seems to meet your previously stated requirements for CONTROL, except that the feedback is NOT CONTINUOUS. Please say more about your labelling of such cases. Thanks!

Regarding possible conflict at higher levels due to lower-order chain-type mechanisms, I still don't think there is necessarily a problem. Chris Malcolm's S-R automata either satisfy or don't satisfy his desires for them; if they don't, he makes changes with, I think, no conflict. Surely he has high-level reference signals for seeing the automata work as he desires... surely he is controlling for seeing them work the way he desires... but (as when one throws a projectile) the feedback is NOT available continuously. All I think I'm trying to say is that you can have PCT-type (negative-feedback hierarchical perceptual) control with intermittent feedback. What's wrong with sampled data in control?

Greg

Date: Thu Apr 02, 1992 3:42 am PST
Subject: CLOSED(OPEN) LOOP

From Greg Williams (920402)

A (slight, no doubt) lull in the discussion on chains and loops. I've got a minute to suggest the direction I'm headed. Toward a theory of how chains can become evolutionarily stabilized in organisms, notwithstanding PCT ideas about how they can't produce consistent ends in the face of disturbances (for very long). Think about a hierarchy NOT exactly like Bill's, but where high-level re-calibration loops adjust the precalibration of low-level chains on the basis of outcomes of particular acts. Bateson discusses this sort of thing in MIND AND NATURE (continuous control of aiming a rifle each time vs. learning over a series of trials to pre-calibrate the efferents for "pointing and shooting" a shotgun in bird-hunting). All of this certainly needs a lot more fleshing out, for which I have no time now. The goal is to model discrete-trial (non-continuous) closed-loop re-calibration of chains (the OUTPUT signals are calibrated to satisfy the INPUT goals of the high-level discontinuous-in-time loops). If there ARE any chains which ARE subject to disturbance, this is how I think they can come to be and continue to be. What I've been trying to say in recent posts is that the chains can be part of overall (discontinuous) CONTROL loops, so that it isn't necessary to argue that chains don't exist if you think it is basically all control.

Item 2: Recently, speaking of birds particularly, but by implication about organisms in general, Bill Powers commented that if control was generally poor or nonexistent (chains? -- but see above for the beginning of an argument that chains can be kept accurate via control), we'd see dead bodies all over the place. Well, Bill, don't glorify the perfection of control of bugs. Says May Berenbaum in the Spring '92 AMERICAN ENTOMOLOGIST:

A pair of flies beginning operations in April, might be progenitors, if all were to live, of 191,010,000,000,000,000,000 flies by August. Allowing one-eighth of a cubic inch to a fly, this number would cover the earth 47 feet deep.

Thank goodness the predators, parasites, and disease organisms have better control than the flies! Good thing the dead bodies are quite small!!

Greg

Date: Thu Apr 02, 1992 2:52 pm PST
Subject: Environment; Degrees of Perception

[from Gary Cziko 920402.1600]

During this brief lull in net activity (not surprising with Rick Marken and Martin Taylor on the road and Greg Wiliams cutting wood), I thought I would bring up two topics from the recent S-R vs. control discussions about which I would appreciate some further clarification.

1. Both Bill Powers and Rick Marken have recently mentioned that when one incorrectly sees control as an S-R phenomenon, what one is seeing are due to properties of the environment and not of the organism. I'm pretty sure I don't understand the reasoning behind this. Help would be appreciated.

2. Bill Powers (920331.1030) in reply to Oded Maler (920331) said:
>When dt is
>on the order of normal neural lags, the behavior predicted by a continuous
>model is already almost exactly like the real behavior; letting dt shrink
>to zero then makes no perceptible difference in the behavior of the model.
>
>These considerations hold true in my model up to about level 6, control of
>relationships. At higher levels, however, discrete variables -- symbols --

>come into being, and the kinds of behavior that occur are more like the
>discrete or sequential kind -- indeed, level 8 is defined as the sequence
>level. Now the S-R interpretation becomes more feasible and we have to look
>elsewhere than basic mathematics to discriminate SR predictions from
>closed-loop predictions.

I find this confusing since I had thought that EVERYWHERE in the control hierarchy perceptions were represented by continuous variables. Bill has often argued that there are DEGREES of all perceptions, including lions and grandmothers. I realize that perceptions at the level of categorizations and above are somewhat "lumpy" or they wouldn't be categories, but doesn't the model still permit (indeed require) continuous variation within the limits of the discrete variables?--Gary

Date: Fri Apr 03, 1992 7:29 am PST
Subject: Miscellaneous answers

[From Bill Powers (920403.0700)]

Avery Andrews (920331) --

"Condition maintenance" is OK, but control systems also do "condition establishment" (from scratch) and "condition variation" (on purpose). When you set the thermostat to 68 degrees you give a thermostat a new reference temperature. The first thing it does is turn the furnace off or on (depending on the previous condition) until the temperature comes to 68 degrees. Then it varies its on-off duty cycles to maintain the temperature at the new level. When you changed its setting, you were deciding to establish a new condition using this lower-level control system. So at that moment you were a condition-altering control system.

Words like "condition maintenance" and so on are useful in explaining specific cases of what control systems do, but before a person can really grasp this subject, the central concept of control has to be understood. The idea of VARYING actions in order to CONTROL outcomes is alien. The longer I bump up against conventional ideas of behavior, the clearer it seems to me that the concept of control may as well have come from Alpha Centauri. Whenever people talk about open loop control, they're tacitly assuming a real controller in the background, one that monitors the outcome and adjusts the system or process on the basis of the observed outcome. When they see a controlled outcome and don't understand control, they try to patch over the hole where the control system is by just assuming that "somehow" the right outcome is produced. They often imagine outlandish and impossible mechanisms for achieving the right effect, and are very careless about their explanations because, after all, the effect did occur, didn't it? For the most part explaining control without actual control requires a total lack of physical intuition -- the "somehow" conceals an impossibility in terms of physical processes, unless true control is involved.

So I'm returning to my former position, which is that control theory is about control and control is about control theory. To compromise on this principle is to leave the way open to imagine that the observed effects can come about through some other process that just happens to stabilize outcomes in preferred conditions. The basic problem control theorists face is that others imagine they are explaining observations, but are either skipping over great conceptual gaps or simply aren't observing what's in front of them. The word for what goes in in behavior is control. If we give up (a) using the word control, and (b) insisting that this is a fundamental process, we might as well give up, period.

Martin Taylor (920331.1700)

> ... Bill's argument that the cockroach behaves so as to reduce the >wind speed can hold only for the initial turn and acceleration. After >that, the approaching hand or foot will be causing an ever increasing >wind speed for which the cockroach cannot compensate.

Martin, this kind of argument discourages me. It's basically a qualitative assessment of a quantitative situation. The "initial turn" and "initial acceleration" are artifacts of event-perception and classification. According to Randy Beer, the cockroach completes its turn in about 60 milliseconds (which itself is a categorical statement that's probably true only under maximum-stimulus conditions). It is probably running at the same time, although to see its actual path would require very close observation, the sort of thing that's never mentioned in mnemonic anecdotes. The cockroach, under maximum-threat conditions, can accelerate very quickly to speeds over 30 miles per hour (running on its rear legs), which is 44 feet per second, or one foot in a little under 25 milliseconds. Of course it has to accelerate: if it can accelerate at 1 g (sticky feet or bumpy surface on the right size scale) it will have gone 1 foot in the first quarter of a second and will be travelling 2 feet per second.

>The biggest wind blast will occur when the hand hits the floor, just
>missing the scuttling cockroach. I don't see where the control system
>can control in any way better than the ballistic control of a golf >ball, in this case.

Control systems don't just "control" or "not control." This is a black-and-white picture of a continuous process. Control systems are organized in such a way that their actions bring perceptions strongly toward a reference condition. For very slow disturbances, the difference is immediately corrected and an observer would not see any effect from the disturbance. For larger and faster disturbances, the momentary error becomes larger. The largest and fastest disturbances cause the largest amounts of error, which of course result in the largest efforts to reduce it because action is driven by error. If the disturbance is within the capacities of the system's output limitations, the error will be restored to zero even in the extreme case, although a perceptible amount of time may be required to do so.

When disturbances are larger than the capacities of the system to oppose their effects, then error simply occurs. The control system doesn't turn into some other kind of system just because it's lost control. It continues to produce maximum effort until either it dies, is turned off by some other system, or regains control as the disturbance wanes.

If you get lucky and your hand comes somewhere near the cockroach, the bug will probably have turned and be running by the time your gigantic appendage has finally descended to the floor and mashed into it like a pulsating blob of slow-motion jello. The final surge of air might overtake the cockroach from behind and momentarily exceed its capacity to reduce the relative air velocity to zero by running faster. But in a few milliseconds (seconds on the cockroach's time-scale) the surge of air will be past and the flow will decrease again, to the range where control is again possible. As Marcos Rodrigues pointed out, the cockroach may actually keep running at top speed for some time, because there has been a giant overload of error signal and the error signal probably takes some time to fall back toward its normal state of nearly zero. All during this overload time, the speed reference signal being sent to the locomotive output machinery is at saturation, falling only when the error signal comes back within the normal control range.

When you say you don't see how the cockroach controls any better than a ballistic golf-ball would do, I believe you. But no amount of qualitative analysis will help you see.

Greg Williams (920401 or so) --

Sampled control is certainly possible and likely. You're proposing that a higher-level sampling control system adjusts the parameters of a stimulus-response system so that the outcome of the SR process suits the reference level of the higher control system. This is control by variation of parameters, a mode we've talked about but haven't done much modeling about.

Evolution can be the higher control system, particularly if active negative feedback control is involved in evolution, as increasingly seems to be the case. So there's nothing wrong IN PRINCIPLE with what you propose -- we can't rule it out on grounds of self-contradiction or that sort of thing.

The basic question is not whether the scenario you propose CAN occur; it's whether it DOES occur. Absent a complete investigation of every proposed SR connection (which has obviously never been done), we can argue only on grounds of likelihood. I consider the arrangement you propose not as impossible, but as highly unlikely.

Consider the siphon-withdrawal response of Aplysia. It's been observed that a touch to the rear of the body "causes" withdrawal; the connections from sensory nerves to muscles have been traced. Is this an SR system? Or is it just a primitive control system with a crude sensor and a crude on-off effector? To support the idea that it's an SR system, you've have to ignore the average effect of the withdrawal on the average stimulus situation, or say that this effect is just a side-effect of no importance, beneficial to the organism only in the sense that presence of this reflex promotes overall survival.

But that would be saying more than we know. A biologist who sees everything in SR terms will see the same nervous system that the control theorist sees. Both would expect to be able to trace a path from the stimulus input to the motor output. But what these observers see in the network between input and output will be very different. If there are signals from elsewhere in the net that enter this input-output path, the SR observer will dismiss them -- they're just routes by which other stimuli can affect the motor output. If those other signals enter the motor neurons, the "alternate response" idea will seem even more obvious. But in either case, the control theorist would see reference signals that bias the response to the original stimulus, changing the effective zero point. And the control theorist would note that the response affects the stimuli over the long run. The external feedback path would be there. Even if the stimulus doesn't come directly from the immediate effects of the motor response, it could still be part of a derived controlled variable. The SR observer wouldn't even think of looking for such a thing in the neural net.

A true SR effect would be one in which there is no feedback from the response that alters the stimulus situation. A true evolutionary SR effect would be one that institutes a true SR effect with the only feedback occurring species-wise, through natural selection. And error-correction in the evolutionary SR case would be so slow that it could benefit no single organism.

I dond't want to argue against evolutionary control or sampled control or control by variation of parameters. Where I hold out against your thesis is at the level of the SR process itself. I think evolution selects for control systems, not SR systems. It wouldn't bother me a lot to find exceptions -- only a SIGNIFICANT NUMBER of exceptions. I refuse to believe analyses of SR behaviors done by people who don't know of any other kind of connection. Their reports seem to verify an SR connection because they haven't noticed any of the things that would indicate a closed-loop connection. To them, an SR connection seems plausible in the light of their model. Their explanations seem adequate because they don't realize that control is going on even when they're staring directly at it. They think they are seeing all the evidence, but they're not.

So I will not accept any observation of an SR connection just on the basis that an SR believer has reported it. The report, until a real investigation shows otherwise, is probably inadequate and biased by ignorance.

One instance I will accept is the praying mantis' striking movement. This occurs at the maximum possible speed and can't be controlled while in progress. But I won't accept that this striking movement is caused by an image in the mantis' eyes: that is the SR assumption, which is unwarranted. My guess would be that the striking movement is the action of a control system (a sampled control system), and is centrally, not peripherally, initiated. It is aimed on the basis of a relationship between kinesthetic control systems and visual control systems, gradually adjusted over many strikes. That's what I'd be looking for in a trace of the mantis' nervous system. The mantis' strikes themselves are not purposive -- no action is purposive -- but the outcomes of those strikes are under the control of a slow purposive sampling control system.

Gary Cziko (920402) --

I stated that SR measurements of control behavior measure only properties of the environment. This isn't so hard to figure out.

Suppose you have a control system controlling an input, and there is a disturbance that influences the input and also an effect of action on the input. If control is excellent, the input thus affected will be stabilized, and will not change.

If the disturbance now changes, the action will change, producing the usual illusion of a simple cause-effect relationship.

What determines how the action will change? Suppose that as the disturbing variable goes through its range, its potential effect on the input variable is linear. And suppose that the effect of the output variable on the input variable is nonlinear -- suppose the effect goes as the square of the amount of output.

We now have the relationship

$$d + o^2 = \text{constant}.$$

Suppose the reference level is zero, so the constant is zero. If 4 units of disturbance are applied to the controlled variable, it will take 4 units of opposition to cancel this effect and keep the controlled variable at zero. But to produce 4 units of opposition, we need only 2 units of output, because the effect of the output goes as the square of the amount of output. In general, a disturbing variable of magnitude d will produce d units of effect on the variable, which can be cancelled by sqrt(d) units of output. So the observed relationship between disturbance magnitude and output magnitude will be o = sqrt(d). This will hold, of course, only for negative disturbances that create positive outputs (negative outputs are forbidden because they'd result in positive feedback in this case).

This square-root relationship is a function of the part of the environment lying between the output variable and the controlled variable: n units of input to this environmental feedback function produces n^2 units of effect on the controlled variable. And we've assumed that between the disturbing variable and the controlled variable, there's a linear connection so 1 unit of disturbing variable produces (when acting alone) 1 unit of effect on the controlled variable.

The relationship between output and disturbance is therefore totally determined by these two environmental paths: one from output to controlled variable, which squares the effect, and one from disturbance to controlled variable, which transmits the effect linearly. The result we see is reducible to the properties of these two environmental paths, given only that the behaving system is capable of controlling the controlled variable by varying its output.

The simplest case is when the output has n times as much effect on the controlled variable as the disturbance has. Then the output will be 1/n of the disturbance magnitude (opposed).

It's no accident that with a feedback function of o^2, we observe that the output depends on the disturbance as the square ROOT of the disturbance. In general, if the disturbance has a linear effect and the output has an effect f(o), the observed relationship will be
o = inverse f(d). The observed relationship will be the inverse of the feedback function (for those functions that allow control to continue). If the feedback effect is the integral of the output, the output will be the first derivative of the disturbance. These principles are the basis of electronic analog computing.

If the disturbance affects the input nonlinearly, and the feedback effect contains the SAME nonlinearity, then the output will depend linearly on the disturbance. I leave the proof to the student. In a neuron, if the output feeds back negatively to inhibit the same neuron (strongly, of course), and if the fed-back inhibition has the same nonlinearity as the effects of an excitatory input signal, then the output current (frequency) will be linearly related to the excitatory input current. The internal nonlinearities of the neuron will cancel out. The "disturbance," of course, is the input signal, and the controlled variable is the state of activation of the neuron. So neurons, despite their nonlinearities, can make very nice operational amplifiers for analog computation.

This is the sort of relationship you'd NEVER discover by tracing individual impulses.

Going off with Mary for a drive to the Bisti Badlands and Canyon du Chelly. See you all Saturday.

Best

Bill P.

Date: Fri Apr 03, 1992 10:12 am PST
Subject: Re: Miscellaneous answers

[Martin Taylor 920403 12:30]
(Bill Powers 920403.0700)

Leaving in a couple of hours, but time for a quick response to Bill critique of my comment that the cockroach's control would be of the sampled. ballistic correction kind.

Bill is quite right in his criticism of my ill thought-out statement:
>> ... Bill's argument that the cockroach behaves so as to reduce the >wind
>>speed can hold only for the initial turn and acceleration. After >that,
>>the approaching hand or foot will be causing an ever increasing >wind speed
>>for which the cockroach cannot compensate.

I thought of why this is wrong some hours after posting it. The argument isn't exactly the one Bill used in his critique, but the following (which is qualitative). I assumed that the cockroach would be controlling for minimum wind velocity, and equated this with an ABILITY to reduce the wind velocity. But the ability is irrelevant. What the cockroach can control is the direction or the wind, and if it is indeed controlling for minimum wind velocity in an ever-increasing blast, it will head away from the centre of the blast at a maximum rate. So, yes, it can be controlling all the way, and it needs no quantitative assessment to see that (even though a quantitative
analysis is always preferable to a qualitative one, as Bill rightly says.)

In reply to Avery Andrews 920331, Bill says:

> Whenever people talk about open loop control, they're tacitly
>assuming a real controller in the background, one that monitors the outcome
>and adjusts the system or process on the basis of the observed outcome.

Yes, I had also come to that conclusion in respect of dialogue, and have made the same statement in a paper I am currently drafting on the integration of voice in a complex interface. In this paper, for the first time, I am explicitly tying PCT together with Layered Protocols. A graduate student in

HCI who saw a draft a few days ago made a written comment : "This stuff is really neat!" I've also had it said that the recent papers I have been drafting
with the same slant but not such explicit reference to Powers have made Layered Protocols much easier to understand than the earlier ones, even though my own view of the theory has not changed except in detail, so far as I can see. So maybe there is hope yet?

See you in about 10 days, for a couple of weeks.

Martin

Date: Fri Apr 03, 1992 10:22 am PST
Subject: Re: Environment; Degrees of Perception

[Martin Taylor 920403 12:40]
(Gary Cziko 920402.1600)

>During this brief lull in net activity (not surprising with Rick Marken and
>Martin Taylor on the road and Greg Wiliams cutting wood), I thought I would
>bring up two topics from the recent S-R vs. control discussions about which
>I would appreciate some further clarification.

Sorry, you haven't got rid of me quite yet--two more hours.

> I realize that perceptions at the level of categorizations
>and above are somewhat "lumpy" or they wouldn't be categories, but doesn't
>the model still permit (indeed require) continuous variation within the
>limits of the discrete variables?

Quite apart from PCT, in my "chaos" analysis of cognition, I informally denote that continuous variation as "adjectival" and the variation across categories as "nominal." Geometrically, one can consider categories developing
from cusp catastrophes. Below the cusp, there is only a continuous variation that can be described only in adjectival terms with "more" or "less" or by mapping agains some number (as Bill does with "lionness"--not "lioness"). Above the cusp, there is a break at a hysteric category boundary, but still there are gradations within the category, of exactly the same kind as there are below the cusp where the category does not exist.

In PCT terms, movement from above to below the cusp is controlled by a reference
to see that a category exists, or not. Before learning about PCT, we called it "contextual stress" or "situational task demand" or something like that. But it is learning that allows the catastrophe cusp to develop, so one can see the same picture in terms of the range of possible control with respect to the reference (i.e. how far the reference can move and still have the percept track it). Some categories are hard NOT to see, some are hard to see, and some you can see or not see, more or less at will.

Think of three sticks lying on the ground in the form of an H, or again in the form of an A, or like an H but with the tops tipped inward. If you are an illiterate seeing this, your percept is likely to be "three sticks tipped inward a little (or a lot, nearly touching)", but if you are literate and the three sticks have on one side other sticks forming "C" and "T", you will see "A". If you are literate and there is no reason to anticipate a deliberate arrangement of the sticks, you can see "three sticks" or "A", as you choose.

Martin

Date: Fri Apr 03, 1992 3:52 pm PST
Subject: Mail Troubles

[from Gary Cziko 920403.1700]

The relative lull continues. So let me take advantage of it for a brief technical note.

If your e-mail connection is disconnected for more than a few days, I may remove you from the CSGnet list. This is because I am privileged to receive copies of all CSGnet messages that get bounced back as undeliverable. This can become quite a lot when more than a few people are unreachable and traffic is heavy.

Therefore, if after getting back on line you find you are not receiving CSGnet messages, just send me a direct message and I will be glad to put you back on the network.--Gary

Date: Fri Apr 03, 1992 8:29 pm PST
Subject: Misc remarks; Anasazi

[From Bill Powers (920403.2000)]

Martin Taylor (920403) --

You probably won't see this until you get back, but I just wanted to thank you for being the way you are. You went away and thought over your post and found your error and said you found it. I appreciate that attitude more than a little.

As to your corrected analysis, you semi-apologized for its "qualitative" nature. Actually I accept it as being a quantitative analysis even if no numbers were involved. The difference between qualitative and quantitative explanations, as I see it, is that qualitative explanations make no room for the variables of nature to exist in a continuum: they simply exist or occur, or they do not. The antecedent results in the consequent, or it doesn't.

As soon as you began talking about controlling for minimum wind velocity, about controlling for direction of the (relative) wind, you saw the error for yourself. Starting with that point of view, you can (if there's a reason) go on to instrument this way of looking at the situation and come up with quantitative measurements that can be plugged into a quantitative theory. If you just say there was a "response" to a "threat," quantitative measurement goes out the window, even as a future possibility.

I'm much more concerned with maintaining a quantitative attitude than with modeling every last little thing. I think this attitude leads to an ability to make correct general predictions about behavior even before you actually test a model against real behavior and refine the general quantitative relationships you imagine.

I am very pleased to hear about your use of PCT with Layered Protocols. I could see from the early papers you sent me that you had already traveled far down the road toward the CT point of view. When you have something written on the next generation of the LP system, I'd like to see it.

I am also eager to see how your concept of the categorization process works out. Gary Cziko has asked me about continuous control at levels higher than relationships, but I don't have a good answer for him. I can see that there are continuous aspects to perceptions at essentially every level -- some sort of quantification or "adjectival" perception always seems possible. At the same time, however, we do have to deal with logical, symbolic, discrete rule-driven processes which appear completely digital in nature. I don't feel that I could get all those conflicting requirements into a single model. I'm still confident of the general control-of-perception principle, but at the higher levels I don't think it can be taken with completely literalness -- it's more that this principle is the overall EFFECT of what goes on. I think it's OK to talk about making perceived system concepts match reference system concepts and so on, but I think we have to stay at arm's length from suggestions as to how, in detail, this net result is accomplished.

Maybe your "chaos" analysis will give us some hints about having it both ways: continuous for purposes of modeling control, discrete for purposes of reasoning. Or something. I'm just blathering.

For modeling perceptual functions, by the way, there's a requirement that people like Freeman don't seem to consider in their "chaos" models. The requirement is that the state of a perceptual function has to be knowable by other parts of the brain. A logical level, for example, has to be able to know that a given experience goes in category A rather than category B. It isn't enough that the categoric perceptual function be in some unique state corresponding to each category. There has to be a way for the category level to tell a higher level which state is present (this applies, of course, at other levels, too). This is why I don't like Freeman's model in which the olfactory system falls into various basins. Who knows that it's in one basin or another? How does the fact of its synchronized oscillations turn into something usable by other functions in the brain? Freeman seems to be satisfied that HE knows what state the system is in. A fat lot of good that does for the rest of the system.

This is pretty stupid because you're going to be gone for ten days before you see this. But if I don't comment now I'll lose the thread: too many things going on in my sedentary retired life.

One thing leads to another: adding the second kinesthetic level to the arm model has led me to rethink how the parameters of the first level are adjusted, and something astonishing has come to light. It seems that by controlling acceleration, the basis is created for a three-tier control system (there are three, not two, in the spinal systems, cleverly collapsed to look like two!) that can behave stably in the presence of a wide variety of loads, without any of the traditional means of stabilization. I have to check this out thoroughly; will report on it in a few days. I've known for

weeks that something funny was going on, especially when with some combinations of parameters the model didn't seem to care how much damping there was. I thought it was a glitch in the program, but it wasn't.

Canyon du Chelly was fascinating, although it turned out to be Chaco Canyon (where we actually went). The Anasazi built communal dwellings all over this canyon area, the largest having some 800 rooms. I love looking at those walls and thinking, "Yeah, I see what you did." It struck me that in the absence of any written record, artifacts were the only time-binding medium that would last across ten or twenty generations to allow the later people to experience exactly what their ancestors "said." I got the feeling that failure to invent written language must have had a terribly crippling effect. The greatest ideas anyone had, if they weren't expressed in tools or utensils or stone walls, simply disappeared, like the Anasazi themselves. We walk through those stone dwellings and find nothing to tell us what happened, or whether the people were afraid to leave, or glad, or what. There's nothing.

Best to all, and hooray for the written word,

Bill P.

Date: Sat Apr 04, 1992 12:47 pm PST
Subject: Belgiums and HPCT

To: CSGnet members
From: Dick Robertson through David Goldstein
Subject: Belgians and PCT
Date: 04/04/92

(This is part of a post I received from Dick Robertson who is in Belgium now teaching about HPCT.)

Hi,

You might also put a request for help on the net for me , requesting anyone who knows of a praticum or internships for two Belgian students who are interested in stress management training and health psychology. These guys have shown a real interest in PCT. Ed Ford 's industrial consulting would be the kind of thing the one is interested in. They were also interested in your paper on the self image which I brought along with me.

Thanks , David
Dick.

Date: Sat Apr 04, 1992 3:17 pm PST
Subject: Sociology and Sanity

[From Rick Marken (920404)]

Well, I'm back -- temporarily. I don't have time to say much (that should be a relief) but I would like to express my thanks to Clark McPhail and Chuck Tucker for a great time at the Midwest Sociological Association meeting. It was really fun and informative. Despite technical difficulties (the overhead projector failed) the talks went pretty well. I had to improvise so much I think I left out the best parts. But Tom Bourbon gave a great talk and demo (on cooperation) and Kent McClelland saved the day by clearly explaining how the crazy rantings of a couple of left wing psychologists might be of interest to people studying interactions between people. One or two people seemed to be seduced by the sermons of the PCT cult. I was surprised to find that Tom is as far over the edge as I am; we were talking to a nice young psychologist who was interested in PCT and Tom would not admit that there might be anything of value in the current psych research literature. I, of course, agree but I wouldn't say it in person to a poor guy who has got to make a living. Well, ok, maybe I would; but I wouldn't like myself for in the morning.

I liked Bill's clear explanation of why s-r laws reflect characteristics of the environment. As I said, I didn't really understand it fully until recently (Bill gives a good, but less intuitive, description of it in terms of "real" data in the "rat experiment" chapter of Living Control Systems p. 47; it's must reading, and re-reading). As I said before, this fact (that s-r laws are environmental laws when there is negative feedback from response to input) just kills it for all of the social and much of the life sciences. I don't know if anything comparable to the kind of revolution implied by this observation has ever happened to a science (short of the development of science itself). Relativity, Copernicus, genes, evolution -- none have had the 'dire' implications for conventional science that control theory has for the social and life sciences. None said "all the data that you have collected up to this point has nothing to do with the phenomenon that you thought you were studying". If control theory is right, then the science of psychology hasn't even started yet. Try telling that to a bunch of scientific psychologists.

Hasta next week,

Rick

Date: Mon Apr 06, 1992 7:02 am PST
Subject: FROM MIDWEST MEETINGS

A Plenary Session at the Midwest Sociological Society-1992
"Individual and Society: An Alternative Perspective"<^*^>
<<<Organizer and Presider>>>:

Clark McPhail
University of Illinois - Urbana-Champaign

<<The Discussant's Comments>>

by

Charles W. Tucker
University of South Carolina at Columbia

I view what was presented here this afternoon NOT as a deviation from the development of an approach, perspective, theory or model for understanding human group life and providing some solutions to our problems BUT as a continuation and, a great leap forward, over the current situation in the social and behavioral sciences. especially sociology.

I am convinced that sociology is floundering or stagnating and may even be on its death bed. I will not try to marshall any evidence to support my case (although I could) but will mention a few items that come to mind. There were, several years ago, a few pieces on "sociology in the doldrums." There were articles by Blalock and Lenski, one saying that sociologists have failed empirically and the other saying that we have failed theoretically, but neither proposed a decent alternative.

Randall Collins' article on "proscience or antiscience" and the answers to it, left me with a sense of uncertainty about the choice. A recent example is much more helpful. Joel Smith calls for a new methodology for the 21<^st^> century. He calls for a return to the systematic examination of human group life while expunging ourselves of such childish squabbles as quantitative versus qualitative research, macro versus micro studies, ethnomethodology versus structural functionalism. I applaud this call.

But if we are serious about comprehending human group life and telling ourselves and others what we know about it then, we had better find a way to get on with it, very soon.

Of course, there is no good reason to believe that all but a few will take up this challenge. But let me remind all of you that this call for a serious reconstruction of sociology has gone out before and what was said today offers a refinement of the message and I believe it would serve us well to carry it out.

It was William James that proposed that we examine human behavior as purposeful and qoal directed; it was John Dewey that, in his famous article on the reflex arc, who provided that basic critique of what came to be know as S-R behaviorism and offered an opposing conceptualization. Dewey's view was carried on by the early Chicago School of Pragmatism.

But most sociologists, notably - Hayes, Bernard, Bain and Lundberg - rejected the pragmatic message and did all that they could to destroy it with their version of Watsonian (later Skinnerian) behaviorism. Their characterization of the phenomena and approach for making sense of and presenting information about social life still dominates the social and behavioral sciences and its also dominates our "common sense ways" of dealing with one another. It has yet to be recognized that the use of the behaviorists' theory and methods has resulted in very little knowledge about human group life and even less about the individual human being.

Along the way sociologists have ignored or discarded almost every pioneer sociologist who could possibly assist in the understanding of social phenomena. We read out the psychologists, even those in sociology like Lester F. Ward and Charles Ellwood. We have ignored the social workers who might be interested is solving problems in the community; the psychiatrists who might have something to say about madness and many others. Sociology has been, in my view, engaged in a purging process of the highest order. And it is still going on in our graduate schools and on our editorial boards.

Yet some have insisted on the basic notion that we can not understand society without both selves and science. Herbert Blumer once stated this rhetorical statement: "The question remains whether human society or social action can be successfully analyzed by schemes which refuse to recognize human beings as they are, namely, as persons constructing individual and collective action through an interpretation of the situations which confront them. (1962:192)."

I construe this to say that neither human society or social action can be sucessfully analyzed by sociological schemes that ignore the fact that human beings are purposive actors.

Although many, if not most, see Garfinkel's ethnomethodology as discarding science, it is, in my judgment, a precise and carefully documented critique of the severe shortcomings, flaws and downright sloopiness of the research done in sociology. Garfinkel and his colleagues challanged sociologists to take science seriously. Most sociologists, in turn, treated Garfinkel as a deviant or at best, as a gadfly worth only of token toleration.

Yet, in spite of their lack of acceptance even today, the progeny of James, Dewey, Mead, Kuhn, Blumer, and Garfinkel, among others, keep insisting on notion that social life cannot be understood, except superficially and trivially, without using both self and science.

What was presented today is consistent with that belief. Perceptual Control Theory conceptualizes the self much as Mead did but with more more elaboration and precision. Briefly stated, the self is self-indications, as Blumer noted again and again. More precisely, self is characterized as a negative feedback process of circular causality.

One of the major implications of self-indicationing is, according to Blumer:

> Instead of the individual being surrounded by an
> environment of pre-existing objects which play upon him

> and call forth his behavior, the proper picture is that he constructs his objects on the basis of his on-going activity. . . . Whatever the action in which he is engaged, the human individual proceeds by pointing out to himself the divergent things which have to be taken into account in the course of his action. He has to note what he wants to do and how he is to do it; he has to point out to himself the various conditions which may be instrumental to his action and those which may obstruct his action; he has to take account of the demands, the expectations, the prohibitions, and the threats as they may arise in the situation in which he is acting. His action is built up step by step through a process of such self-indications. The human individual pieces together and guides his action by taking account of different things and interpreting their significance for his prospective action. There is no instance of conscious action of which this is not true. (1962:182).

There is not one idea noted in Blumer's statement that is inconsistent with what was stated today as Perceptual Control Theory.

Many of you are probably skeptical because the word 'control' was used throughout this presentation but if you mean by the word 'control' that the individual is manipulated by others against his/her wishes and that this theory proposes such practices. Do not fear! Nothing could be further from the major tenet of Perceptual Control Theory. William Powers, who developed this model rejects the notion of others controlling the individual, or vice versa when he notes:

> Control of behavior is not wrong or sinful or irrational or evil. It is simply inconsistent with the facts of human nature. If we become trapped into talking about control in terms of right and wrong, we miss the essential point completely. We start arguing over who will control the controllers, and so on, tacitly assuming that control is really possible in the first place. <<It is not possible>>. People cannot get inside each other's brains to operate the control systems there, and those control systems are what cause behavior. (271) . . . In order to avoid self-destruction, I think that all we need do is consider openly and very carefully the implications of this basic concept of human nature. That one concept, so antithetical in its implications to the ways in which people have always thought about each other and themselves, gives us a place to stand from which we can move the world. (1973:272).

I encourage all of you to consider anew this perspective on human group life.

REFERENCES

Blumer, Herbert. 1962. "Society as Symbolic Interaction" Pp. 179-192 in Arnold Rose (Ed.) <<Human Behavior and Social

Processes>>. New York: Houghton Mifflin.

Powers, William T. 1973. <<Behavior: The Control of Perception>>. Chicago: Aldine.

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*The session contained these presentations: Richard S. Marken "A Perceptual Control Theory Analysis of the Individual in Society"; Thomas Bourbon "A Perceptual Control Theory Analysis of Individuals in Cooperative Behavior"; Kent McClelland "Implications of Perceptual Control Theory for a Sociological Understanding of Individual and Society". I thank Clark McPhail for his suggestions on an earlier draft of these remarks.

Date: Mon Apr 06, 1992 1:54 pm PST
From: Dag Forssell / MCI ID: 474-2580

TO: csg (Ems)
EMS: INTERNET / MCI ID: 376-5414
MBX: CSG-L@VMD.CSO.UIUC.EDU
Subject: What you see
Message-Id: 72920406215427/0004742580NA3EM

[From Dag Forssell 920406]

Gary Cziko (920402.1600)

>1. Both Bill Powers and Rick Marken have recently mentioned that
>when one incorrectly sees control as an S-R phenomenon, what one
>is seeing are due to properties of the environment and not of the
>organism. I'm pretty sure I don't understand the reasoning behind
>this. Help would be appreciated.

Gary, a while back, I responded to your query about "tight coupling" without realizing that Bill had already answered. This time I realize that Bill answered in a full page analysis. Bill (920403.0700), but hope you will find this of interest.

Again, I think that our rubber bands provides an excellent illustration. Suppose you pull on your end of the bands to keep the knot over the dot. YOU control! I disturb the position of the knot by moving my end. I provide stimulus and you respond. My disturbance is a property of the environment from your point of view. (I can represent any kind of machine or natural effect, disturbing my end, I am NOT trying to control). So is the quality of the rubber band which converts your action (and my disturbance) into an influence on the knot.

As long as you do control, your action will be WHAT IT HAS TO BE to keep the knot over the dot. YOUR ACTION IS 100% DETERMINED BY the disturbance and the nature of the rubber band, all of which are PROPERTIES OF THE ENVIRONMENT.

The only requirement is that you do control somehow. The rubber band experiment illustrates the fact that you DO control. It tells you NOTHING about how you are organized inside to accomplish this control.

Therefore, what you see (your erratic movements) is due to properties of the environment and not of the organism (you).

This is most clear when you do the rubber band exercise slowly, allowing near perfect control. The knot stays steady over the dot and your actions are perfect mirror images of the disturbance.

Bill Powers (920403.0700)

>I refuse to believe analyses of SR behaviors done by people who
>don't know of any other kind of connection. Their reports seem to
>verify an SR connection because they haven't noticed any of the
>things that would indicate a closed-loop connection. To them, an
>SR connection seems plausible in the light of their model. Their
>explanations seem adequate because they don't realize that control
>is going on even when they're staring directly at it. They think
>they are seeing all the evidence, but they're not.

Through Clark McPhail's syllabus, my attention was drawn to pages 105-107 in Phil Runkel's book: Casting Nets and Testing Specimens. (NY, Praeger 1990). As with many other references, I had obtained it based on recommendations on this net, but had hardly read it yet.

Here is an excellent, detailed description of the rubber band experiment that is MORE INSTRUCTIVE than the way I was introduced to it. You invite a friend to experiment ON YOU. "You are the experimenter. Move your finger as you like. Watch what I do. When you can explain what is causing me to do what I do, let me know."

Phil spells out the typical suggestions of friends. I have confirmed this. Saturday, I had a group of six, who had no notion of PCT. I use an easel with the above instruction printed in the center. My rubber bands have a yellow ping pong ball over the knot, to make it visible at a distance. I kept the ping pong ball over one letter. All I got was that I was mirroring the experimenter. Of course the experimenter causes me to do what I do. I kept telling them that that was not the cause and challenging them to come up with a better explanation. No luck.

IT IS TRUE THAT PEOPLE CANNOT SEE CONTROL EVEN WHEN IT IS STARING THEM IN THE FACE.

Starting the experiment this way makes the paradigm shift stand out. You can point out that an absence of a point of view makes it impossible to see the phenomenon. Your ignorance makes you blind - literally!

Only AFTER this sequence, do I experiment on my friend by asking him/her to keep the knot over the dot.

Later, one can point out that the better the control, the less exciting the appearance. Good control is invisible, because nothing happens.

Dag

Command: Date: Mon Apr 06, 1992 4:02 pm PST
Subject: Closed, Open, Clopen?

From Greg Williams (920406)

Finally, I have a little time for replies to Bill.

>Bill Powers (920331.1030)]

>I think that if anyone seriously wanted to test the choice between an open-
>loop and a closed-loop escape response, it would not be hard to think of
>experiments that would settle the question. Any takers out there among the
>bug people?

I haven't had much time to go over the Camhi, et al. papers which I found at the U. of Ky., but it looks to me like they don't really address the question as they should have -- they didn't look at changes in escape actions when disturbances were applied. (Of course, they might have in papers which I didn't find, or somebody else might have. Who knows?) In the seminal paper's summary (J. COMPARATIVE PHYSIOLOGY A 128, 1978, 193-201), Camhi and Tom write (p. 193): "The turning response was properly oriented, and the leg movements properly directed, even when sampled during the first 0-16 ms of movement. This presumably was prior to the time when feedback from the animal's own turning movement would be available to influence the turn. Thus the turning behavior may be executed in an open loop manner." I don't see how the "thus" works here. On p. 199-200: "Our stop-frame [64 frames per second] analysis suggested that sensory feedback during the turn played at best a minimal role in determining the direction and magnitude of the turn. This is because both the direction of the leg movements and the direction of the the body's turn, as seen in the first cine frame of the insect's movement, depended upon wing angle. This first frame captures the insect's position, on different trials, at some moment between 0 and 16 ms (on average, 8 ms) after the onset of the turn. Within this time, sensory feedback is unlikely to have been generated, been centrally integrated, and begun to influence the insect's behavior. Therefore, the cockroach presumably does not have time to begin a turn, determine through sensory feedback whether the turn is in the wrong direction, and then correct any wrong turns." Footnote on p. 200: "The shortest known sensory-to-motor latency in the cockroach is that of trochanteral hair cells exciting leg motor neuron Ds. The latency from mechanical input to movement has not been directly measured... [but] is probably more than 10 ms and possibly much more." Rather infuriating -- here they have this neat set-up, and they don't apply any disturbances!

>What overhead?

If the outcome isn't critical, I still think it possible that open-loop circuitry could be less complex than closed-loop circuitry. And if "away" truly is all that matters much of the time, a chain mechanism that BLASTS the organism "away" at high speed could be preferred evolutionarily to a slower but more precise loop mechanism.

>If higher level control loops exist which modify lower-level open-loop

>responses, then control will exist at the higher level, but not at the
>lower level. This can work only if the inaccuracies and inappropriateness
>of the lower-level responses are unimportant.

Yes!

>When the outcome matters, control is continuous.

When the PRECISION of meeting a goal matters, control is continuous. The outcome can matter and the goal can be met with low precision: like "away."

>You don't drive a car at high speed on a mountain road by looking out the
>windshield and giving the wheel a twitch once per second or so. If you did
>that your control bandwidth would be far too low and you'd go off the road or
>run into those Falling Rocks.

On the other hand, if I "get used to" driving a sports car and then switch to a Mack truck, I feel very "busy" -- the (presumptive) model-based "adaptive" mechanisms in my control structure have to recalibrate. This is what Bar-Kana was arguing about months ago -- that there is a pre-calibrated component alongside the closed-loop component of control. And here it isn't so easy to disentangle "control" and "non-control."

>So when you imagine a direct connection between a stimulus and an outcome,
>you're implying far more complexity in the intervening processes than is
>obvious.

That's why Skinner had to have his (half-way) revolution.

>Bill Powers (920403.0700)

>I consider the arrangement you propose not as impossible, but as highly
>unlikely.

Fine by me. But I'll be listening to see whether you or Rick ever erroneously claim that precalibrated "S-R" actions are flatly impossible! The foot is in the door, and I think for the good of PCTers' relations with nonPCTers. Unjustifiable "Impossibilities" and "nevers" and "alwayses" and so forth are STUPENDOUS BARRIERS to communication, and I, for one, intend to see that they don't remain as barriers preventing all of psychology from becoming PCT psychology, as it should have long ago!

>I refuse to believe analyses of SR behaviors done by people who don't know of
>any other kind of connection. Their reports seem to verify an SR connection
>because they haven't noticed any of the things that would indicate a closed-
>loop connection. To them, an SR connection seems plausible in the light of
>their model. Their explanations seem adequate because they don't realize that
>control is going on even when they're staring directly at it. They think they
>are seeing all the evidence, but they're not.

I agree, generally. But I'm not ready to say that, in every case, someone's model (or lack thereof) automatically voids their data. Sometimes you and Rick and maybe Tom seem to say that.

>My guess would be that the striking movement is the action of a control
>system (a sampled control system), and is centrally, not peripherally,
>initiated. It is aimed on the basis of a relationship between kinesthetic
>control systems and visual control systems, gradually adjusted over many
>strikes. That's what I'd be looking for in a trace of the mantis' nervous
>system. The mantis' strikes themselves are not purposive -- no action is
>purposive -- but the outcomes of those strikes are under the control of a
>slow purposive sampling control system.

Believe it or not, some nonPCTers have actually been looking for such sampling control systems. I still think a subscription to BIOLOGICAL CYBERNETICS would improve your impression of the worth of (some) nonPCTers' work. A subscription to the JOURNAL OF COMPARATIVE PHYSIOLOGY would help, too. You've been exposed only to the fad-science in SCIENCE for too long!!!

Date: Tue Apr 07, 1992 8:32 am PST
Subject: Rubber bands

[from Gary Cziko 920406.2100]

Dag Forssell (920406) said about Runkel's version of the rubber band demo:

>Here is an excellent, detailed description of the rubber band
>experiment that is MORE INSTRUCTIVE than the way I was introduced
>to it. You invite a friend to experiment ON YOU. "You are the
>experimenter. Move your finger as you like. Watch what I do. When
>you can explain what is causing me to do what I do, let me know."

Another variation I often use when presenting to a group is to ask for a volunteer. I then whisper to the volunteer "Keep the knot over the dot (or other landmark)" and then I disturb. The audience has to figure out what the subject is doing and make guesses, but the SUBJECT reponds as to whether the guess is right or wrong. I can even then have someone in the audience be the experimenter.

It's amazing how difficult it is for some people to find the control variable. It seems the more psychology one knows, the LESS likely one is to find the answer. That's understandable. But why the very sharp control systems engineer I tried it on gave up after a few minutes remains a mystery to me.--Gary]

P.S. I am playing with the idea of compiling a "catalog" all of the various portable control demos that people have used and presenting this at the Durango meeting. If you have a good one that I don't know about (or suspect that I have forgotten it) please send me a personal note about it.

Gary A. Cziko

Date: Tue Apr 07, 1992 9:16 am PST
Subject: Modeling

[From Bill Powers (920407)]

On modeling.

Of the hundred-odd people on this net, I don't suppose more than a handful understand what some CSGers mean when they talk about modeling behavior. So I thought I'd explain it a little, at least as the process appears to me. Talking about modeling is a little like talking about control -- most people have some concept to go with the word, but not many outside the engineering professions (and not everyone in them) mean what I mean by it.

I'm working now on a model of pointing behavior. On the surface, it's not very impressive. The computer screen shows a little stick man with one arm who reaches out and touches, or continuously tracks, a floating triangle that the user can move around from the keyboard in a perspective drawing of a three-dimensional space. It looks like a cartoon of a not very interesting behavior. While movements are a bit more realistic than you find in most cartoons, most people have seen more impressive cartoons on TV in which more interesting action occurs. But behind this surface appearance is the model; what's interesting is not so much what happens on the screen, but how it happens. To explain how it happens I have to distinguish the kind of modeling I use from other kinds.

The first distinction of importance is that this kind of model is not an animation. That is, the various movements of the arm (and head -- the little man always looks at the target) are not simply drawn frame by frame as in the Disney Studios. It's not done the way interactive video games are done, by switching from one animated sequence to another depending on what the user does at the keyboard. Instead, the program is reacting directly to the location and movements of the floating triangle, which are totally unpredictable by the program. I can guarantee that the program makes no attempt to predict the target movements, because I wrote it.

The second distinction of importance is that in this kind of model there is nothing in the program that computes the actual movements of the arm as we see them. If the arm's fingertip moves in a straight line, this is not because something in the program computes the detailed actions needed to produce a straight line. Likewise for curved movements, or movements that begin fast and slow down as the fingertip nears the target. None of these aspects of movement corresponds to any specific calculation of path or speed in the program.

In some approaches to modeling, such calculations are the heart of the method. One looks at the actions, and figures out what commands would be needed to produce them. If the fingertip is to move along a path and intersect a moving target, such a model would use the target movement information as input, and find a path and a speed profile that would bring the finger to the same place as the target some time in the future. Then it would drive the computed arm so as to achieve that path and speed profile, thus bringing about the predicted intersection. Basically, this concept of modeling attempts to reproduce the visible behavior by calculating its details, given all the physical factors of the situation.

The approach I use is more properly called "simulation." Inside the computer are program modules. Each module computes what some simple element of the real system would do when presented with continually-varying inputs. Some of the modules are perceptual modules: they compute what certain nerve signals would do as the aspect of the environment to which a sensor is sensitive changes its effects on the sensor. For example, one module represents a muscle spindle, which emits a signal that depends both on the length of the muscle and on another neural signal, the gamma efferent signal. Another represents the tendon receptors that are affected by the muscle tension.

One of the modules is an effector module: it represents the muscle's response to a motor signal from a spinal motoneuron (including the shortening of its contractile part and the consequent stretching of its spring-like component to produce a force). And there are many more modules that represent the way hypothetical sets of neurons respond to neural signals by producing more neural signals. There are sets of modules that are repeated, with the same interconnections, for each muscle in the model.

In this model, by the way, I don't use actual models of individual neurons, although I could. Such a level of detail would not add anything to the performance of the model and would increase the size of the program and slow its operation. What I do instead is use simple calculations similar to what a neural model would do: add signals, subtract one signal from another, amplify signals, and do time integrations and (rarely) differentiations. Nothing more complex.

Each module is meant to represent the way some small part of the real living system works, as nearly as I understand it. Many of the modules represent guesses based on hints from neurology or even from waving my own arms around and paying attention to the details, and constitute the conjectural parts of the model.

The model is not just a collection of computing modules: it is also a pattern of connections joining one module to one or more others. For example, there are modules representing the static and dynamic parts of the stretch receptors in muscles. The outputs of these modules, conceptualized as neural signals, become inputs to the module representing the spinal motor neuron. This motoneuron module produces an output that is the sum of several positive inputs from other modules and a negative input from the tendon receptor module. The output of the spinal neuron module becomes the input to the module that computes the muscle force output.

And so on. Each module is woven into the whole model through its input and output connections from and to other modules.

A more subtle aspect of this process is that the model contains adjustable parameters in the links between modules. The dynamic stretch receptor module, for instance, sends its signal to the spinal motorneuron module, but there's a parameter that determines how much effect this signal is to have at the spinal motoneuron, and the sign of the effect. If the parameter is set to a high value, the simulated arm behaves sluggishly or, at the extreme breaks into high-frequency oscillations. If it's set to a low value, the arm begins to wobble around, and even goes into ever-increasing low-frequency oscillations. If the parameter has the wrong sign, the arm will behave more and more wildly until the whole program blows up.

So it's not enough to model the right kinds of components of the real system, or even to connect them into a network like the real neural network, with the right signals going to the right places. The quantitative parameters can be adjusted to make a model with any given components and any given pattern of interconnections do completely different-looking behaviors.

Finally, there's a real-time aspect of this "simulation" kind of modeling. All the computations in all the modules are carried out effectively in parallel. One such parallel computation covering all modules represents one increment of real time, dt. In the arm model, dt represents 0.01 second of physical time (regardless of how long it takes the computer to finish all the computations). The last computation is to recompute all the outputs of the modules, so they have all changed before the next cycle when they will be treated as inputs to other modules. This sometimes requires paying close attention to the way the program is written, so that things supposed to be happening at the same time don't accidentally happen in sequence -- one dt too late. In an analog computer this requirement would be easy to meet, because all the computing components would be acting at the same time. But in a digital computer, where there is only one busy central processor that has to do everything, achieving the effect of simultaneity isn't always easy.

After each round of calculations, all the modules have new outputs, which become inputs to other modules (or even the same module) at the start of the next time increment. With a dt of 0.01 second, the result is very close to continuous operation, with all signals (inputs and outputs of modules) varying smoothly and simultaneously. The test to see whether the incremental approach is sufficiently like a true continuous computation is to decrease the size of dt -- let each complete computing cycle represent, say, 0.001 second. If the same behavior results, but in smaller steps of movement, then the larger time increment is short enough. It's nice to use longer intervals, so the movements of the model become fast enough to see between breakfast and lunch. The arm model in its present form runs at about 1/5 of real time (on a 10 MHz AT programmed in C).

One of the modules is a physical model of the arm. The inputs to this module are three torques being applied by the muscle modules to the three joints during one time increment. Using kinematic equations, calculating Coriolis forces and all that, these torques are transformed into angular accelerations around the three joints (taking the moments of inertia and masses of the arm segments into account). Those accelerations are integrated to produce angular velocity, which is integrated to produce angular position. The three angular positions are inputs to the behavioral model, determining the new joint angles and angular velocities, and the new muscle lengths and rates of change of muscle length for the start of the next dt.

There are two inputs to each muscle control system: an alpha efferent and a gamma efferent. When these signals are varied (for testing purposes), the arm will go through certain motions on the screen. I use a standard test signal which simply switches from a positive value to zero and back again, with a half-second interval between transitions.

What the arm segment being tested SHOULD do is move quickly from one angle to another, stay there for a half second, and move quickly back and dwell for another half second, over and over. What DOES happen, of course, is initially something very different. There are five parameters to adjust, representing five meaningful aspects of the control system: three sensor sensitivites, one sensitivity of muscle contraction to driving signals, and the spring constant of the muscle. Only the muscle spring constant can be estimated from observations and data in the literature. The other four have to be guessed at. Finding the right combinations of values can be done in part through computations, but there are so many interactions and nonlinearities in the model that exact predictions are impossible (certainly for me). So what one ends up doing is changing the parameters experimentally until the arm begins behaving properly, or as nearly properly as possible without adjusting the parameters of the other control systems, too.

Estimates of parameter values and especially of the behavioral effects of varying parameters can be made, but only for small segments of the model such as a single control system for a single joint. Such estimates get you in the right ballpark for each control system's parameters. But it's impossible to write the equation for the whole model and solve it for the best values of parameters. The equations are all nonlinear differential equations (made more nonlinear when the visual part of the model comes into play), and the interactions among parts of the model are large (extending the arm at the elbow joint affects both arm segments, for example, through inertial interactions). This brings us to the heart of simulations.

The reason we do simulations is precisely that we can't analyze or even understand the whole model at one time. The postulates of the model are in the definitions of the modules. These modules are each very simple and are closely related to simple properties of the nervous system and muscles. So we can easily understand what each module does or is postulated to do.

What we can't easily understand is what will happen when we connect the modules together in some specific way, with specific interconnection parameters. Our postulates about the modules completely determine the behavior that is implied; the only problem is that we can't deduce our way from the postulates to their actual implications.

A simulation shows us the implications directly. It says to us, "I don't know what you thought you were modeling, but here's what you DID model." It's just like a computer program, which does what you told it to do instead of what you wanted done. A simulation cuts through all the fuzz of verbal explanations and imprecise reasoning about what a particular model OUGHT to do. A simulation is a way of finding out the implications of propositions that are linked together in such a complex way that human reasoning is inadequate to reach a conclusion.

Human reasoning becomes inadequate for most real systems with more than three or four components. Even mathematical analysis is usually impossible in the real world, which doesn't fit the idealized forms that we know how to handle analytically. One result of this fact is that people regularly try to fit the real world to those mathematical methods they DO know how to handle. Every new discovery of some tractable mathematical phenomenon is followed by a hoard of people trying to make nature behave that way. Hence chaos theory and its application to literally every unsolved problem, particularly in the nervous system. There are phenomena to which chaos theory applies; in fact chaos was discovered through observing a working simulation of the weather. But in other contexts it's a solution looking for a problem.

An alternative to analysis is simulation. You hook up a model of the system in which the simple components are represented or plausibly conjectured, turn it on, and gape at what it does. The model then becomes an experimental object. You can play with it, altering its components, their interconnections, and the connection parameters, and learn the effects of each kind of change. Each variation leads you to understand something about the real system. You find out why a given connection is positive instead of negative. You find out why certain connections are present in the real system and others are not. The "why" in every case is simply that the model doesn't act like the real system in some relatively dramatic way. And you can SEE why it doesn't.

A simulation is like an X-ray into the real system, showing you aspects of its functioning that can't be observed directly. Like an X-ray, the simulation can be ambiguous; the observed behavior can be accomplished by more than one plausible model. As with X-ray interpretations, however, we don't have to rely on ambiguous indications; we can think up alternative diagnostic tests that will rule out some possible models, and with increases in technical skill, we can even open up the system and see some of the connections, even monitor some of the circuit activities. Every added piece of observational evidence narrows the field of models that would behave correctly AND work by the right means.

There's another side to the subject of observational evidence. Often the observational evidence is available, but isn't understood. To say it isn't understood is to say that there's no model that needs that evidence. The combined stretch and tendon reflexes are a case in point. These reflexes have been known for close to a century. But nobody has understood what they are for. There have been vague qualitative conjectures, of course. But the arm model I'm working on shows quantitatively what these reflexes do. The tendon reflex controls applied force. The dynamic stretch reflex controls the integral of applied force, or angular velocity. The static stretch reflex controls the integral of velocity, or angular position at a joint. The model shows that with certain values of the parameters, this combination of control systems makes the arm extraordinarily stable, quick to respond to driving signals, and consistent in response over a wide range of external conditions and internal condition of the muscles. While I haven't demonstrated this yet, it's clear now that this combination of reflexes easily compensates for the extreme nonlinearity of the muscle's tension-extension curve. In fact, when I realized finally how this system works, I was amazed at its cleverness and simplicity.

But those who traced the circuits and measured their details couldn't have seen that cleverness and simplicity, because not having modeled the system, they didn't see all the problems that it solves with such economy. These reflexes can be seen as a remarkable design only after you have looked into the problem of controlling a jointed arm in some detail. I couldn't have designed that system. I simply designed the model to be as much like what I knew about the stretch and tendon reflexes as possible, turned it on, played with the parameters, and discovered beauty.

The whole arm model is built up this way. It behaves as it does because of the interactions among its modules. It reaches out and touches the target, and follows the target around when it moves, and looks at the target, and resists gravity, and moves at various speeds and along various paths in the process, because there is nothing else it CAN do. We are seeing in this kind of behavior the necessary consequence of organizing a system the way the model is organized. Maybe another organization would also have to behave this way. But this one behaves like a human being, at least at these levels of organization, and to the extent possible its modules are similar in function to known modules in human systems. The external physics and optics in the model conform to what is known about physics and optics, near enough. Some parts of the model are in one-to-one correspondence with direct observations. Some parts are conjectured. But the X-ray seems to be showing a convincing shadow of the real system, at least as it is seen from this angle.

That is probably more than most of you wanted to hear about modeling.

Best Bill P.

Date: Tue Apr 07, 1992 11:18 am PST
Subject: Re: Rubber bands

#########[[FROM CHUCK TUCKER 920407]]#########

Not to appear self-serving but I have found that the demo I put together for Continuing Conversation several years ago works very well in my classes and Clark and Dennis report that it works fine in theirs. Recently I put on the net two notes about a rubberband demo which used a target with letters; that target was what I used in one of the exercises in the CC demo. I believe it is crucial for testing purposes that the S not be told what to do with his/her finger except to put it in the loop of the rubberband; the focus should
be on the goal or reference signal. Of course, if you wanted to change the instructions to "test" their influence that would be interesting.

I encourage Gary to put together these demos; I think I have put all of mine on the net. Regards, Chuck

Date: Tue Apr 07, 1992 1:53 pm PST
Subject: S-R Rubber Band; S-R vs control models

[From Bill Powers (920407.1100)]
Dag Forssell (920406) --

Re: rubber bands and showing that SR experiments measure environmental properties.

It's true that the disturbance is a property of the environment and thus counts as an example of what I said. But what I meant goes even further. Thanks for reminding me about the rubber bands, because the point is easily illustrated using them. Try this:

Knot THREE rubber bands together at a common point. Do the experiment on a large sheet of paper or against a blackboard. Use three positions of the disturbing end of the rubber band measured relative to the known target position of the knot: large, medium, and small distance from the knot. Make these positions only about an inch different from one to the next. The positions can be pre-marked on the paper or blackboard. The experimenter pulls back to each position and records where the subject's finger goes, marking the positions on the paper or blackboard.

In Experiment 1, the disturber loops two of the rubber bands around his finger, leaving one for the subject.

In Experiment 2, disturber and subject get one rubber band each, the third one just dangling.

In Experiment 3, the subject loops the finger through two of the rubber bands, leaving one for the disturber.

To distinguish the data for the runs, label the subject's finger position marks as 1a,1b,1c, 2a,2b,2c, 3a,3b,3c.

In all three experiments, the size and direction of the disturbance is the same small, medium, or large amount. The subject, however, will respond very differently in the three experiments, as can easily be seen during the experiment and by measurements with a ruler afterward.

I'm not going to tell what happens. You should be able to reason it out from elementary PCT principles, then verify that your prediction is quantitatively correct, using the method outlined above.

If you get the right answer, as of course everyone will, you will realize that you don't even need a subject for this experiment: you can play both parts. All subjects who keep the knot over the dot will behave in exactly the same ways in each of the three experiments. These measurements are not measuring any properties of the subjects. I leave it to the advanced student to say what they are measuring.

Greg Williams (920406) --

Yes, it's possible to look right at a control system and not see it.

Consider the system that Camhi and Tom imagine. If there's a wind from the left, the cockroach turns right. If there's a wind from the right, it turns left. If the wind is directly behind, it doesn't turn either way. That's a nice qualitative picture of how the cockroach turns "away" from the wind.

To make this a little bit quantitative, we could say that the more to the left the wind is, the more the cockroach turns to the right, and so on. If the final direction is to be downwind, more or less, the amount of turn has to be less when the wind is more nearly from behind -- otherwise, if the bug always turned by the same amount, the bug could turn upwind toward the threat when it was already pointed nearly downwind. So in modeling the bug we must arrange for sensor signals from left and right to have differential quantitative effects on rate of turning. The more nearly to the rear the wind is, the less turning is needed. Of course an unbalance to the right should create left turning, and so on.

This model would react as fast as sensor signals could get to the locomotor machinery and as fast as the turning process could be commenced. The amount of signal would affect either step size or step frequency, or both, speeding up legs on one side and slowing or reversing the others to create a turn.

So far we have described essentially the model that Camhi and Tom imagine, an S-R model. The turns begin the instant the wind is detected.
Here is a diagram of the S-R system:

```
----------------------------------------------------------------------
  Left wind sensors \
                 difference -----> turning machinery -> orientation
 Right wind sensors /
---------------------------------------------------------------------
```

Here is my model of a control system for doing the same thing:

```
----------------------------------------------------------------------
  ===>>Left wind sensors \  / Reference difference = 0
||                    difference --> turning machinery ->orientation
||==>>Right wind sensors /                                  ||
||                                                          ||
  ================================<<==========================
           Feedback effect of orientation on relative wind
----------------------------------------------------------------------
```

The only significant difference between these models is that mine contains an explicit connection from the result of acting to the sensors, via an effect on the relative wind direction. That connection is present in the Camhi conception, too. It's simply ignored.

I'm irritated with the Camhi-Tom citation not just for the ignorance of control theory it shows; the worst part is the way the authors, out of nothing but misguided imagination and despite their ignorance, confidently describe how control processes work. They are slow, complex, work only after the fact, and deal only in whole behaviors (like "turning in the wrong direction"). This straw man is set up so that anyone (like another biologist) inclined to dismiss control theory will have his or her prejudices confirmed before the first word of refutation has been spoken. This is sleazy, but unfortunately typical of the way biologists argue against control theory. On the basis of sketchy and largely wrong concepts of how control works, they conclude that there's no reason to learn any more about control theory. They then give reasons why negative feedback is not important, based on superficial and mostly wrong assertions about how a control system would act. It's a circular process: having assumed from the start that control systems can't do the job, they feel free to invent properties of control systems that show how inadequate they are to do the job, and they also feel justified in not wasting any time learning how control systems really operate (after all, they can't do the job). If I weren't so pissed off I'd feel sorry for them.

There are many sloppy arguments in the citation you sent. For example, the authors say that there's at least a 10-millisecond latency in the fastest known response (hair cell affecting leg movement). From this they conclude that no further modification through feedback would be possible for ten milliseconds. The implication is that the maximum hair-cell frequency of firing is 100 pulses per second, which is obviously wrong. They forget that "latency" doesn't mean that the nerves take turns firing, or wait for movements to occur before firing again. Their conclusion is an artifact of the way traditional experiments are done: you're careful to start with no stimulus; then you suddenly apply one; then you look for a "movement" to

become large enough to see and count that as the onset of the "response." This is just naive. The observed "latencies" have nothing to do with neural transit times -- they include integration lags and who knows what other effects, including sloppy observations. This whole approach is just amateurish.

When I get back to the cockroach model, I'll put an escape response into it. There's just no other way to refute all the crap that's being put out on this subject.

You aren't being too careful yourself:

>If the outcome isn't critical, I still think it possible that open-loop >circuitry could be less complex than closed-loop circuitry. And if >"away" truly is all that matters much of the time, a chain mechanism >that BLASTS the organism "away" at high speed could be preferred >evolutionarily to a slower but more precise loop mechanism.

Again I repeat: what overhead, what added complexity, does a control mechanism have in it? Look at the diagrams above: there's no added circuitry at all if you recognize that a reference signal set permanently to zero can just be omitted.

When you refer to a chain mechanism that BLASTS the organism "away", you're arguing from a vacuum: you're imagining that somehow the organism can move "away" without having to compute, somehow, which direction that is. The words sound simple, but that's only because they just skim the surface of the problem and assume that a blasting mechanism must somehow be simpler than a control mechanism. It isn't. I wish you'd think through just what kinds of inner processes would be required, and show me that a blasting model would be any simpler than a control model. I think that S-R models that would actually create something like the required responses would have to be MORE complex than control system models. Put your model where your mouth is.

The whole problem here is that we're dealing in piecemeal examples to "prove" general arguments. Control systems can obviously get into situations where their control is poor. They can be hit by sudden large disturbances they can only partially counteract, or can't counteract at all. They can be required to act with such sudden and extreme outputs that no fine control is possible. In those situations you will see all sorts of "open-loop" phenomena. But they're trivial. The only reason they're of any interest at all is that they tend to occur in abruptly-occurring life-threatening situations, which seem to be the main concern of biologists and others. The control system behaves differently -- abnormally -- in such situations. But it works internally just as it always works. It's just not succeeding in its primary design objective.

I don't see why we have to worry about such trivia. When we get good control system models, we can subject them to highly abnormal situations if we like, and we will see the typical responses of organisms under abnormal situations. Observing how control fails in abnormal situations doesn't tell us much about how it works in normal ones. You can't see any error correction going on when a disturbance mashes the system up against the stops. You can't see any control when some weird external load throws the system into violent oscillations. You have to know how the system operates under normal conditions before you can even identify an "abnormal situation." Unfortunately, most of the data we have on behavior is taken under abnormal conditions where you get responses large enough and dramatically different enough to be seen through the fog of a wrong model.

>... I'm not ready to say that, in every case, someone's model (or lack
>thereof) automatically voids their data. Sometimes you and Rick and >maybe
Tom seem to say that.

It isn't their data that are voided, but their interpretations. And the main problem isn't the data that are taken, but the data that are ignored. Furthermore, we have a lot of "data" reported that is nothing but mislabeling or imagination. The "latency" of motor responses, for example. Most of that latency is the result of doing two time integrations in a row in order to produce enough change of position for a casual (and I use the term intentionally) observer to notice.

Among the things that I consider unlikely is that psychology will become PCT psychology through our efforts of diplomacy or persuasion. The change will occur when the right funerals occur. We depend for our influence entirely on people who willingly learn what PCT has to say and willingly give up their former points of view once they learn it. The remainder will continue their struggle to preserve what they believe in and depend upon, with reason, facts, and models having essentially no effect on them. Only those who have put up a struggle to preserve faith in their own disciplines and have in their own judgment failed are looking for something else.

All we can do is get on with the work. Let the rest of them catch up when they finally realize that they're about to miss the boat. I'm full up with the kinds of opposition, obfuscation, delaying tactics, and face-saving that are put up by people who only want to absorb control theory to the point of removing it as a threat. One can waste his whole life trying to get such people to make the final move, only to discover that they never had any intention of moving.

Speaking of right funerals, a wrong one just occured. Isaac Asimov, to my great and surprising sorrow, died. Isaac Asimov gave me an early inspiration: I wanted to grow up to be Hari Seldon (even though I now completely disbelieve the basis of his science (fiction)). Isaac created reference signals that were worth trying on.

He died when he was 72. If I'm no better off than he was, I have a little over 6 years left. I'll be damned if I spend them suffering fools gladly.

Best to all Bill P.

Date: Tue Apr 07, 1992 5:20 pm PST
Subject: From cockroaches to fleas

From Greg Williams (920407)

>Bill Powers (920407)

>Yes, it's possible to look right at a control system and not see it.

Actually, I can excuse that in Camhi/Tom -- after all, EVERYBODY did that for thousands of years, and ALMOST EVERYBODY still does! Their incoherent logic is what irks me.

>The only significant difference between these models is that mine contains
>an explicit connection from the result of acting to the sensors, via an
>effect on the relative wind direction. That connection is present in the
>Camhi conception, too. It's simply ignored.

But Camhi/Tom think (but do nothing to PROVE!) that during the (initial part of) the turn, there is no functional connection between the sensors (now receiving changing stimulation) and the turning machinery, so one could argue that they aren't simply ignoring the feedback, but saying that it DOESN'T MATTER to the turning machinery (that, functionally, "your" loop is broken).

>If I weren't so pissed off I'd feel sorry for them.

I second that.

>I wish you'd think through just what kinds of inner processes would be
>required, and show me that a blasting model would be any simpler than a
>control model. I think that S-R models that would actually create something
>like the required responses would have to be MORE complex than control system
>models. Put your model where your mouth is.

Consider a simplified flea. It has one sensor, sensitive to air puffs from any direction. An air-puff-from-any-direction of sufficient intensity "triggers" a train of neural impulses from the sensor which are conducted directly to a muscle which contracts and releases a latching mechanism which in turn releases a "cocked" rubber-band-like spring which moves lever-arm legs quickly and results in the flea jumping. (The re-cocking mechanism happens somehow, later.) Some, but not ALL, of the time when there is an above-threshold air-puff directed at the flea by some "problem" organism or non-organism, the flea will jump "away" from the "problem." The jump trajectory will depend on the position of the flea in its environment prior to jumping, and on all sorts of disturbances, and sometimes will lead into the "problem"'s MOUTH. No matter, the odds of not jumping into the mouth could be good enough (after all, the mouth usually only covers a small portion of the possible jumping directions) for evolution to favor the chain mechanism vs. a more complicated (higher energy cost to maintain the comparator) and slower closed-loop system.

Of course, co-evolution of the "problem" and the flea could force the refinement of a closed loop. But I don't see how that solves the speed problem. How could a closed-loop system "respond" as quickly as this "triggered" chain? Even if the closed loop was "maxed out," it would still take longer, so far as I can tell. Remember, the (simplified) chain here has NO synapses and NO comparators. Even when "maxed out," if the loop goes through a comparator (synapse), that would slow it down. If you say there doesn't need to be a comparator for the closed-loop case, because the reference level is always constant, then you will still need a different sort of (non-triggered) apparatus at the output to continually adjust for altered sensory input. But my proposed chain requires none of that extra overhead.

What I'm saying is that triggers are quick and simple. Of course, they are NOT precise with regard to trajectory determination. But such precision isn't always necessary.

One way I see to make a closed loop as fast as the chain is to cheat and add a chain like mine running in parallel. In fact, that could be (in the absence of good data) how the cockroach "escape" is organized: closed-loop when you have the luxury of time to make comparisons, chain when you don't. Ditto for "escape responses" of other invertebrates.

>It isn't their data that are voided, but their interpretations.

Agreed.

>And the main problem isn't the data that are taken, but the data that are ignored.

Yes.

>Among the things that I consider unlikely is that psychology will become PCT
>psychology through our efforts of diplomacy or persuasion. The change will
>occur when the right funerals occur. We depend for our influence entirely on
>people who willingly learn what PCT has to say and willingly give up their
>former points of view once they learn it. The remainder will continue their
>struggle to preserve what they believe in and depend upon, with reason, facts,
>and models having essentially no effect on them. Only those who have put up a
>struggle to preserve faith in their own disciplines and have in their own
>judgment failed are looking for something else.

I think that there IS a way of winning over the masses which will give many nonPCTers what they are looking for. The best example of this method is how Einstein presented his relativity theory: as a MORE GENERAL theory which SUBSUMES the previously held theory AS A LIMITING CASE. In fact, relativity set aside many of the fondest concepts of the Newtonians -- but they found that out only after they were sucked in by the charmingly extended generality promised by relativity theory. It solved problems which couldn't be solved otherwise. I think the same kind of relationship holds for PCT and "Newtonian" (is it ever!) behaviorism, as implied in recent posts, including some of yours. And I propose that Rick (who is so good at this sort of thing) write a paper essentially comprised of questions which nonPCTers can't answer (even some they haven't thought about asking) and the answers provided by PCTers.

Also, the complaints about PCT which I've heard from nonPCTers who "write off" PCT have to do with the lack of empirical support for your detailed hierarchical model, rather than with the overall (ECS) control model. So maybe it would be better to separate the two conceptions a bit more. As I've said before, the overall model can be DEDUCED from a few simple and believable assumptions, and it is the core of the PCT revolution. The detailed hierarchical model actually seems to dilute the impact of the overall model when people are being introduced to control notions. At this stage of neurological expertise, there are endless battles ahead regarding the detailed model. Not so, regarding the overall model; if somebody wants evidence, they get it -- in spades!

Note: It appears that Einstein suffered fools quite well -- and saw his theory become adopted within his lifetime.

Best wishes for many, many more than 6 years!

Greg

Date: Tue Apr 07, 1992 6:31 pm PST
Subject: From cockroaches to fleas

Avery Andrews (920407)

(Greg Williams 920407)

As for jumping fleas, as a long-time chaser of small critters for many years, it is my experience that motion towards the "problem" is often just as or even more effective than motion away from it. What really matters is to get out of optimal grabbing/striking range.

Avery.Andrews@anu.edu.au
(currently andrews@csli.stanford.edu)

Date: Tue Apr 07, 1992 8:57 pm PST
Subject: stick-man model; epistemology

[from Wayne Hershberger]

Bill Powers (920407)
>The tendon reflex controls applied force. The dynamic stretch
>reflex controls the integral of applied force, or angular
>velocity. The static stretch reflex controls the integral of
>velocity, or angular position at a joint. The model shows that
>with certain values of the parameters, this combination of
>control systems makes the arm extraordinarily stable, quick to
>respond to driving signals, and consistent in response over a
>wide range of external conditions and internal condition of the
>muscles. While I haven't demonstrated this yet, it's clear now
>that this combination of reflexes easily compensates for the
>extreme nonlinearity of the muscle's tension-extension curve. In
>fact, when I realized finally how this system works, I was
>amazed at its cleverness and simplicity.

I am delighted by your progress. Please tell us more. I am not sure I understand the basis of your amazement. The role of the three receptors is surely not surprising: tendon-force, dynamic stretch-velocity, and static stretch-length. Is it how the control modules are combined? You mentioned in an earlier post that the reference signal is provided by the alpha efferents. Is the hierarchy, from top to bottom, force-velocity-length, rather than length-velocity-force? What exactly is the cleverness and simplicity that amazed you?

Greg Williams (920330)
>EPISTEMOLOGY (and another topic, if there's room, but it's
>looking like there won't be) will comprise the subject matter in
>the next CLOSED LOOP, due out this month, by yours truly's
>unilateral decree. Those who don't like unilaterality (and even

>those who do) are welcome to suggest possible subjects for the
>July and later issues.

Greg, I wrote the following epistemological remarks earlier this year but did not post them because I preferred that Bill spend his time on the other things he was doing, particularly his exciting work on the little-stick-man model. However, you may want to consider it for inclusion in Closed Loop. And it is still appropriate for the net. So here it is.

****
[from Wayne Hershberger 920106]
I have tried to do my part to slow down the flood of mail on the network, but I have a nagging itch demanding to be scratched.

Bill Powers (920104
>Wayne wants physics to be part of the immanent order.

No. I would say that physics is a science, involving conceptual modeling, as I imagine you might say. I would say that there is order immanent in the phenomenal domain that is modeled by physics. I use your word _model_ to refer to the intellectual achievements of physicists. That is, I use the word model to denote something man-made. Unfortunately, the word model has another, unintended, connotation: a replica of an original. Like Linus Pauling, I do not regard scientific models as being replicas of divine (Noumenal) originals. Theoretical physics does not involve "reading God's mind." I view Einstein's saying that it did as a metaphor.

>you assume...that phenomenal objects and attributes of objects are >something other than neural signals. I assume they are the same >thing. How do we get past that?

As I see it, the issue is a difference between what your theory assumes, and what you say your theory assumes (or implies). I seriously doubt that your hierarchical control theory necessarily implies (or assumes) that phenomenal objects are neural signals. In claiming that your theory is not solipsistic, I find myself in the paradoxical position of arguing that your theory is better than you say it is. That is a sort of disagreement, but one that I think belies a fundamental agreement.

Let me say some things about phenomenal objects, because such descriptions comprise the specifications which we are attempting to reverse-engineer. Please understand that what I say is not presented as an alternative to your theoretical model. What I am trying to do is describe some of the specs that all our psychological models must be able to realize.

Phenomenal objects are simply the particulars of experience. They are the constituents of the empirical world that we are wont to call things. The layman calls them objects or physical objects, and supposes that their substance is essentially material. In contrast, philosophers such as Bishop Berkeley called them perceptions and supposed that their substance is essentially mental.

It seems to me that arguing whether phenomena are substantially mental or material is much the same as arguing whether a magnet is essentially a north or a south pole. The argument makes no sense to me, because phenomena, like magnets, appear to be bipolar, with each instance involving an observer-observed (knower-known) dipole. A dipole, NOT a dichotomy. For instance, the visible surface of every phenomenal object in my study is the one facing that ubiquitous phenomenal object I have learned to call myself. Inasmuch as this personal "perspective" inheres in every phenomenal object, there is more of me to be found in the phenomenal world than is to be found in the phenomenal object I call myself.

This widely distributed aspect of myself which permeates the phenomenal world lends a proprietary aspect to the phenomenal world, making it mine, as it were. That is, the phenomenal world presents itself as a personal "perspective" with that unique point of view being tied to the phenomenal object I call myself. (I put the term "perspective" in quotation marks to signify the observer-observed relationship noted above: a dipole not a dichotomy.)

Locating oneself is an empirical matter, and does not involve merely locating one's brain, as Dennett, for one, has nicely illustrated in his delightfully humorous essay, "Where am I?". Locating onself involves a determination of the spatial relationship obtaining between what might be called the sentient self and the sensed seld

@++ü¦íà-ü¦òïï++V Æ_X[Y.H+½ï$ZX+e called the relationship between I and Me. In my own case, Me is the human male residing (i.e., located) at 436 Gayle Avenue in DeKalb, illinois. I, on the other hand, am distributed throughout my phenomenal world. If _I_ am to be assigned a single spatial location it must be in terms of an interpolated personal station point, or personal point of regard, defined by the personal perspective immanent in the phenomenal world called mine. Normally, my personal point of regard (i.e., I), appears to coincide with Me, particularly Me's head.

When persons are asked to point directly at themselves they tend to point at the bridge of their nose (i.e., at Hering's virtual cyclopian eye). The fact that they are then pointing at their brain is accidental. Imagine a set of siamese twins in which the brain in head X is connected to the nerves of the body attached to head Y, and vice versa. If a flash card bearing the request, "please point at yourself" is presented only to the eyes in head X, at which head would the pointing arm likely point? At X, surely. And if the request were "please point at your brain," at which head would the arm likely point? Might there not be a different reply to the two questions? And if the hand points at heads X and Y, respectively, in response to these two requests, who would have the authority to question those answers? (By the way, I see none of this as being inconsistent with your HCT.)

The same can be argued about the relationship between the We and the Us. The two of Us, You and Me, are in Durango and DeKalb, respectively, but We, You and I, have come together in a dialogue, searching for a common perspective, point of view, or parsing of the world. That is, the proprietary aspect of the phenomenal world includes Our as well as Mine. For one thing, I can imagine (project) my phenomenal world as if from various points in phenomenal space including those that are currently occupied by other individuals. More importantly, I escape an exclusively personal perspective simply to the degree that I demonstrably share a common perspective with others. That is, I escape epistemic isolation (solipsism) not by dint of effort but simply by default. I can not imagine how colors look to a dichromat (they sort pigmented chips differently than I) but I've got an excellent idea about the trichromat's phenomenal world of colors without even trying-- because we judge (see/sort) pigmented chips alike. Claiming that people who sort all possible pigmented chips perfectly alike do not necessarily see colors alike, as some mischievous philosophers are wont to say, presupposes a fictitious absolute standard of comparison [Noumenal color], because the claim of a difference without a superordinate frame of reference is totally meaningless; further, if such a fictional frame of reference is assumed, for sake of argument, in order to allow the claim to acquire a certain syntactical sense (as does the statement, all invisible things are red), it still is devoid of empirical meaning. Paraphrasing Wittgenstein, I submit that a putative difference that makes no difference in phenomenal fact, is in fact no difference.

Whereas it is easy to escape epistemic isolation from others, it appears to be impossible to transcend the phenomenal world itself except metaphorically, that is, by a leap of intellect. We may IMAGINE a noumenal world of "things in themselves" that transcends all experience, but that is not what science does or should be doing, according to the likes of Pauling and Bridgeman. The theoretical models that scientists conceive must be able to generate precise predictions in the phenomenal domain, because that is where the truth value of the models must be tested.

Science models the order that is immanent in the phenomenal domain. Physics is the branch of science that models the aspect of the phenomenal domain that we call the environment. Physiology models the aspect of the phenomenal domain that we call organisms. That is, physiology and physics conceptually model those aspects of the phenomenal world laymen perceptually model as Me/Us and The Environment, respectively. In contrast, Psychology is a science concerned with the conceptual modeling of the I and the We. The psychology of perception is that branch of the science concerned with the problem of modeling the observer-observed dipole as such. That is, when one models the putative process said to underlie the perceptual aspects of phenomena, one may be said to be modeling a modeling process. In other words, you an I are here involved with conceiving perceiving, or of conceptually modeling perceptual modeling.

When I try to imagine phenomena's substance from a psychological perspective (i.e., the essential substance of the epistemological dipole) I find myself coming up with words like immanent order or detectable structure or information--all of which are compatible with physics and physiology. It does not appear inappropriate to call such information, "signals," but it does appear inappropriate to call them "neural signals," thereby excluding all other signal types, because that is to forget the bipolar nature of phenomena. THE EPISTEMIC UNIT IS THE DIPOLE. For example, I comprise a dipole characterized as me AND my environment. In your model, this epistemic unit takes the form of an ecological control loop having two poles, characterized as a unique organism and its environment, including all other organisms. Because there are as many dipoles as there are organisms, with each organism being part of many dipoles, your control theory model is not necessarily solipsistic.

Because a single organism plays a unique role in each of these dipoles it is tempting to suppose that the dipole is within that unique organism. That is, it is tempting to suppose that I am in my head, but that notion is not only illogical, it is also contraindicated by the fact that my phenomenal head is in my phenomenal world--along with a bunch of other phenomenal heads. Therefore, whenever I use the word perception to denote this personal aspect of phenomena, I try to remember that I am referring to a personal perspective or point of view rather than to a personal replica.

The BIPOLAR nature of OBJECTIVE phenomena is what our reverse engineering must explain. Your HCT model accounts for both of these in terms of interacting control loops. As far as I can see, your model poses no epistemological problems, and it disturbs me to hear you imply, sometimes, that it does. If anything, your model promises to resolve epistemological problems, not create them. That's the way I see it.

Warm regards, Wayne

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Date: Wed Apr 08, 1992 6:23 am PST

Subject: Individual and Society

[From Kent McClelland (920408)]

A couple of days ago Chuck Tucker shared with the net his remarks from the plenary session organized by Clark McPhail at which Rick Marken, Tom Bourbon and I also spoke. Today I'm sending along the text of my remarks as well. I apologize for the length, but I hope they'll be of some interest to the net, since they include another take on the issue of social control, which was a hot topic a few months back.

As Rick and Chuck have reported, the session was well received and I certainly had a good time getting together with all those CSG types at a sociological convention, no less!

Implications of Perceptual Control Theory for a Sociological Understanding of Individual and Society*

by Kent McClelland
Grinnell College

Midwestern Sociological Society
Annual Meeting
April 3, 1992

ABSTRACT: The perspective of Perceptual Contrlol Theory (PCT) calls for some rethinking of current sociological conceptions of the individual and of the processes by which social order is attained. Individuals who are organized as a perceptual control systems must engage in one of three basic strategies when sharing their environment with other similar individuals: noninterference, coercion, or cooperation, all of which I define in PCT terms. Assuming cooperation as the basis of social structure leads to an important implication of PCT for sociology: that many "social facts" are better seen as myths, structures to which we attribute control when actual control is being exerted only by individuals. Three techniques of self-persuasion support these myths: techniques relying on rituals, architecture, and documents. Noting that many insurgent movements within sociology have been proposing similar views of social structure in recent years, I conclude that PCT has more in common with these approaches than might at first be assumed and that serious consideration of PCT can contribute to a radical reconceptualization of social theory.

What should sociologists make of the Perceptual Control Theory perspective to which you have been introduced today? As Rick Marken and Tom Bourbon have pointed out, PCT offers a new interpretation of the way individuals behave. As sociologists, however, we're interested not only in individual behavior but also in society. My task is to offer some observations on the implications of the PCT perspective for our view of the individual and society. How must sociological theories change, if we are to take seriously the contention that individuals are hierarchically organized perceptual control systems?

I'll begin my part of today's session by inquiring into the ways that PCT-organized humans can come to act cooperatively, and I'll go on from there to discuss the view PCT gives us of social structures and how they operate. Some of what I say may sound to you like an attempt to go back to first principles, something like the discourse that the "fathers" of sociology, like Durkheim, Weber, Mead, Simmel, Cooley and others engaged in 100 years ago or more. I'm afraid that we are forced to do some of that, because psychology took its wrong turn at about that same time, moving away from the insights of William James to the rigidly externalized views of behaviorism, which held sway throughout the period in which modern sociology developed. For sociologists to take the insights of perceptual control theory seriously means, to some extent, at least, starting again from scratch.

I want to start from the premise that every individual is different in what he or she has learned to control. To put it in PCT terms, each of us has constructed a different set of reference values for controlling our perceptions, built up through a lifetime of learning by the random reorganization of our neural networks in the face of our own unique experiences. In other words, each of us lives in his or her own personally constructed perceptual world. To us, that world is reality. Hence, my reality is not the same as yours. PCT theorists are quick to point out, however, that we all have to operate in the same physical environment, and that our own and other people's bodies and actions are important parts of that environment.

A first sociological conclusion to be drawn from PCT, then, is that, with all these independently guided control systems bouncing around in the same confined environment, social life might be expected to be pretty messy, even anarchic or chaotic. And, indeed, looking around us we do see a good deal of crime, disorder, near-chaos, and general messing up. But we also see social order, and we must ask ourselves, how have people managed to accommodate the creative anarchy implicit in the mix of so many self-guided control systems? In more traditional language, I'm asking, how is an ordered social life possible?

To address this question, we need to consider how control systems can influence each other. When you and I are together in the same environment, my physical body and actions can provide disturbances for your perceptual loops. Similarly, you can disturb my perceptual loops. That's all. Everything you do, moving, making noises, pushing or pulling various objects, or just being there, only affects my behavior to the extent that I monitor it (or some physical consequence of it) perceptually, according to PCT. The practical problem, from the point of view of one control system confronted with the co-presence of another is to produce a set of systematic disturbances which will influence the other person's actions in ways compatible with our own goals.

The problem can be solved in at least three ways: noninterference, coercion, or cooperation. First, we often need simply to prevent others from interfering with the parts of the environment that we are using as part of our control loops. If your goal and my goal don't involve the use of the same segments of our shared environment, or if the environment we share has lots of room (many "degrees of freedom" as Bill Powers puts it), our co-presence will not be a big problem for either of us. In such a situation, noninterference may take place with out any special effort. The two of us may just stay out of each other's way. You'll go your way and I'll go mine. No real cooperation will be required, just minor adjustments to deal with the controllable disturbances that other people's actions present for one's own perceptions.

In more restricted environments, noninterference isn't as likely to work, and coercion is another tactic which requires minimal cooperation from the other person. By disturbing sufficiently the parts of the environment the other person is using to reach a goal, in other words, by deliberately interfering with the other person's perceptual control, you may get the other person to desist from the disturbance which is making the control of your own perceptions more difficult or else to produce a pattern of actions which will suit your goals. (Here Tom Bourbon's two-person control demos are a good illustration.) We're talking about physical coercion when the part of the environment the other person had been using is his or her own body. Of course, coercion often invites resistance and counter-coercion, and to be successful coercion requires constant surveillance of the control system you are trying to coerce, so the tactic has its limitations.

I would submit that most social regularities are achieved only by some degree of positive cooperation, which involves, in PCT terms, the alignment of our goals and perceptions. Somehow, the co-existing control systems in a given environment must adopt the "same," or at least sufficiently similar reference standards. This can be done in a variety of ways. At the simplest level, we can imagine relatively straightforward episodes of cooperation, as when two people pick something up to move it somewhere, or people share some food or join each other in a song (or maybe a sexual encounter?). At a somewhat more complex level, we have the active coordination of efforts (as McPhail and Tucker , 1990, have pointed out), where two or more people take their cues from the instructions of a third person, the coordinator. At the extreme level of complexity, we have what sociologists call social structures or institutions, relatively permanent patterns of alignment of people's goals and actions.

In describing social structures in this way, as cooperative agreements among the participants, I am not saying anything new, but I am laying the groundwork for what I regard as one major implication of PCT for sociology. PCT forces us to take a skeptical, or at the very best, agnostic view of the reality of phenomena we usually designate as social facts. Specifically, PCT describes individuals as control systems but casts extreme doubt on the possibility that so-called "social systems" are truly systems that control. Now, it's obvious that cooperation would happen a lot more smoothly, if the social systems in which people are embedded were control systems in their own right. If groups had the ability to control their perceptions in the same way as people do, many conflicts could be avoided. What the PCT perspective tells us, however, is that while people are control systems, groups just aren't. Groups do not have the nerve connections, the hierarchy of comparators, the physical body to correct errors. Everything that groups do is done by individual people acting as parallel control systems in concert (or failing to act in concert, as the case may be).

I'm perfectly willing to concede that many groups are intended by their members to act as control systems. The people who set them up agree on high-level goals to be reached or standards to be protected and attempt to coordinate their own actions in such a way as to mimic on the group scale the behavior of a real control system. Organizations are the clearest example of this. You might even think of organizations as metaphoric control systems. We often visualize organizations as taking in information, comparing it to internally determined standards, and instructing their members to act in such a way as to eliminate the perceptual errors. For business organizations, evidently, the highest reference standard turns out to be the bottom line. Nevertheless, organizations are not control systems. The only control actually taking place in an organization is carried out in a distributed fashion by the cooperating individuals involved, and they are, in truth, controlling only their own perceptions, since the organization has no central control mechanism apart from the behavior of its members.

Thus, my PCT-inspired conclusion is that social control structures of all kinds are in some sense collective fictions or illusions. We, indeed, reify them not only by giving them attributes of things but also by giving them the attributes of control systems, that is, wide-ranging perception and control. We do our best to convince ourselves that these structures from "society" on down to "the family" or even "best friends" exist in some dimension of reality beyond the day-to-day actions of the people involved.

PCT suggests that we can succeed in fooling ourselves mainly because our own perceptions of the social structures we participate in are vastly oversimplified and imperfect. Our mental models just don't capture the whole reality. Groups often involve the actions of many people spread out over spaces that are outside our range of immediate perception. Likewise, they often are extended in time, coming into existence before our birth and continuing after our death. With such limitations inherent in our perceptions, it's hardly surprising that the mental models used by various participants in social structures often fail to coincide. Still, we generally succeed in attributing to our own inadequate views a semblance of reality. Ethnomethodologists have done a good job for us of documenting the ways by which people manage to deal with ambiguous, contradictory, and incomplete perceptions and still sustain an illusion of normality (e.g., Benson and Hughes, 1983; Mehan and Wood, 1975).

The point is that people have very strong reasons for wanting to believe in social control structures. On the one hand, the alignment of our perceptions in a belief that social structures really exist is a key for getting cooperation from others. On the other hand, disenchantment with society can lead to various anti-social acts. We heard just yesterday, here in Kansas City, news stories about a run on a local bank. Apparently, the rumor got around that the bank was about to close, and long lines formed all over town as people who had suspended their belief in the reality and permanence of the bank sought to withdraw their money (perhaps increasing the possibility that the bank would indeed have to close). Thus, we go about constructing a practical and theoretical discourse to convince ourselves of the reality of our collective fictions. One might describe this as a technology of persuasion that social facts are real. Among the ways in which we convince ourselves and others that social control systems exist, let me focus on three main techniques: rituals, architecture, and documents.

As Randall Collins (1988) has often pointed out, social groups of all kinds make use of ritual occasions to build group solidarity. From the PCT perspective, rituals serve both practical and rhetorical purposes for the participants. First, rituals help people to bring their goals and reference standards into alignment. By participating over and over in the same experiences, people can reorganize their perceptual equipment in similar ways. Furthermore, rituals also are useful in convincing participants to believe in the reality of the collective fiction that the organization exists as a virtual control system. Think back with me to the colorful televised images most of us watched a month or two ago, from the opening and closing ceremonies of the winter olympics. The coordination of the movements of hundreds or thousands of people in the performance of this sort of ritual is a vivid demonstration, to participants and on-lookers alike, of the potency of the social control apparently being exercised by the group, and the impact of these performances on the emotions can help to drive the message home.

My second technique of persuasion, architectural manipulation of the environment, can serve the same sorts of practical and rhetorical purposes in demonstrating to people the apparent power of organizations. Transformation of the physical environment by the erection of buildings, roads, lawns and gardens, etc., puts definite limits on the degrees of freedom others have to reach their personal goals, even as the structures serve their intended purposes of make it easier for individuals to carry out the shared goals of the organization. Physical structures are also tangible evidence of the reality of the "social structures" which put them in place and maintain them.

Finally, control of communication is a key to convincing people of the reality of social control. I referred to this a few moments ago by the term "documents," but much more, of course, is involved. I'm talking about the media, art, and advertising, as well as governmental and commercial documents and academic works. As theorists of ideology have noted, frequent repetition and wide-spread dissemination of ideas and images may serve not only to impose upon people similar models of the world but also to persuade them of the reality and power of the organizations which sponsor the messages. Ironically, sociology has gotten involved in this propaganda exercise. As feminist Dorothy Smith (1989, 1990) has pointed out in her discussion how documents embody the "relations of ruling," ever since the day that Durkheim proclaimed the reality of social facts, sociology itself has been an arrayed as a discipline on the side of those who would attribute agency to social actors, rather than to individuals.

Although I've been arguing here, from a PCT point of view, for the mythical status of social control systems, and have listed some of the techniques people use to prop up that myth, namely rituals, and architectural constructions, and documents, let me close with a couple of disclaimers. First, I am in no way discounting the importance of these myths. To the extent that everybody else is aligned in believing that social systems work that way, a single individual for practical purposes has little option but to go along with it. I'm perfectly happy to agree with W.I. Thomas's dictum that if situations are collectively defined as real, "they are real in their consequences." Any alignment of individual reference standards to form a social structure immediately raises the issue of power, both in its positive guise as enhanced ability to achieve goals and in its negative guise as constraint on the freedom of other people. Indeed, I've spent a good part of my own research time recently trying to apply insights from PCT to questions of power.

Finally, I expect that my message about the mythical status of social control systems has not been altogether new to you. In fact, many groups within the field of sociology have already been saying similar sorts of things for a number of years now. I mentioned earlier the views of ethnomethodologists, who have pointed out the scary depths of uncertainty lurking below the smooth surface of consensual reality, and also feminists, who have made a good case that officially delivered versions of reality tend to suppress other voices and the subjective points of view of powerless groups. Similarly, postmodernists have examined "discourses of power" and have called into question the "master narratives" of our time (see Murphy, 1989). I'll mention, too, the well-known theorist Anthony Giddens(1984), who has commented on reflexivity of social structures and our tendency to reify them. Finally, the most notable convergence between PCT views and current sociology comes from the close similarity between the PCT view of human control systems and the long Symbolic Interactionist tradition of viewing human actors as a creative interpreters of social life. A recent issue of ASR has an article by Peter Burke (1991), who has begun to explore this interface, and, of course, Clark McPhail and Chuck Tucker (1989; McPhail, Powers, and Tucker, 1992) have accomplished some highly significant work in this area, most notably in their simulation models of gatherings.

While I don't expect most theorists from of these persuasions to welcome perceptual control theory with open arms, if only because of the rhetorical stretch involved in adopting PCT terminology, I think the theory has a great deal to offer them, nonetheless. It provides a plausible psychological scaffolding for their attack on the sociological status quo. I take the vigor of these various oppositional groups in sociology as an indicator of a much more generalized dissatisfaction on the part of many sociologists with the lack of progress in the discipline in the hundred years of so since Durkheim's proclamation of his rules of the sociological method. It's about time, I say, for sociologists to begin seriously to reorganize their mental models of society, and the insights of perceptual control theory are a good place to start.

REFERENCES

Benson, Douglas, and John A. Hughes. 1983. The Perspective of Ethnomethodology. New York: Longman.

Burke, Peter J. 1991. "Identity Processes and Social Stress" American Sociological Review 56(December): 836-849.

Collins, Randall. 1988. Theoretical Sociology. San Diego: Harcourt Brace Jovanovich.

Giddens, Anthony. 1984. The Constitution of Society: Outline of the Theory of Structuration. Berkeley : University of California Press.

McPhail, Clark, and Charles W. Tucker. 1990. "Purposive Collective Action." American Behavioral Scientist 34(1):81-94.

McPhail, Clark, William T. Powers, and Charles W. Tucker. 1992. "Simulating Individual and Collective Action in Temporary Gatherings."

Mehan, Hugh, and Houston Wood. 1975. The Reality of Ethnomethodology. New York: Wiley

Murphy, John W. 1989. Postmodern Social Analysis and Criticism. New York: Greenwood.

Smith, Dorothy E. 1989. "Sociological Theory: Writing Patriarchy into Feminist Texts." In Feminism and Sociological Theory, edited by Ruth Wallace. New York: Sage.

Smith, Dorothy E. 1990. The Conceptual Practices of Power: A Feminist Sociology of Knowledge. Boston: Northeastern University Press.

* Presented in a plenary session on "Individual and Society: An Alternative View." This presentation followed presentations by Richard. Marken on "A Perceptual Control Theory Analysis of the Individual in Society," and by Thomas Bourbon on "A Perceptual Control Theory Analysis of Individuals in Cooperative Behavior." I thank Thomas Bourbon, Richard Marken, Clark McPhail, Charles Tucker, Rebecca Bowen, Elena Bernal, Rebecca Meyer, and Maria E. Walinski for their comments on an earlier draft of this document.

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Date: Wed Apr 08, 1992 8:20 am PST
Subject: Subsuming fleas; Random control; Arm

[From Bill Powers (920408.0830)]

Greg Williams (920407) --

Your jumping flea illustrates about the minimal control system that does anything useful. It has one input and one output. The number scale on which its inputs and outputs are measured is the coarsest one that can exist: 1 or 0. The reference level is also the simplest possible one: 0. Thus only a sensor and an effector are physically required.

If no disturbance exists, the perceptual signal is 0, the (virtual) error is 0, and the output is 0. A disturbance must be large enough to cause the perceptual signal to become 1 if any action is to occur. When the perceptual signal becomes 1, the error signal (virtual) is 1 and the output signal becomes 1. When the output signal becomes 1, a physical process is triggered. Normally, this physical process causes the effect of the disturbance to go to 0 and the perceptual signal goes to 0. The physical process can't occur again until the mechanism is reset (which may simply be an automatic process, or may require another control system, or may occur as the initial part of the output action).

The normal result of this control action, however crude, is to maintain the effect of the disturbance (measured at the sensor) at 0. This is therefore a control system, not a stimulus-response system. It would be a stimulus-response system only if the action did not normally remove or at least lessen the effect of the disturbance (although "lessening," in this case, can mean only reducing the sensor signal to 0).

If you want to subsume the S-R concept under the PCT concept, you can say that a control system is an S-R system that produces just those responses to stimuli that bring the effect of the stimulus on the sensors toward some preferred state, in the normal environment to which the system is adapted. Primitive control systems have a preferred state for their inputs of 0 or 1. More advanced control systems have preferred states that can range between 0 and some maximum on a measurement scale with more than two states. Still more advanced ones are hierarchically constructed so that the preferred state becomes an adjustable output available for control of other inputs than the one in the lowest loop.

At an even lower level of complexity than the minimal control system, there can be systems that produce responses to distal stimuli that do not affect the proximal stimulus, but instead oppose the effect of some other environmental disturbance on something else in the system. I would consider this a proto-living system. Something like this may have been at work in the first self-replicating molecules that had negative feedback effects on the conditions that can disturb replication. Because the response has only an indirect effect on some other input variable, its effect will not be reliable; if the effect is incorrect, the response will not change. If the stimulus occurs at an inappropriate time, the response will occur even though the indirect process does not need correction; the response will therefore cause a disturbance instead of opposing it. So this indirect type of control system will work only on the average, and its control effects will be uncertain and weak. This would be enough to bias an evolutionary process toward greater control, but would not itself be a good enough control system to promote survival of the individual.

If, of course, the stimulus in this system were closely tied to occurrances of the disturbance causing the indirect effect, we would have a compensatory system, not a control system. This sort of system could survive an evolutionary process up to the point where compensation was not sufficient to keep a critical variable from being disturbed. The only control loop then would be the evolutionary one. It's an interesting thought that compensatory systems may have preceded control systems. A few vestiges of them may still exist. Like the vestibular reflex in the human being, however, they would probably become incorporated into larger control systems that continually adjust their parameters -- compensatory behavior alone is not sufficient for true control. There is no way to compensate for changes in the output function, in the connection from the output to the critical variable, or in the connection from the disturbance to the critical variable. Nevertheless, in terms of fitness a compensatory system would be better than a blind indirect S-R system, which in turn is better than none.

This sequence strengthens the idea that there is a direction in evolution:

toward greater control of the inputs that matter to the organism. An species can achieve greater control through acquisition of better sensors and output functions, by developong better neural control systems, and by seeking (or constructing) niches that restrict the range of disturbances that can affect it. The cockroach has clearly developed good control systems, but has also opted for small size and simple niches that don't demand greater capabilities of its sensors, nervous system, and muscles. So the living control system can end up in many different sizes, shapes, and degrees of internal complexity. None of those details matter if it can control all the inputs that are most important to it.

Is this getting closer to the kind of subsumption that you have in mind? Would you care to add to it and put it into a more orderly form?

Avery Andrews (920407) --

Re: Small critters

See above for the latest distinction between control systems and S-R systems. If the small critter has a good chance of escaping by running in a random direction, then that would qualify as a primitive control system. Provided, of course, that the ultimate effect was to reduce the stimulus that gave rise to the response. If the result had no effect on the stimulus, or increased it, it's doubtful whether such critters would be around for long.

How long will you be at Stanford? Got a phone number?

Wayne Hershberger (920307) --

Arm model:

>The role of the three receptors is surely not surprising: tendon-force,
>dynamic stretch-velocity, and static stretch-length. Is it how the
>control modules are combined?

Yes. When you just draw the system, it looks confusing because there's only one comparator serving three loops (I posted a drawing like that). But in trying to get the functions of the loops separated, I drew it so that there are three comparators, two of them just being dummy adders that don't alter the functional organization at all. Then it turns into a neat hierarchy, which I hadn't seen before. With that simple conceptual change, I could see how to adjust the parameters, the immediate result being that it's hard, now, to find a combination that ISN'T stable, instead of one that IS. Weird.

I'm sorry to tease like this, but I still have a lot of work to do on the program. With the second kinesthetic level that I've now added, it looks as though the visual systems are going to have to be sampled systems, at least for large jumps of the target, in order to get fingertip trajectories that look like the ones in some literature Greg sent me. With continuous visual control, the fingertip wants to follow some strange curves while the finger is in transit between a starting and ending position that are far apart. But the new kinesthetic level by itself reproduces those trajectories almost perfectly. So clearly the visual systems are not doing any controlling during rapid motions; the final reference position is set and then the kinesthetic systems seek it. This means sampled control, as far as I can see, to keep the visual systems from messing up the trajectories. I could be on the wrong track here, but I'll quit when I get to its end, and let the version 2 out into the world. Shouldn't be more than another week or two.

I'm going to avoid the temptation to get back into the epistemological argument and let you have the last word for Closed Loop. I like your exposition considerably, but there are still problems to work out -- like what we should say neural signals are FOR, in our models.

Best, Bill P.

Date: Wed Apr 08, 1992 12:12 pm PST
Subject: bug #2

From Greg Williams (920408)

>Bill Powers (920408) on subsuming fleas

Before I answer your post, I need to become a little less confused about your dichotomy between S-R and control mechanisms. You can help by giving your views on whether Simplified Bug #2, the BEETLE WHICH PLAYS DEAD, is S-R or control or whatever. This one also has a sensor like the simplified flea, this time hooked up in INHIBITORY fashion to all leg muscles, so that when the air-puff stimulus is sufficiently large, the beetle stops moving (maybe even collapse in a heap!) and just sits still. What "revives" the beetle? NOT anything related to air-puff stimulation, which might or might not continue indefinitely, depending on the nature of the "problem" generating the air-puff; rather, there is an INTERNAL CLOCK which turns off the inhibition after, say, 10 seconds, regardless of the status of the air-puff. After the inhibition is removed, if the air-puff is still present (or there is a "new" one), inhibition resumes. This might be selected for by evolution if the "problem" is usually a predator (like a frog) which tends to notice MOVING objects, but not stationary ones.

Best, Greg

Date: Wed Apr 08, 1992 1:28 pm PST
Subject: S-R

[From Bill Powers (920408.1300)]

Greg Williams (920408) --

Beetle that plays dead:

Is this behavior a control behavior? If the action restores the input to a preferred level, yes. Is it a fast control system? Obviously not. Is it a sampled control system? Yes, if it checks the state of the input only every 10 seconds or whatever. Do I want to guess what the controlled input is? Nope. Is it an S-R system? No: the action affects the state of the input toward a preferred state (by some long path involving, evidently, the behavior of a predators that are sensitive to moving objects). The predator or something depending on its presence is detected, compared with a reference level of 0, and the action becomes 1: freeze. Ten seconds later it checks again: is that bad input still there? If so, go on freezing. If not, by George, it worked again!

The moth that plummets, the cockroach that escapes, the flea that jumps, the beetle that plays dead. Odd that each of these organisms does only one interesting thing. The other 99.9 percent of the time, do they just sit around waiting for an opportunity to plummet, escape, jump, or play dead?

Actually, I suspect that each of these organisms spends 99.9 percent of its time controlling dozens of variables in the manner of a competent control system, perhaps in hierarchical relationships, perhaps in ways that would astound even a control theorist. But that's the sort of thing a conventional scientist sees as "nothing happening." The things that catch the eye of the human observer are the events that stand out: big, sudden, fast, unusual changes from the normal background of nothing much. Much like the things that catch the attention of reporters. After all, "responses" don't go on continuously, do they? How could they, when "stimuli" only occur occasionally? Maybe life is quantized, so it only proceeds when those sporadic stimuli give it something to do.

A true S-R system is one that generates a response that has no effect on the related input in any way except through evolution. So far, nobody has brought up any examples of that kind of behavior. One simple example would be a cockroach that salivates when a particular hair on its butt is tickled. All the examples of that sort that I can think of come out silly, because it's hard to think of a realistic response that's irrelevant to the stimulus that produced it.

Maybe I can think of another kind. At the highest level of organization in any critter, there may be reference signals that turn on when the genes say it's time. Those reference signals result in a response that can consist of learning and modifying control systems as appropriate in the current environment, and controlling thousands of variables at many levels, to produce a perception that matches the new reference signal. That ENTIRE response has no effect on the process that led to turning on the reference signal, except for the evolutionary effects.

Of course evolution is controlled, so maybe there really aren't any living S-R systems at all. Perhaps the person who really wants to study S-R systems should take up physics or inorganic chemistry.

Best Bill P.

Date: Wed Apr 08, 1992 4:56 pm PST
Subject: NOT S-R, CHAINS

From Greg Williams (920408-2)

>Bill Powers (920408.1300)

You have too facilely substituted "S-R" (and your definition of same) for what I was talking about: chains -- that is, chains AS PARTS OF control loops. Both the flea and the beetle are indeed discontinuously acting control systems, and my argument has been that discontinuously acting control systems with some pre-calibrated actions can sometimes be favored evolutionarily over continuously acting control systems, because the former can be simpler and faster.

Your "S-R" is a red herring -- I have agreed with you that what is seen by nonPCTers as "S-R" is really control, insufficiently investigated. "S-R" is a mythology. But chain mechanisms are not. Both the flea and the beetle have chain mechanisms which can be construed as "more economical" and "quicker" than continuous control mechanisms. There is a genuine dichotomy between types of control mechanisms.

We are in agreement that chains can (at least occasionally) be found in organismic control mechanisms, and you have agreed with me that control needn't always be continuous. Now, I see no reason why continuous control should out-evolve discontinuous control in situations (and I suspect there are plenty of them) when precise trajectories are unnecessary and (in some cases) speed is of the utmost, so I doubt that there are only a handful of cases of discontinuous control. But the answer to that dispute lies in the data (interpreted in light of control theory, of course), not in modeling.

Best wishes, Greg

Date: Wed Apr 08, 1992 7:05 pm PST
From Ken Hacker [920408]

RE: McClelland's paper

I found your paper to be very interesting and important in its connections of social processes with PCT. It makes control theory relevant conceptually to the study of social interaction and that is no small task. It is too easy to keep repeating that individuals control their own lives through their own perceptions and cerebral processing. It is much more interesting to read something which begins to suggest the interconnections between the internal and external interactions of human being. One assertion I disagree with concerns organizations. I think that organizational control is something that is an analogue to financial control or to management in general. The "control" center of an organization is contructed by bringing together essential variables and reference levels of the organizational entity into performance standards, organizational cultural values, policies of management, etc. In this way, the organization, like and individual, is disturbed, compares disturbances to desired steady states, makes adjustments, etc. etc. Obviously an organization is not a human. But humans and organizations are both self-organizing. The "self" of an organization is constructed by decision makers and is the secret to organizational successes and failures, i.e., reaching goals or not reaching them.

Thanks again for the inspiring paper. Ken Hacker

Dept. of Communication Studies
New Mexico State University, Las Cruces, NM 88003

Date: Wed Apr 08, 1992 7:15 pm PST

Subject: S-R & Einstein; Evolution

[from Gary Cziko 920408.2000]

Greg Williams (920407) said:

>I think that there IS a way of winning over the masses which will give many
>nonPCTers what they are looking for. The best example of this method is how
>Einstein presented his relativity theory: as a MORE GENERAL theory which
>SUBSUMES the previously held theory AS A LIMITING CASE. In fact, relativity
>set aside many of the fondest concepts of the Newtonians -- but they found
>that out only after they were sucked in by the charmingly extended generality
>promised by relativity theory. It solved problems which couldn't be solved
>otherwise. I think the same kind of relationship holds for PCT and "Newtonian"
>(is it ever!) behaviorism, . . .

While I appreciate Greg's continued efforts at bridge building, as a consequence of the recent S-R vs. control discussions I think I understand much better Bill Powers's reluctance to do so.

While we can see Newtonian mechanics as a special, limiting case of Einstein's relativity, I understand Bill as saying that S-R is SO limiting and SO special that for the most part it is just wrong. And even though technically speaking Einstein's physics is more general than Newton's, practially speaking it is the other way around. For just about anything I want to do in my world of medium dimensions, Newton is right on and it is Einstein who is very special (speeds near the speed of light; masses as big as the sun). (I would guess that even NASA engineers need know nothing about relativity.) Perhaps the better analogy is not that Newton is to Einstein as Skinner is to Powers but rather that Skinner is to Powers as Bishop Paley was to Darwin.

Bill Powers (920403.0700) said:

>Evolution can be the higher control system, particularly if active negative
>feedback control is involved in evolution, as increasingly seems to be the
>case.

Bill, is there some new evidence or new thinking you have here? Also, if evolution IS just due to Darwin's hammer (as I believe the orthodox view still is), would you still want to refer to this type of control at the species level as "active negative feedback"?

Finally, if evolution is a control process as you have speculated, wouldn't it necessarily have to be of the "E. Coli-type" control which involves blind reorganization and selective retention? Or can something more sophisticated be going on?-

-Gary

Date: Thu Apr 09, 1992 5:33 am PST
Subject: Paley/Darwin/BATESON

From Greg Williams (920409)

>Gary Cziko 920408.2000

>Perhaps the better analogy is not that Newton is to Einstein as Skinner is to
>Powers but rather that Skinner is to Powers as Bishop Paley was to Darwin.

I think an even better analogy is Paley (Design): Watson (S-R), Darwin (selection): Skinner (operant), and William Bateson (Mendelian mechanism): Powers (PCT). Darwin and Skinner got part way there, but the (underlying!) mechanism was missing in their theories. Bateson and Powers publicized the missing (and essential) "pieces." Note that Bateson emphasized the importance of the "piece" he rediscovered for COMPLETING the partial theory of Darwin, and neo-Darwinism took off quickly. If PCTers continue to emphasize DISCONTINUITY between their ideas and what went before (and keep trying to portray operant theories as S-R, when in fact they were (not completely successful, to be sure) attempts to get beyond S-R), then I expect there will have to be a LOT of funerals before PCT is widely adopted! I think PCTers waste time arguing about S-R theory which could be better spent showing operant theorists how PCT could help solve problems they've been unable to solve. The operant analysis of its "paradigm" experiment (Skinner box) is truly a special case of control theory analysis, with important concepts left out. It is INCOMPLETE, and PCTers can show the operant folks how to make it COMPLETE, just as Bateson showed Darwin's followers how to become neoDarwinists.

Best,

Greg

Date: Thu Apr 09, 1992 9:54 am PST
Subject: Camhi's cockroach data

[From Bill Powers (920409.1000)]

Greg Williams (920408-2) --

OK, I buy "chains" as "discontinuously acting control systems with some pre-calibrated actions." I'm not so sure about saying they can "sometimes be favored evolutionarily over continuously acting control systems, because the former can be simpler and faster." It is harder to stabilize a discontinuously-acting control system than a continuously-acting one, particularly if it has to operate near the maximum sampling frequency and requires any sort of loop gain. Somewhere in the loop (internally or externally) there has to be a filter that limits the amount of correction made on each cycle to essentially the same amount that a continuously-acting system with the same loop gain would make in the same length of time. But I'll keep my mind open on the subject.

I got hold of Camhi, J.M., Neuroethology (Sinauer, Sunderland MA, 1984), and went through most of the book, but mainly Chapter 4 on the escape response of the cockroach. I must apologize for using bad words about Camhi such as sloppy and amateurish. There is some good experimental science in this chapter, with most of what I would criticize being understandable in a person who knows nothing of control theory.

I now understand what the "cerci" are: they're the two structures sticking out horizontally at about 45-degree angles to the rear of the body. On them are 220 wind-sensing hairs, in the adult, of great sensitivity (they can detect relative winds of 3 mm/sec; compare with the normal walking speed of 100 mm/sec). The two organs together provide directional wind sensing, the signals being proportional both to wind velocity and roughly to a cardiod function of direction. The escape response occurs when the relative wind increases by 12 mm/sec (min) with a minimum acceleration of 600 mm/sec/sec (min). The directional cerci signals (some are nondirectional) make a one-synapse connection to the motor neurons that drive the legs which cause the initial turning response in the appropriate direction.

The amount of turning response is proportional (roughly) to the cerci signal. For obvious technical reasons, no recordings of neural frequencies were made during escape responses. However, in diagrams on p. 80 and 81, the control theorist can find evidence that control is continuous during the turning response.

At the onset of the response, the body turns at a rate that reaches a peak in about 2 frames of the 60fps "high-speed" camera. For the remainder of the turn, the rotational speed drops off asymptotically as the orientation approaches the final direction of running. In all three illustrations, careful examination of the tracings from the movie films shows that the angular orientation exhibits a damped oscillation, overshooting one or more times by rapidly-decreasing amounts. These are, of course, eyeball estimates; one would have to examine the original films with a protractor to be more precise. It's a pity, too, that the high-speed camera was so slow -- the body turns as much as 30 to 40 degrees from one frame to the next. It would be hard to explain the damped oscillations (forces adjusting back and forth) unless there were active control.

In two of the drawings the initial part of the run is shown as a series of orientation vectors. The turn blends into the run, the final directional oscillations being almost zero on the last position shown about 11 cm into the run (measuring the drawing in the text). The running velocity appears almost constant by the time the turn is finished. This can happen because the turn pivots about a point at the very rear of the body, so the turn is also providing a linear velocity component of the center of mass. From my crude measures, it appears that the running velocity is about 90 cm/sec during the straight run, or 2.2 miles per hour (a distance bar was shown and the frames are 16 msec apart). This is considerably below the maximum running speed, as I understand it. If the cockroach survives the initial lunge by the toad (55% did), it apparently is in no great hurry to get farther way. Of course only 11 cm of the run is shown.

Judging the relative wind from the data is difficult. The wind sensor (evidently some sort of direct-coupled microphone probe) is stationary in a position about 1 cm from the cockroach while the toad strikes. It records a maximum wind velocity of 1 to 2 meters per second with a duration of a little over 100 milliseconds. The toad's maximum lunge velocity relative to the cockroach (no absolute measurements given) is about 60 cm/sec just before the cockroach's response begins (from measurements of Fig. 4), so this is a lower bound for the wind velocity. If we guess that the actual peak wind velocity is 100 cm/sec, we can see that the cockroach's running speed (90 cm/sec) nearly matches the wind speed near the point of origin.

Of course the duration of the relative wind will be much longer than that of the wind recorded by a stationary probe, as the cockroach is running with the gust. The cockroach begins its response when the relative wind speed is about 12 mm/sec, or about 1/80 of the peak wind velocity at the wind probe's fixed position. By the middle of the frog's lunge, the relative distance from toad to cockroach has become stationary, so the cockroach is moving away by 60 cm/sec at that time. The relative wind is thus the absolute wind velocity at that point minus 60 cm/sec. Presumably the cockroach will continue its run as long as the relative wind is higher than 12 mm/sec. This means that the cockroach could still be controlling for relative wind speed as much as 1 meter from the start of the run (although the physics of moving air disturbances would have to be worked out to see how the absolute wind velocity would decrease with distance).

The hypothesis that the cockroach is controlling relative wind velocity during the run is far from ruled out.

The latency between the cerci stimulation and the onset of leg movement was measured at about 40-44 milliseconds. With very large puffs it dropped as low as 11 msec. We can take 11 msec as a maximum transport lag in this control system, because neural transmission speed can't be made longer by using smaller stimuli. Camhi mentions that in other experiments, electrical stimulation was applied to the giant interneuron near the cerci, and the action potentials were measured in the axon of the leg's motor neuron. This, of course, would show the actual transport lag. Unfortunately, no number was given. There is, however, a reference:

Ritzman, R. E. & Camhi, J. M. (1978) Excitation of leg motor neurons by giant internurons in the cockroach Periplaneta americana. Journal of Comparative Physiology _125_, 305-316.

Even a transport lag of 11 msec would not rule out continuous control, because the turn then occupies about 9 lag-times (about 96 msec), and the loop gain could be quite high -- ten or so. If the lags are even shorter, the loop gain could be even higher.

I would greatly appreciate it if someone with access to that journal would check out the minimum delay between the first excitatory impulse and the first impulse in the motor nerve axon. This will be the true transport lag, with the remainder of the behavioral latency being due to integrative lags and body/leg dynamics. I suspect that this lag will be shorter than 11 msec; the giant interneuron has an unusually large diameter and will be a fast conductor. The synaptic delay can't be more than a millisecond.

The reason I want to know is to judge whether in addition to this immediate turning response, there can be higher-order contributions to the turning control before the turn is complete. How long does it take the cerci signals to move the approx. 3-4 cm to the cockroach's head? Obviously the running requires higher-order organizations, to coordinate the legs. The turning response would seem to consist simply of shoving sideways with the front legs, a single stroke proportional to the direction error that starts the turn. If more than one stroke is required, a higher system must first reset the legs to the position where another shove can be generated. I suspect that by this time, the higher level system is using the variable pattern generators to continue both the turn and the running (there's a crab-like motion visible after the turn is well under way). The running system does not need direction information; only relative wind velocity information. The directional control system, which does use the direction-sensitive signals, will keep the path headed downwind (equal signals from left and right wind sensors). The other control system will keep the relative wind at the lower level of detection.

I predict that the cockroach will run until the relative wind drops below 12 mm/sec and will then stop, after running one meter or so (very roughly). There are hints in the text that there is a reference signal for relative wind velocity set to match the walking speed. I suspect that this reference signal is set to zero when the escape response begins, or shortly thereafter (anyway, escapes were begun at a low or zero walking speed). Just a wild guess, folks.

I think it's clear that we won't know for certain whether the escape response is preprogrammed or is a control process until these experiments are repeated in toto by a control theorist. The data presented by Camhi is too qualitative at crucial points and is taken at too low a time resolution to settle the question.

By the way, note that figure of 55% for number of escape responses that succeeded. I think we have to relabel this response as an "attempt-to-escape" response. I trust that cockroaches don't encounter this relationship with toads very often in their normal niches (Chuck Tucker's kitchen).

Cockroaches of the world, unite! You have nothing to lose but your chains.

Best Bill P.

Date: Thu Apr 09, 1992 11:36 am PST
Subject: Organizational control

[From Kent McClelland 920409]

(Ken Hacker 920408)

Thanks for your kind comments on the paper I circulated this week (920408). You also raise an interesting issue:

>One assertion I disagree with
>concerns organizations. I think that organizational control is something
>that is an analogue to financial control or to management in general. The
>"control" center of an organization is contructed by bringing together
>essential variables and reference levels of the organizational entity into
>performance standards, organizational cultural values, policies of
>management, etc. In this way, the organization, like and individual, is
>disturbed, compares disturbances to desired steady states, makes adjustments,

>etc. etc. Obviously an organization is not a human. But humans and
>organizations are both self-organizing. The "self" of an organization is
>constructed by decision makers and is the secret to organizational
>successes and failures, i.e., reaching goals or not reaching them.

I imagine sociology will have to make a lot of progress to come up with a definitive answer to your question about whether organizations are "really" control systems. I'm willing to grant that the control loops of one person (the boss!) may may include the actions of other people as part of the loop, and that a powerful person in an organization may be able to control many of his perceptions in that way (assuming his subordinates continue to cooperate). It also seems plausible to me that individuals encountering an organization from the outside may easily sustain the illusion that they are dealing with an integrated control system, if the actions of the organizational members are well coordinated. In fact, I asked a question on the net some time last fall about how one would model the circumstances under which several independently operating control systems using similar reference values might collectively act "as if" they are one big (higher gain?) control system (at least from the point of view of another control system for which the actions represent a disturbance). While I didn't at the time get a chance to explain the rationale for asking the question this way, what I had in mind was trying to construct a PCT model for an organization or other social structure.

Clark McPhail, Chuck Tucker, and I hatched a plan last summer in Durange to write a paper that among other things would delineate the ways in which social structures of various kinds do or do not function as control systems. As far as I know, none of us has made much progress yet on this plan, but maybe they have some better-formed thoughts than I do on the subject. In any case, at this point I think I have to agree with the position taken by Bill Powers in a fairly obscure publication (1986) in which he responded to a James White who made a claim similar to yours, namely that families were "really" control systems. Bill argued that there may be some similarities but that application of system concepts to these entities must be considered a metaphor rather than a model. Maybe he has more to add on the subject now. Another useful source is Philip Runkel's unpublished book-length manuscript, Inside and Outside, in which he has a lot to say about organizations from a PCT view. Of course, his recent book, Casting Nets and Testing Specimens (1990), also touches on the subject and is well worth reading.

I imagine this hasn't convinced you to give up the idea of considering organizations as control systems. Maybe you could give a little more detail about just what an organzational "self" consists of, which seems to be a key to your view.

Best,

Kent

REFERENCES

Powers, W. T. 1986 . "Interaction: The Control of Perception. Commentary on a Paper by J. M. White" CSR Working Paper No. 86-4. Edmonton: Center for Systems Research, University of Alberta.

Runkel, Philip J. 1990. Casting Nets and Testing Specimens. New York: Praeger.

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Date: Thu Apr 09, 1992 5:56 pm PST
Subject: social control

[From Bill Powers (920409.1900)]

Ken Hacker (920408) --

My position is that social or business organizations can't literally be control systems in the same sense that an organism is one. The basic reason is that in organisms, there are components specialized to perform single aspects of a control process AND NOTHING ELSE. A muscle is neither a perceptual function nor a comparator, nor can it be either one. A sensory nerve-ending reports the intensity of stimulation reaching it, but offers no opinions about the larger patterns of which that one stimulus is a part. So there are no problems within an organism of one component usurping the functions of another, and the whole can work smoothly as an organic system.

Organizations can be constructed deliberately so as to mimic or simulate control systems. In my opinion, such organizations work in spite of this attempt to imitate a control system, not because of it. The difficulty with trying to build a simulated organism using people as the components is that the higher levels can't simply assume unquestioning obedience by the lower levels -- not realistically. Neither can the higher levels confidently take reports relayed from lower levels as truthful representations of what is being perceived at the lower levels.

The traditional form of organizations puts a commander-in-chief in charge, loosely answerable to a committee but mostly in terms of the CEO's own choosing. The idea is to focus the major decisions in one person who will not be paralyzed by conflicting considerations. Traditionally, however, this person issues orders to lower levels of management, who in turn issue orders to sub-managers, and so on down to the supervisors of the workforce. The workforce, the "muscles" of the organization, do as they are told and accept whatever recompense is indicated by the economics of production, sales, reserves, capital investment, and profit to the shareholders. So at all levels lower than the CEO, the success of the corporation depends on its operating like a control system: adopting whatever goals are given, and seeing to it that they are met.

This is a power-based structure. People at any level must do as they are told or be replaced. They are not allowed a choice: if they were able to substitute their personal objectives for the ones they are commanded to meet, the CEO would lose control. They, in turn, can offer their subordinates little leeway because they are not free to alter their own objectives. The entire structure depends on obedience, just as in organic control systems.

And that is the problem: people will not work that way. The toiler in the engine-room has opinions about company policy, and also has many other objectives that have nothing to do with the job. What actually happens in organizations like these is that each person does what he or she considers to be the right job, and if that doesn't jibe with the objectives of higher management, the person simply reports what the managers want to hear and goes on interpreting orders as the individual thinks best. I have never worked in a company where this has not been the main mode of operation.

The lowest levels of management are quite aware of this situation and know there is nothing they can do about it. But in my experience, awareness of what is really going on becomes less and less as the level of management gets higher, until at a certain level the managers tend to believe that the company is running exactly as they order it to run. What they don't realize is that only the ability of the lower levels to reinterpret orders and tell half-truths in their reports enables the organization to work at all. The higher the level of management, the less the managers know about the specifics of the organization. They make disastrous decisions which are prevented from being disastrous by the re-interpretations at the lower levels. As a consequence, of course, the upper managers remain unaware of just how bad their decisions have been -- somehow, the desired objective is brought about. They believe that their decisions account for the successes of the organization, where in fact the organization has succeeded by essentially ignoring everything but the desire that the decision was supposed to accomplish.

I'm speaking of a traditional organization, which is traditional in that it represents an attempt to reproduce in a social system the top-down hierarchical control system that is inherent in every person. What makes such social systems seem to work is not the overt structure, but the fact that the people in the organization have decided to support the overall policies and goals of the organization, and take it upon themselves, in their own interests, to do what they can to assure success in spite of mistakes and misconceptions by various individuals. When that committment to some overall concept of the organization is missing, the inevitable result is internal conflict, loss of coherence, and ultimately failure of the organization. When management makes the mistake of enforcing its decisions at all costs, the people do as they are told instead of doing what is required. The result is disaster.

So I claim that for any organization to succeed, it needs to be structured in some way other than as a top-down hierarchy. I'm not saying that it's impossible to simulate a control hierarchy in an organization. I'm just saying that to do so is a mistake.

Best, Bill P.

Date: Thu Apr 09, 1992 5:56 pm PST
Subject: Eigen on Evolution

From Greg Williams (920409)

Gary Cziko asked about what's new in evolution. The following blurb for a new book came in the mail today.

Manfred Eigen, STEPS TOWARD LIFE: A PERSPECTIVE ON EVOLUTION, Oxford University Press, 1992, 220 pp., $29.95

(The following quote is Copyright 1992 by Newbridge Communications, Inc.)

"On the border between non-life and life is the element of chance. But were chance and physics the sole creators of living molecules? Manfred Eigen doesn't think so. Instead, he postulates that the engine driving the origins of life was none other than Darwin's laws of natural selection which, he suggests, do not apply only to organisms with complex biochemistry and recognizable behavior. Rather, these laws reach back to the very beginnings of our evolutionary history to influence the organization and reproduction of the first genes. Not only can laws be formulated governing the emergence of life, they can be tested experimentally. It is even possible to construct evolutionary accelerators, machines that optimize the conditions for certain events.
...
"Eigen proposes that the mechanism of selection plays the role of an entropy-countering Maxwell's demon. Selection, he says, is highly active, driven by an internal feedback mechanism that searches in a very discriminating way for the best route to optimal performance. This is not because selection possesses an inherent drive toward a predestined goal, but because of its inherent non-linear mechanism, which gives the appearance of goal-directedness.
...
"Eigen goes on to discuss the different types of mutations that can arise, forming a 'hierarchy' with the better-adapted arising more frequently and in greater numbers than others. Thus, at odds with the classical Darwinian interpretation of random mutations, he shows that the process of evolution is steered in the direction of the 'optimal value peak,' and steered extraordinarily effectively. This brings about a quantitative acceleration of evolution so great that it is as if selection had the ability to 'see ahead'"

A personal note. An article by Eigen on proto-biological self-organization is what got this (at the time) know-nothing (about biology, at least) soon-to-be mechanical engineer who loved thermodynamics to begin studying molecular biology, which eventually led (with help especially from the late, great Hans Lukas Teuber) to psychology, neurophysiology, and PCT. So this from Eigen is a blast from the past that connects with the present. It really IS all connected, isn't it. ("Like, a loop, man! Far out!!")

Eigen is now head of the Max Planck Institute for Biophysical Chemistry, and he won a Nobel Prize in 1976. I predict this new book isn't semi-senile ravings from on high; Eigen never went in for fad science.

Greg

Date: Thu Apr 09, 1992 11:21 pm PST
Subject: Re: Organizational control

From Ken Hacker [920409]

Kent, I agree that the control concept may be more metaphorical for organizations that any kind of actual model. Still, a good metaphor can go a long way toward asking interesting questions and thinking in new ways. I suggest the following assumptions to my inquiry. First, social groupings have the necessary requirements of systems and hence, we have social systems. Second, social systems are hierarhical and have centers of decision making. Third, human decision making, whether personal or social, entails the basic tenets of PCT. Fourth, humans working in social systems can coordinate their perceptions and work toward managing and steering their systems (influencing

more than controlling, of course). I know that the concept of social control is oxymoronic on this hotline! Still, I think there are some connections between organizational coordination, management, and systems adaptation, which I think can be informed by control theory. While we think as individuals, we can also think out loud as an individual process shared with others. Perhaps, control theory cannot by definition go beyond the level of individual perception. If that is the case, I think there are some clarifications that need to be made. I am open to ideas on this and am not committed to any firm position at this point. Perhaps what might be a kind of social cybernetics has
no substantive connection to control theory and the two angles on systems descriptions need differentiation. Thanks. KEN Hacker

Date: Thu Apr 09, 1992 11:49 pm PST
Subject: Re: social control

[From Oded Maler 920410]

>Bill Powers (920409.1900)]

>My position is that social or business organizations can't literally be
>control systems in the same sense that an organism is one. The basic reason
>is that in organisms, there are components specialized to perform single
                                            ^^^^^^^^^^^^^^^^^^^^^^^^^^^^^
>aspects of a control process AND NOTHING ELSE. A muscle is neither a
^^
>perceptual function nor a comparator, nor can it be either one. A sensory
>nerve-ending reports the intensity of stimulation reaching it, but offers
>no opinions about the larger patterns of which that one stimulus is a part.
>So there are no problems within an organism of one component usurping the
>functions of another, and the whole can work smoothly as an organic system

Not exactly. Muscles and neurons are composed of cells which in spite of being control components of higher-level systems, they have their own metabolism which is a complex control system by itself. If you can consider such a complex purposeful mechanism as "just" a comparator or actuator in a higher-level system, there is no reason, in principle, not to consider humans a components of a higher-level systems.
But I'm sure that muscle-nerve control theory is not so popular among the cells themselves...

--Oded

Date: Fri Apr 10, 1992 9:35 am PST
Subject: He's ba-ack

[From Rick Marken 920410 10:00]

I am back from a trip to the mighty halls of social control -- Washington,DC. I'm exhausted but I have tried to work through some of the mail that confronted me on my return. It was not only voluminous but quite high in quality so I will try to give it my undivided attention this weekend.

Not that we need any new topics, but in case anyone is interested I thought of two threads that might be worth pursuing in the future. One was suggested to me by a recent article on phantom limbs in Scientific American. This is a remarkable phenomenon and might have some interesting implications for the nature of the "imagination" connection in the PCT model. The article was interesting -- but so input-output oriented that it was pitiful. The authors never even considered the possible involvement of efferent neural impulses in this phenomenon. I'd love to hear what anyone else things about the article and the phenomenon described; I think it's in the April issue os Scientific American.

The second topic is related to the SR thread. I realized that there is a very clear example of psychological laws being the inverse of feedback funtions in the psychophysical literature. It is the log vs power law difference in psychometrics. Fechner claimed that the relationship between stimulus (proximal) and response (perceptual) was logarithmic; p = k log s. Stevens said it was a power function p = s ^ k. Actually, these functions describe observed (or , in Fechner's case, derived) relationships between s and r (reponse measures) where r is assumed to be a measure of p. There have been all kinds of snazzy rationales for why this difference occurs -- usually based on assumptinos about how p is mapped into r. I think it has to do with the difference in how r affects a controlled variable. The responses used by Fechner and Stevens are different -- and would be expected to be related to whatever subjects control in these experiments differently. Fechner has subjects say whether stimuli are different or not -- and uses proportion correct (adjusted) as the measure of difference between p values. Stevens has subjects assign a number to the stimulus -- (actually p, of course) that is proportional in size to the magnitude of p.

I don't know how to do the analysis, but I think it should be possible to hypothsize controlled variables that are influenced in a way that is approximately proportional to the inverse of the log and power functions. This would be a way for PCT to show how it can explain some of the "basic facts" of conventional psychology. I haven't spent much time thinking about how to do this; any suggestions?

Dag Forssell (920406) says:

>IT IS TRUE THAT PEOPLE CANNOT SEE CONTROL EVEN WHEN IT IS STARING
>THEM IN THE FACE.

Your example of this is excellent. It reminds me of some of my early experiences with PCT. When I was at Augsburg College I showed the compensatory tracking experiment to a very nice Political Science teacher (he asked) and explained that what is amazing is that the instantaneous distance between the target and cursor cannot really be considered the "stimulus" for the handle movements that keep the cursor on target. Of course, he never believed this. The natural incliation is to view behavior is SR terms -- even when you KNOW what the controlled variable is. It is indeed very difficult to see control unless you know both what you are looking for and what you are looking AT when you find it (the latter requiring an understanding of control theory and the fact that the value at which the controlled variable is stabilized is determined by the setting of a reference INSIDE THE SYSTEM).

It's not easy.

Hasta luego Rick

Date: Fri Apr 10, 1992 2:03 pm PST
Subject: Re: social control

[From Rick Marken (920410 14:00)]

Oded Maler (920410), responding to Bill Powers' comment that in organismic control systems the components are specialized to perform a specific function and nothing else, says:

>Not exactly. Muscles and neurons are composed of cells which in spite
>of being control components of higher-level systems, they have their
>own metabolism which is a complex control system by itself.

This is a good point because it demands further clarification of Bill's point. Let me take a stab at it:

The difference between people and muscles as components of control organizations is that people are control systems at the same level at which they are expected to perform as components; muscles (and neurons and other components of human control systems) are not control systems at the level at which they act as components. For example, suppose I am a component of a social control system aimed at keeping criminals off the streets. I am the "muscle" component of this control system -- a policeman. I respond to certain stimuli ('crimes") by forcibly (if necessary) putting people in jail. The court people then compare this behavior to reference standards (laws) to determine if the person should remain in jail. My behavior (responding to "crimes") happens at the same level at which I ordinarily act as a control system. My perception of a persons actions are influenced by my actions-- so I am in a negative feedback situation with respect to the behavior that I am expected to carry out as a policeman. This is the sense in which my "component of the social control" behavior occurs at the same level as my own control behavior.

This is different for the muscle. The cells in the muscle may be controlling all kinds of chemical and other variables (pressures, whatever). But the behavior of the muscle that is the component of the control loop is not negative feedback itself; it is open loop (dare I say it). The tension of the muscle (which is used by the organismic control system to control, say, the force exerted at a joint) is caused by the electrical impulses coming into it from the motor neurons. But the muscle does not directly influence the motor impulses entering it. Well, it does indirectly when the muscle is part of the control loop. But not if you remove the muscle from the loop. Then muscle tension depends on efferent stimulation but not vice versa. But in the case of the policeman, the feedback loop still exists, even when you take him/her out of the social control loop. A person's response to "criminal behavior" still influences their perception of criminal behavior -- even when they are not purposefully acting as a component of a social control loop.

Another way to say this is: the muscle does not control the input that determines its behavior in the control loop; that is, the muscle itself is not a control system with respect to THAT input (the efferent neural impulses); but the policeman is a control system with respect to the input that is SUPPOSED TO determine his/her behavior in the social control loop (perceived crime). This means that the policeman (unlike the muscle) could set a different reference for the input that is supposed to be perceived as crime. Thus we get many more policeman who revise their idea about what events deserve to be responded to as crimes than we do muscles who revise their idea about what events deserve to be responded to as tension increasers/decreasers. There are "crooked cops" but only faulty muscles (muscles can fail but they don't TRY to betry you).

I think it should be easy to make this distinction formally; it may be important enough to try to do so. I think one of the most important contributions of control theory to the social sciences would be to clarify the difference between a real control system (one that controls its sensory inputs with components that do not control their reference inputs) and a social control system (one that controls its sensory inputs with components (people) that control the reference inputs which are, of course, sensory inputs provided, in part, by other componenets (other people) of the social control system).

Hasta luego Rick

****

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Date: Sun Apr 12, 1992 4:46 am PST
Subject: Purposeful Robots

I'm sorry not to have replied yet to the many postings in response to my assertions about purposeful but sensorless robots. My job usually keeps me far too busy even to read all the CSG postings. I'm just off for a short hol, and will reply in detail when I return, i.e., within a fortnight.

Chris Malcolm

Date: Sun Apr 12, 1992 8:10 am PST
Subject: a clinical application
From: David Goldstein

For the past 21 months, I have been working in a residential treatment setting for teenagers. We have a capacity for 45 residents, 25 males and 20 females. In this post, I would like to describe a new program which I, as Clinical Director, have started which is based on HPCT principles.

Before I will describe the new HPCT program, let me give you some background material. All the clinical staff who work at the Center are part-time except for me. The consultants include: two psychiatrists who are the team leaders, four psychologists who are the therapists and one who does evaluations, one occupational therapist, one drug and alcohol counselor, one recreational therapist. The theoretical backgrounds of the consultants are: psychoanalytic, cognitive/behavioral, family/systems, eclectic.

The clinical programs include a token economy-like status system with five steps. The higher the step, the more privileges the residents are given. A resident moves up the steps by following the rules which are set out in the residential handbook. If a resident violates an expectation, this results in points lost for the day and/or a temporary loss of status. When 25 or more points are lost during an episode of misbehavior, the youth worker staff must write a "critical incident" report which describes the episode.

The HPCT program I have devised is called: Post-Critical Incident Counseling. Unlike the routine individual and group therapy, Post-Critical Incident Counseling is very specific and deals with something that happened at the Center. It is time limited to about 20 minutes. A resident can earn back 60% of the points lost or can have 60% of the days of "loss of privilege" dropped.

Our residents are very resistent to therapy and therapists. They don't think that they have any problems and if they do admit to problems, are not inclined to talk about them. They distrust adults. Most of them come from a background of being abused and/or neglected and/or very dysfunctional families. I devised the Post-Critical Incident Counseling with these difficulties in mind. I thought that something that was brief, provided them with a concrete incentive for talking about a problem which was hard to deny because it happened here, and was oriented to obtaining their viewpoint about the incident might have a chance to work.

For those familiar with Ed Ford's book Freedom From Stress, I am sure that you will recognize it as the steps he goes through when teaching responsibility.

Step 1: Exploration--The clinical staff person offers the resident an opportunity to talk about a specific critical incident and thereby earn some points back and/or knock some LOP days off. The resident is asked some questions to find out: what happened from the resident's viewpoint, what actions(including words said) the resident took during the episode, what the resident wanted to happen, and what the resident was feeling inside.

Note on Step 1: In HPCT terms, the p, r, e, are identified.

Step 2: Evaluation--The clinical staff person asks the resident to judge all of the above. What was good and bad about what the resident did? What kind of person were you during the episode and is this the way you want to be? How do your three main treatment goals, which are the reasons why a resident was placed in residential treatment, relate to the critical incident?

Note on Step 2: In HPCT terms, the resident is being asked to go up a level and view the episode from a higher level. This step involves the sensing of conflict. For example, asking what is good and bad about the actions taken will get to this. Many times the resident can not see anything bad about the actions taken other than the fact that points were lost or days of status were lost or legal charges were placed. If the resident is not in conflict about the actions, the prognosis is less favorable. This step does not formally use the method of levels but it is built into the kind of questions which are asked.

Step 3: Committment-- Based on steps 2 and 3, the resident is asked if he/she is willing to commit to handling the "situation" differently than he/she does. The situation is now defined in a more general way as a result of step 2. The experience(s) which were not being controlled have been identified, hopefully. If there is no committment, then the resident is saying that he/she is unwilling to change in anyway.

Note on step 3--I think this you step has to do with the concept of gain. If we decide that something is really, really important to us, then we tolerate less well deviations from this experience and make more efforts to try to get what we want. We persist when our first efforts do not work.

Step 4: Plan--The discussion turns to how the resident will handle the situation differently. For example, when I am bored in school, I will pull out a magazine and look at it instead of walking out of the classroom or going to sleep. When I am mad at someone, I will get permission to take a walk and calm down first instead taking any action against the person or property.

In many cases, the plan might involve a different choice of actions. However, it could involve redefining what is understood. It might involve redefining what is wanted.

Note about step 4--This is guided reorganization I think. This is the step which requires the most creativity on the part of the resident and therapist. If the therapist finds out that the resident has implemented the plan, additional bonus points can be awarded.

It is too early for me to make any statements about how well it is working. Actually, we are still working out the administrative bugs. My main goal in starting this program is to find a way to shorten the time which goes by before the residents are willing to talk about their problems. And when then do talk about problems, they will have a systematic, 4-step approach, which they could apply to them.

If the program can be demonstrated to work with the clinical staff, I plan to train the social workers and then the youth work supervisors to do it.

The behavioral points system/ status levels is something which existed at the Center before I came. It is basically run by the Residential Living and Education Departments. I have been looking for a way to extract more therapeutic benefit from it and to link it to the individual, group and family therapy which goes on.

Sometimes the youth workers and teachers who use the system get carried away with the punishing aspects of it. The Post-Critical Incident Counseling provides a way for the clinicians to exert a check and balance on this and at the same time, take advantage of the fact that the residents are motivated to reach the higher status levels in order to be elgible for discharge.

Another main goal is to introduce the therapists to HPCT in a way which minimizes conflicts with their existing therapy approaches. This is happening, I think, because the Post-Critical Incident Counseling is a special situation to which the other approaches are not designed to apply. Also, when I introduced Post-Critical Incident Counseling, I didn't emphasize greatly that it was based on an application of HPCT. It was simply a different way to get the residents to talk about personal problems.

During our regular meetings of the clinical staff on Mondays, we have had case reviews in which we discussed the motivation for a particular action. For example, we have a new male resident who steals cars and goes for joy rides. The clinical staff came up with two psychoanalytic interpretations: castration anxiety and phallic narcissism. The staff person who raised these interpretations thought that only through many sessions with resident would it be possible to decide between them or come up with an alternative.

When I discussed the incident with the resident using steps 1 and 2 of the Post-Critical Counseling Format, it turned out that neither of these possible interpretations were supported by what the resident was experiencing at the moment when the car was taken. In view of the interview information, the staff person who proposed the psychoanalytic interpretations reformulated it. The resident was now thought to be fixated at an even earlier stage of psychosexual development than previously considered. This staff person, a psychiatrist, has stated that HPCT therapy and psychoanalytic therapy have more in common then either one would like to admit. While I think this is somewhat true, I have been interested in seeing how people with different theories approach a concrete instance such as the car stealing one. In the HPCT approach, the emphasis on the experience of the resident while engaging in the car stealing seems to have payed off in coming up with an explanation which comes closer to what was going on.

Date: Sun Apr 12, 1992 2:10 pm PST
Subject: social organizations and PCT

from Ed Ford (920412.15:07)

Regarding Social Organizations and PCT:

I have been meeting with a group of my former students every month for the past year or so and recently we decided the following: To attempt to apply PCT to their work, specifically to the way they organize their people, run their organizations, deal with people, etc. Anything they do, from running a staff meeting to a group meeting, from setting standards to dealing with individuals, is to be done with PCT in mind. As a first step, they had to think of others as living control systems. Each time we meet, everyone has to say what they have tried at their various places of employment. We review what we've done and whether it's been effective, and is it their knowledge and application of PCT that has helped. The more you people talk about modeling, the more I realized that I had to get some modeling going of my own.

The jobs held by this group are most interesting. One is a superintendent of schools for Arizona's juvenile residential correctional system. Another heads a residential treatment center for sexually abused 7-11 year old girls. Another is in charge of counseling and training at a teenage boys residential treatment center. Another works with the toughest behavior problems in a school district. Another is a adult probation officer. Another works with the more violent people in a state mental hospital. Another runs groups for women with various types of problems.

These are the kinds of settings where the rubber hits the pavement. These people don't play games, they're serious about succeeding and doing a better job. Both the supervisors and staff in these kinds of settings generally are all looking for a better way to make their job easier, more efficient, and more satisfying. One example we're trying is developing a way to get your staff to do a good job. I think you first have to get each member to explain what he/she has done successfully and what they are presently working on, where they need help and from whom. Problems are brought up, and a person from the staff volunteers to take the responsibility for researching and bringing back the results to the group, for a group decision, yet my experience is there is an ongoing recognition where the final decision rests. In this kind of system, people begin to perceive they have some control for setting reference signals and inputing the system. They become much more cooperative, must more willing to look for a better way. The staff is still operating as an individual control systems but each finds they can, through a cooperative structure, input what is happening, know what other systems are perceiving and controling for, and yet be able to control more easily for what they want and their specific tasks.

As Bill suggested (Powers 920409) When that commitment to some overall concept of the organization is missing, the inevitable result is internal conflict........

Each person is attempting to teach his/her staff PCT. The real key is our working together at our monthly meetings to reflect on what each is doing using PCT as a basis, giving our own input and then watching the results. I guess this would be called modeling in the real world. I have a close friend who is a Catholic priest and was recently assigned his own parish. He already has invited me to give these ideas a try at his parish.

Ed Ford ATEDF@ASUVM.INRE.ASU.EDU
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Date: Sun Apr 12, 1992 6:10 pm PST
Subject: rubber bands

Forgive my naivete regarding control theory, but in reading Dag's explanation of how S-R theory could explain the rubberband phenomena, he said that the act of keeping the rubberband knot stable over a target dot would be <100 % determined by the disturbance and the nature of the rubber band, all of which are properties of the environment.> Isn't the action determined by the actor's PERCEPTION of the disturbance if you're using contol theory to explain it and you are assuming a closed system? Is this the reason why feedback is so crucial to CT and not to S-R (or is it to S-R, too?)

Cynthia Cochran
Dept. of English
U. of Illinois

Date: Sun Apr 12, 1992 10:56 pm PST
From: Dag Forssell / MCI ID: 474-2580

TO: csg (Ems)
EMS: INTERNET / MCI ID: 376-5414
MBX: CSG-L@VMD.CSO.UIUC.EDU
Subject: Rubber band, Disturbance
Message-Id: 33920413065633/0004742580NA4EM

[From Dag Forssell (920412)]
Cynthia Cochran (920412)

>Isn't the action determined by the actor's PERCEPTION of the
>disturbance.....

Cynthia, you are focusing on the wrong thing. In real life, DISTURBANCES ARE NOT NECESSARILY VISIBLE to us. Do not expect to perceive a disturbance. It is only when we talk about Control Theory that we identify disturbances. As a Perceptual Control System yourself, all you have is your perceptions. You cannot with certainty know what causes a particular perception.

What we focus our perception on is the thing we care about, what we in PCT call the VARIABLE. We do our best to PERCEIVE the VARIABLE. As any disturbance influences the variable so that our perception of the variable deviates from the perception we want, we ACT somehow to bring our PERCEPTION of the VARIABLE to the REFERENCE perception we WANT. When we are successful in that, our COMPARISON between the perception we want and the perception we perceive gives zero ERROR SIGNAL. We cease acting.

With regard to the rubber band, your action is determined only by the difference between where you want the knot to be and where you perceive it to be at the moment. If you cannot see the disturbance at all, your action has to be the same. The knot might shift because a wind blows, because the temperature changes, because the rubber ages, because somebody moves a magnet behind you or any as yet unexplained natural phenomenon. It does not matter whether you know what or where or how much if any "disturbance" there is. It could just the same be a change in the output of your own muscles. You can also yourself change where you want the knot. Either way, you change the output that directs your muscles so that in the end you perceive what you want to perceive. BEHAVIOR IS THE CONTROL OF PERCEPTION.

>Is this the reason why feedback is so crucial to CT and not to S-R
>(or is it to S-R, too?)

Once you understand PCT, you understand that with PCT you can explain and model the nature of what is going on. With "S-R" you cannot explain or model anything, because it is only half the story. Feedback is present in and crucial to any living organism at all levels from inside single cells and up to the highest levels of complexity.

Is this comprehensible?

Dag

Date: Mon Apr 13, 1992 2:45 pm PST
Subject: Chaotic perception

[From Bill Powers: Relaying some direct mail at Martin's suggestion]
[Martin Taylor 920412 20:35] (Bill Powers 920403.2000)

I'm sort of back--a bit jet-lagged and with a nasty cold, so not thinking well.

But--
>For modeling perceptual functions, by the way, there's a requirement that
>people like Freeman don't seem to consider in their "chaos" models. The
>requirement is that the state of a perceptual function has to be knowable
>by other parts of the brain. A logical level, for example, has to be able
>to know that a given experience goes in category A rather than category B.
>It isn't enough that the categoric perceptual function be in some unique
>state corresponding to each category. There has to be a way for the
>category level to tell a higher level which state is present (this
>applies,
>of course, at other levels, too). This is why I don't like Freeman's model
>in which the olfactory system falls into various basins. Who knows that
>it's in one basin or another?
>
>---
>This is pretty stupid because you're going to be gone for ten days before
>you see this. But if I don't comment now I'll lose the thread: too many
>things going on in my sedentary retired life.
>--

Well, the older mail is what I see first, and respond to before I see the more recent stuff, so maybe you have forgotten what this is all about. The next trip (in 2 weeks) will be for 6 weeks, which is much worse.

The big problem in the quoted paragraph is in the interpretation of "knowing." It is quite true that there is a problem in the chaotic systems of identifying the basin into which the orbit has hopped. That has been one of the most difficult aspects of the problem for us, not just for Freemen et al. The way I have been treating it is that the various basins do have unique and separable characteristic features, or at least their attractors do, whereas the set of possible orbits do not. This makes the basins categorizable by their attractors, but it doesn't tell how to make that categorization. Thus far, we haven't gone further than to say "the information is there, so a way can be found." It could well be that there are sequence detectors in some perceptual functions of some ECSs that tune to the different attractors. I don't know. But if something like that exists, then the individual ECSs would each get a signal of the degree to which "their" pattern was present. How one would get that into a reference signal, I haven't a clue.

Martin

PS. You made a nice comment about accepting one's own error. But what else can one do when one has made an error and knows it?
==

[From Bill Powers (920412.2100)]

Welcome back --

RE: chaos as a perceptual model

>The big problem in the quoted paragraph is in the interpretation of "knowing."

My criterion for models of perception is very simple: they must account for the world of experience (i.e., the whole world you see around you and all its phenomena) and for the ways in which some perceptions depend systematically on others. I don't see how the behavior of a chaotic model bears any resemblance to the world of experience. Neither can I see how hierarchical relationships can be handled by chaotic models (which is the point I was making in asking how other parts of the brain know the state of a chaotic perceptual function). It seems to me that by assuming a chaotic perceptual function, one simply pushes the problem of perception back a level: now we need a recognizer that can distinguish among a very great number of subtle differences between chaotic states. It seems to me that this problem will be no easier to solve than is the problem of how perceptions are derived from non-chaotic signals, under a different set of assumptions.

When you said "That has been one of the most difficult aspects of the problem for us, not just for Freemen et al." I thought "Oh-oh!" That "for us" hints of an ongoing project that is deeply committed to using the chaos model to explain perception. If this is the case, I should really just drop this matter entirely, because there is no way I can avoid stepping on toes. I have no committment to any model of perception, but so far I have seen nothing that suggests a relationship between perceptual processes and chaotic processes. This looks to me like someone is putting the cart before the horse: chaos is a hot new concept, so there must be some application to perception. I think we must proceed the other way: state what it is about perception that we're trying to explain, and then look for the best and simplest model to explain it. I don't see anything very persuasive about the chaos model, except, of course, to explain phenomena that show the properties of chaotic systems: overdriven nonlinear resonators or oscillators.

Somehow, whatever model of perception is used, one must come up with the world we experience: a world that is as nearly noise-free as anything I can imagine. The real world contains clearly delineated objects that can move, behave, and relate to other objects, all in smoothly-variable ways that are systematic and regular to the limit of detection. Any perceptual model has to account for this appearance, because this appearance IS perception. Until this basic problem is solved, I don't see any advantages in speculating about more complex perceptual functions -- any answer we guess at now is bound to be radically changed when we find out how the world of appearances is actually created in the brain.

There's one more requirement: a perceptual model has to work in a control system. Any model that can't do that is probably the wrong model.

-- Best Bill P.

From Martin Taylor by way of Bill Powers

RE: Chaotic perception

>When you said "That has been one of the most difficult aspects of the
>problem for us, not just for Freemen et al." I thought "Oh-oh!" That
>"for us" hints of an ongoing project that is deeply committed to using
>the chaos model to explain perception. If this is the case, I should
>really just drop this matter entirely, because there is no way I can
>avoid stepping on toes. I have no committment to any model of
>perception, but so far I have seen nothing that suggests a relationship
>between perceptual processes and chaotic processes.

No. Actually, it worked quite the other way round, and there isn't a great committment. What happened was, I suspect, much like what happened to you when you detected the beauty and necessity of control systems. We were running a kind of informal weekly seminar group dealing with perception and cognition, and getting a whole lot of concepts coordinated, dealing with feedback in the perceptual system (not the behavioural-environmental loop PCT deals with). What happened was that we found that the chaos people had been there before us, and it was exactly what we were blindly stumbling toward, as a necessity for all systems that might pretend to life. If those proto- life systems also pretended to symbolic logic (categorization), they also required catastrophic effects, which we modelled (bad word, perhaps) as cusps.

My toes are pretty numb. The don't mind being stepped on, though my ears prefer to hear nice things.

I suppose your last sentence is at the heart of the problem. No, one doesn't see the chaotic processes. Like quarks, they are almost inherently hidden. (Actually, after writing that, I was amused, because one of the other fascinating connections is that the mathematics leading to the basins of attraction in the perception is extraordinarily like quantum electrodynamics. QED).

>There's one more requirement: a perceptual model has to work in a
>control system. Any model that can't do that is probably the wrong
>model.

> That, I'll go along with.

Good night. I don't think I have a copy of what I sent you instead of the net, so if you think it might be worth posting, please do it for me. Yours, and this too, if you don't think it too great a waste of bandwidth.

Martin
==
[From Bill Powers (920413.1600)]

[Added for this post]

>If those proto-life systems also pretended to symbolic logic
>(categorization),
>they also required catastrophic effects, which we
>modelled (bad word, perhaps) as cusps.

As an old electroniker, I was pleased to see flip-flops, trigger circuits, and one-shots given a fancy new name. On the other hand, considering some of my failures, the name -- catastrophic effects -- is perhaps a bit TOO appropriate.

If a chaos model of perception corresponds to an inherently hidden phenomenon, it will be a little hard to test.

Bill P.

Date: Mon Apr 13, 1992 3:14 pm PST
Subject: Re: Chaotic perception

[Martin Taylor 920413 18:30]
(Bill Powers acting as relay)

>As an old electroniker, I was pleased to see flip-flops, trigger circuits,
>and one-shots given a fancy new name. On the other hand, considering some
>of my failures, the name -- catastrophic effects -- is perhaps a bit TOO
>appropriate.

I interpret that as facetious, but it has a germ of truth. The fundamental structure that permits the computer to operate reliably is a fold catastrophe, of which a flip-flop is a good example. But it's more than a fancy new name. It's a generic description rather than a specific one, which all your three examples are. It's usually a good idea to see the general structure underlying
the specific examples. Also, by noting that the catastrophe underlies the operation of the computer, we can see why it is inherently difficult for the computer to model the continuous functions of cognition.

>If a chaos model of perception corresponds to an inherently hidden
>phenomenon, it will be a little hard to test.

True, but by analogy to the quarks, the effects should be discernible. The nature of life demands that the chaos be there. The nature of control demands that it be not readily apparent in living things. That doesn't mean one should be unable to test it or take advantage of knowing it is there.

Martin

Date: Mon Apr 13, 1992 8:44 pm PST
Subject: CT and organizations

From Ken Hacker [920413]

Bill Powers (920409) --

I concur with your argument that organizations cannot literally be control systems. I also agree that those which try are authoritarian. However, the fact that humans in an organization interpret and change signals to meet their own ends does not negate the possibility of social organization involving some of the principles of control theory. I think the issue is where those principle are and are not in effect.

Forms of hierarchy are irrelevant if we look directly at the process of human organizing. When a person organizes his or her affairs and when a company organizes its actitivites, the same basic processes are involved: perception, comparison, regulation, adaptation. While the signals within a biological network are "pure," the signals, messages, and exchanges of human communication are far from pure. Yet, we still move toward the same type of goals, namely steady states.

In your "power-based structure," (what structure is not power based, even CSG-L??), people must do what they are told. But let's lessen the authority factor and we still see that all social organization is dependent upon the management of perceptions and actions directed toward desired states. Employees can be persuaded, enticed, or simply stimulated toward self-management practices; force and coercion are not necessary to gain compliance in organizations.

More importantly, it may be that organization and control theory are mainly limited to the individual level of analysis and that social influence is what connects one control system to another, whether one-on-one or regarding an entire social system. If this is true, then the organization functions with control theory as distributed control systems working in networks of influence and information flow. Such a possibility is consistent with current theories of organizational communication. So I agree with the argument that social systems should be regulated by individuals regulating themselves. What needs to be explained is how we help individuals do their own controlling as effectively was possibile while facilitaing a kind of management which brings together individual and organizational essential variables. This may be the essence of what managers are seeking when they talk about self-management. SM is probably CT in new clothes!

KEN

Date: Tue Apr 14, 1992 2:07 am PST
Subject: RE: a clinical application

David:

The incident counselling process looks good. I am a graduate thesis student under Tom Bourbon at Stephen F. Austin State University, and I am also being trained in the professional program in psych. I will attain my M.A. in May. My experience in working with adolescents in psychiatric hospitals dates back to 1988. The type of clients you are treating, often sabotage their program when they get "in the hole" or reach neg. points. Offering them a way out via gaining 60% of the points lost in an incident is a great idea to help them get "out of the hole." Also the means of getting to that goal, which is stating thoughts & feelings in the 1. exploration 2. evaluation 3. commitment & 4. plan steps, is a great way to encourage the adolescents' participation in counselling. I hope that HPCT is beneficial to the staff and can be used to prevent those power struggles that staff and adolescent clients so often have.

Clifford Gann (Trey)
SFASU, Nacogdoches, TX

Date: Tue Apr 14, 1992 8:12 pm PST
Subject: PCT Successes at U. Ill.

[from Gary A. Cziko 920414.2300]

Partly in response to recent discussions about PCT's acceptance (or lack of) by mainstream psychology and social sciences, I thought I would share some heartening news from my university.

First, a short while ago Joel Judd (my doctoral student and oftentimes teacher) passed his final doctoral examination (now you know why Joel has not been active on the net lately). The title of his dissertation is "Second Language Acquisition as the Control of Nonprimary Linguistic Perception: A Critique of Research and Theory."

There are a number of noteworthy aspects of Joel's achievement. Perhaps the most remarkable is that he was able to get three other faculty members of this prestigious research institution to pass a dissertation which pulls the rug out from under the considerable research literature in second/foreign language acquisition. The interaction with the committee wasn't always pretty, but Joel's written and oral defense of this radically different perspective was such that they didn't have much choice but to eventually sign on the dotted line.

Second, I just received word that the article I submitted to _Educational Researcher_ with the title "Purposeful Behavior as the Control of Perception: Implications for Educational Research" has been accepted for publication. Three out of the four reviewers were positive, the remaining one was mixed. I am particularly pleased that although the article is almost twice as long as the usual limits for articles in this journal, I have not been asked to reduce its total length. Indeed, the editor asked me if I could expand the PCT discussion even more by reducing some of the non-PCT parts of the paper (the paper begins as a rebuttal of a critique of an earlier paper from my "chaos" period). _Educational Researcher_ is a widely read publication specializing in basic issues in educational theory and research methodology and is received by all members of the American Educational Research Association. Part of the abstract reads:

"In the course of this discussion, a theory of purposeful behavior known as perceptual control theory is presented which provides an explicit, working model of how individuals are able to produce repeatable outcomes via variable means. It is argued that the traditional "scientific" method of educational research which attempts to find relationships between "independent" and "dependent" variables over groups of individuals is in principle incapable of leading to valid explanations of the how and why of purposeful human behavior."

I must admit that I was a bit disappointed not to have gotten many of the amusing comments of the type that Marken and Powers and other PCT "old timers" have been so successful in collecting. The best I got was one reviewer's assertion to the effect that perception and behavior are not simultaneous!--Gary

----

Gary A. Cziko Telephone: (217) 333-4382
Educational Psychology FAX: (217) 244-0538
University of Illinois E-mail: g-cziko@uiuc.edu
1310 S. Sixth Street Radio: N9MJZ
210 Education Building
Champaign, Illinois 61820-6990
USA

Date: Wed Apr 15, 1992 4:37 am PST
Subject: Congratulations!!

From Bill Cunningham (920415.0815)
Gary Cziko (920414.2300)

Congratulations to you both!!

Does this mark UIUC as the new center for iconoclasm?

Bill C.

Date: Wed Apr 15, 1992 7:21 am PST
Subject: Gain, Misc

[From Rick Marken 920415 08:00]

While working on a spreadsheet model that demonstrates the behavioral illusion (I've got to try to have something to present at the CSG meeting) I realized how important it is for control systems to be able to adapt to changes in the gain of feedback loops caused by changes in the function, g(), that relates output to input. We often talk about the importance of control organization in a world where the results of actions can be influenced by unpredictable disturbances. But control is also important in a world where your own effect on the results that you care about can be quite unpredictable. If these changes in gain are relatively small then you won't notice them and the control loop will take care of them. But if the changes are large enough (and I think they often are) then you must have other control systems monitoring your effect on controlled variables and altering gain as necessary. This makes me think that gain control systems probably had to emerge very early in the evolution of living control systems. They must be an very important aspect of our own nature. I bet these gain control systems have a lot to do with what we experience as emotions. The experience of being in a loop with too much or too little gain may have something to do with what we experience as "elation" or "depression". Any coherent ideas about this would be most welcome.

Since things seem fairly quiet these days I thought I'd ask some questions about mundane matters. First, to President Ford -- any news about the CSG meeting? Same format as last year? Will there be any scheduled talks, sessions, demos, whatever? Is there a newletter coming out soon? Second, Greg Williams -- how is the LCS-2 book coming along? When can I buy a copy?

Joel Judd and Gary Cziko -- congratuations to both of you. Gary, could you send me a reprint of the article that was accepted? Thanks.

Best regards Rick

Date: Wed Apr 15, 1992 7:54 am PST
From: Dag Forssell / MCI ID: 474-2580
TO: Gary (Ems)
EMS: INTERNET / MCI ID: 376-5414
MBX: G-CZIKO@UIUC.EDU
Subject: Acknowledgement
Message-Id: 04920415155440/0004742580NA4EM

Gary, further to my gripe. I just thought about it. What motivates me is not mostly a matter of courtesy. I agree that may belong outside the net.

On my next post I plan to state that:

Part of my personal purpose in responding to your question is to test my own understanding and my ability to spell it out. Please acknowledge my attempt to answer your question and let me know if it appears valid, clear and addressed your question.

How is that? Dag

Date: Wed Apr 15, 1992 8:25 am PST
Subject: Thanks

[from Joel Judd]

To everyone,

Since Gary has already sprung the news and uttered lavish praise, I must reciprocate and elaborate. Since a book based on my thesis may be awhile coming, I'll give you a preview of the acknowledgments page by expressing my thanks publicly for all the discussion and personal advice I've recieved from you all the past year and a half. It was indispensible to the final product. I'm reluctant to name names because I'll leave someone out, but one thing Gary didn't mention is that I cited several postings in my dissertation also (so be careful what you say on here). Gary also ran a lot of the interference from committee members to get the thesis approved.

It's funny, but now that it's done, seems like 'twas nothin' special. Must be the post-reorganization vantage point. Again, thanks to all.

Anybody want to hire a language learning professional?

Date: Wed Apr 15, 1992 10:14 am PST
From: g cziko
EMS: INTERNET / MCI ID: 376-5414
MBX: g-cziko@uiuc.edu

TO: * Dag Forssell / MCI ID: 474-2580
Subject: Re: Acknowledgement

Dag:

>On my next post I plan to state that:
>
>Part of my personal purpose in responding to your question is to test my
>own understanding and my ability to spell it out. Please acknowledge my
>attempt to answer your question and let me know if it appears valid,
>clear and addressed your question.

Sounds fine, except for the "personal purpose" part. How can a purpose be anything but personal?

I appreciate your concern for keeping the net running smoothly.--Gary

Date: Wed Apr 15, 1992 10:17 am PST
Subject: To Martin Taylor

I was having some difficulty posting this directly to Martin Taylor's address so I am taking the liberty of posting it to the net. Martin wisely responded to my post about psychophysical laws in private -- I can't imagine this being a hot topic for many people on this net.

[From Rick Marken 920415 10:30)]

Hi Martin

Thanks for the reply to my post about Fechner/Stevens law.

> (By the way, Stevens used many dimensions
> other than number for the controlled variable).

I know -- cross modality matching. Fun stuff.

> The net result is that I never believed
>Stevens, and thought Fechner had a simplified view of an essentially correct
>position.

I think you are on the track of the kind of thing I was thinking of. I wouldn't be surprised if something like what I am suggesting (that the psychophysical law is a reflection, basically, of procedure -- ie , the feedback function from output to input) has already been done in some way. I seem to recall some work by a fellow named G. Lockhart, at Duke I think, who showed how log scales are basically an artifact of category scaling. I'll try to think of a simpler way of demonstrating what I am thinking of if my life becomes a bit less hectic again sometime. Ah, if I could only return to the idyllic pastures of academia -- and still stay here in LA LA Land.

Best regards Rick

Date: Wed Apr 15, 1992 12:44 pm PST
Subject: Gateway to NetNews (Usenet)

[from Gary Cziko 920415.1530]

I propose that CSGnet (CSG-L) be "gatewayed" globally to NetNews (formerly referred to as "Usenet") so that it will appear as a newsgroup on the worldwide NetNews system.

NewNews is a global electronic bulletin board with hundreds of newsgroups. Users with access to machines connected to NetNews (usually "mainframe" computers) can read and post to any of these groups (some groups are moderated). The advantage of NetNews is that is allows one to participate in electronic discussions without having one's mailbox fill up with network messages. A global gateway would also make CSGnet available to thousands of sites throughout the world. More information on the advantages of the NetNews connection is described in the attached document.

It is now possible to obtain a Listserv (current CSGnet status) to NetNews gateway WITHOUT a required voting process. The steps to this process are included in the attachment. No. 1 has already been accomplished. We now need to handle No. 2 which says:

"2. Get the approval of the Listserv list readers. This could be done somewhat informally by posting to the list and asking if there are objections. If the issue was controversial, a formal vote should be held according to the same guidelines as for creating a new Usenet group. If there was no major dissenting opinion a vote will not be needed."

At the last CSG meeting it was decided that we should attempt a NetNews link. However, some recent discussion on CSGnet has doubted if this would be a good idea, fearing that this might dilute the quality of our discussions and make CSGnet open to certain undesirable elements. I don't believe that these are real dangers. Here are my reasons.

1. We would be listed in NetNews with other listserv linked groups and our name would be "bit.listserv.csg-l." This is not likely to catch the attention of a potentially trouble-making list peruser (in contrast to such names as "alt.bestiality" and the like). We would NOT be listed with the prominent "science" groups starting with "sci." such as "sci.psychology" or "sci.biology."

2. I know of two other listserv groups that have recently made the NetNews link with no ill effects. Joyce Neu said: "I found no drawbacks to having SLART-L or XCULT-L on Usenet. And none of our subscribers has ever voiced any complaints."

3. As Listowner I can disconnect the gateway connection at any time if it proves undesirable. Alternatively, the gateway can be turned into a one-way connection so that NetNews connectees can see but not be heard.

If there are no objections raised witin the next seven days, I will proceed with the gateway connection. If objections are raised which cannot be quelled, a vote will be taken.

P.S. CSGnet has been a local NetNews group here at the University of Illinois for about a year now. This allows students with small electronic mailboxes to participate in the net without disrupting their personal e-mail. This would be possible worldwide if we set up a global Usenet gateway.

==

****
*** General Information on Listserv/Netnews gateways ***
****

This document gives general information on bi-directional (or optionally uni-directional) gateways between Listserv and Netnews as implemented in Netnews version 2.4 by Linda Littleton at Penn State University. It includes guidelines on how to establish a new Listserv/Netnews gateway.

*** What is Listserv?

Listserv, which stands for List Server, is a mail list server that runs on VM/CMS. It provides "mail-exploding" capabilities so that people with a common interest can communicate with each other by sending mail to a particular address (one address per Listserv list), which then redistributes the mail to each person "subscribed" to the list. Each person subscribed to a particular list gets a copy of each piece of mail in their mailbox.

*** What is Netnews?

Netnews is a bulletin board system in which articles on a variety of topics are arranged in "newsgroups" and stored in a shared location from which individual users can read them. These newsgroups can be local newsgroups, available only at a user's site, or may be shared with other sites to form a world-wide bulletin board system called Usenet.

*** What does the gateway do?

The gateway software (which is a built-in part of the VM/CMS Netnews server from Penn State) puts each piece of Listserv mail for a particular list into a corresponding Netnews newsgroup and also sends each Netnews-originating posting to the newsgroup back to the Listserv list. On a per-group basis, the gateway can be either bi-directional or can be uni-directional in either direction. Generally Listserv groups on Netnews are given the name bit.listserv.<listname>. When appropriate, the items in a Listserv list might also be cross-posted to a "mainstream" Usenet group.

*** Why have a gateway?

The major reason sites decide to carry gatewayed Listserv lists is so their users can read these lists via shared disks, rather than requiring each user to receive each item to their mailbox. In addition, the gateway gives greater exposure and wider readership to the list since the list can be now be read by users at hundreds of Usenet sites.

*** Setting up the gateway.

The gateway is set up by having the Netnews service machine subscribe to the Listserv list in the same way that a subscriber would, but with the addition of setting Listserv options FULLHDR (so that message-ids are put on messages) and NOFILES (so that non-mail files are not sent).

*** Guidelines for establishing a Listserv/Netnews gateway.

It is the responsibility of the person requesting the gateway to do the following:

1. Get the approval of the Listserv list owner(s) and the Listserv administrator at the host node. Send them each a copy of this document. If the list owner or Listserv host administrator objects, the gateway is not done.

2. Get the approval of the Listserv list readers. This could be done somewhat informally by posting to the list and asking if there are objections. If the issue was controversial, a formal vote should be held according to the same guidelines as for creating a new Usenet group. If there was no major dissenting opinion a vote will not be needed.

3. If there is a Usenet group where crossposting would be logical, get the approval of the people who read that group (in the same way as approval of the Listserv readers was gotten).

4. Post to bit.admin to see if there are any objections. The subject of the posting should be "Gateway for <listname> under discussion". Explain briefly what the list is for. If you are proposing that the list be gated to something besides "bit.listserv.<listname>", this should be stated. Again, if there was no major dissenting opinion within seven days, a vote would not be needed; otherwise a vote would be held.

   Steps 2, 3, and 4 can all be done at the same time.

5. Write to news-admin@auvm.american.edu or NEWS-ADM@AUVM.BITNET to say that all of the criteria have been met. Indicate the gateway site (if you wish some site besides auvm.american.edu to be the gateway), Listserv list name, Listserv host node, list owner(s), and a short (45 character maximum) description of the list.

News-admin will establish and/or register the gateway, and then post to bit.admin to say that the gateway is operational. The subject of the posting will be "Gateway for <listname> operational".

If for some reason you cannot follow the steps outlined here (for example, if you do not get the bit groups, so cannot post to bit.admin), write to news-admin@auvm.american.edu to explain the situation.

*** Where to address questions

Questions about Listserv/Netnews gateways can be posted to bit.admin or sent to news-admin@auvm.american.edu or NEWS-ADM@AUVM.BITNET.

Date: Wed Apr 15, 1992 1:24 pm PST
Subject: Re: Gateway to NetNews (Usenet)

[Martin Taylor 920415 17:00]

Gary again proposes to link CSG-L to UseNet. I still don't think it a good idea, for the same old reasons, but I'm willing to give it a go if there is strong support from other CSGers. (The old reasons boil down to a dilution of sensible discussion and a tendency for antagonistic people to dominate discussions). It is indeed a good idea for the ideas to be widely propagated, and maybe a one-way link would be better than no link. People wanting to participate could always ask to be connected to the mailing list.
Martin

Date: Wed Apr 15, 1992 1:43 pm PST
Subject: NetNews (Usenet) Link

[from Gary Cziko 920415.1622]

Martin Taylor 920415 17:00 said:

>It is indeed a good idea for the ideas to be widely propagated,
>and maybe a one-way link would be better than no link. People wanting to
>participate could always ask to be connected to the mailing list.

I am proposing to start off with a two-way link. We can switch to a one-way link if a problem develops, as Jim McIntosh suggests below. (McIntosh sets up the links as requested; we would be a "bit.*.*" newsgroup).--Gary
==

[from Jim McInstosh <jim@american.edu>]

The estimates I've seen are that UseNet reaches 20,000 hosts, which a possible readership of up to 5,000,000. The bit.** newsgroups are carried at a subset of these sites, but the larger sites are more likely to carry them. I would guess perhaps two or three thousand sites carry the bit.** newsgroups, and the potential readership approaches one million.

What does this mean in terms of mail volume? There are over 1200 news groups in Usenet, so most people only read the ones that cover areas they are interested in. Of these, more read than post. The lists that I read and that I've established gateways for usually show an increase in three or four posters. There is no way to tell how many more people are reading the posts.

A potential problem is that UseNet is fairly uncontrolled. Unlike a subscriber who gets out of control, it would be impossible to limit posts coming through the gateway. Although this has never been a problem on a list I read, I understand there have been a couple of problems of this nature in other newsgroups.

What I would suggest, if this happened, would be to make the gateway unidirectional until the person learns that he is being ignored, and then restore it to full function once he or she goes away. I am willing to assist in any way I can, so if you want the gateway for a while to try it out, or decide for whatever reason that you want it stopped, just let me know.

Date: Wed Apr 15, 1992 8:30 pm PST
Subject: NetNews gateway.

[From Joe Lubin (920415.2200)]

Gary Cziko 920415.1530 --

> I propose that CSGnet (CSG-L) be "gatewayed" globally to NetNews

This seems like a bad idea to me. This group is such high quality, it would be tragic to dilute it. Some of the characteristics that I think would suffer include: mutual respect, conciseness (beleive it or not), high-level of information exchange, etc.

If a one-way gate was used the rest of the world could benefit from the wisdom that is regularly tossed around without being able to dilute the exchanges. Requiring a user to commit to the group in some nontrivial way before enabling write access seems essential to me. Too many cybernauts out there spend too much time responding with fire to a few lines of a message that can't be understood without a significant committment to the subject matter. The bulk of CSG-L members are heavily committed to every nuance of what goes past; this level of sincerity must be maintained. Also, the mindnumbing amount of work and brilliance that Bill Powers shares on a semi-daily basis should not be trod upon by stampedes of loose-fingered geeks. As it stands CSG-L is one of my most precious resources -- allowing write access to the world would be akin to asking a bunch of street artists to spray paint "Fuck" on my son.

Joseph Lubin jmlubin@phoenix.princeton.edu
Civil Eng. Dept. 609-683-5301
Princeton University 609-258-4598
Princeton NJ 08544

Date: Thu Apr 16, 1992 4:41 am PST
Subject: NetNews gateway

Note that if we do open on NetNews as a read-only forum, Gary or someone will have to post at frequent intervals some information about how to subscribe to csg-l as a first-class citizen.

Seems to me this is a variant of the same conflict manifested in the SR vs CT disucssion: desire to promulgate CT vs. desire to get on with it in a constructive and focused way. I imagine this conflict surfaces regularly for you, Bill, between modelling and discoursing about the basics yet again.

One way to go up a level might be to apply and test our notions about social "control" and influence, using these problems as our laboratory. Can we fish find out about water? (As opposed to eddies, currents, and fin thrusts.) For example, are we able to demonstrate HPCT-based conflict resolution if J. Random Obnox drops in? That could happen on CSG-L as now constituted, though it is easier with a Usenet link.

Thank you, Bill, for that excellent exposition of the process of creating and working with models. Something I expect to return to repeatedly for insight and inspiration.

I'll be back when I get through this tangle of intersecting deadlines. Hand me that machete there, wouldja?

Bruce Nevin
bn@bbn.com

Date: Thu Apr 16, 1992 6:03 am PST
Subject: CSG Book Publishing

From Greg Williams (920416)

Report on Control Systems Group Book Publishing

Available now:

LIVING CONTROL SYSTEMS II
SELECTED PAPERS OF WILLIAM T. POWERS
$22.00 postpaid

"Powers turned the millennia-old idea that living systems act to produce intended perceptions into a formal theory of behavior: perceptual control theory. Perceptual control theory identifies behavior as the necessarily variable means by which organisms control their perceptions of the world. Working first on a built-it-yourself computer, then on a first-generation IBM personal computer, Powers created elegant demonstrations in which the simple-idea-turned-formal-model generates remarkably accurate quantitative simulations and predictions of behavior and its consequences. He identified a first principle for behavioral, social, and life sciences and showed the way to a new foundation of theory and method."

- from the Foreword by W. Thomas Bourbon

All of the papers in this second collection of writings by William T. Powers are published here for the first time. Their underlying theme, developed over three decades, is control theory's potential for revolutionizing the life sciences, particularly psychology. Powers critiques the theories of mainstream behavioral scientists, showing how their defects are avoided by applying control theory, instead. He also demonstrates the necessity for constructing truly GENERATIVE models if a genuine science of living control systems is to be developed.

Available soon:

MIND READINGS: EXPERIMENTAL STUDIES OF PURPOSE
By Richard S. Marken
price not yet set

Includes 12 previously published papers, a newly written Introduction, and a Foreword by William T. Powers [hint, hint, Bill!]. (We hope to send the proof copy to Rick next week for final checking, and publish by the end of this month or early in May.)

Still available:

LIVING CONTROL SYSTEMS
SELECTED PAPERS OF WILLIAM T. POWERS
$16.50 postpaid

INTRODUCTION TO MODERN PSYCHOLOGY: THE CONTROL-THEORY VIEW
Edited by Richard J. Robertson and William T. Powers
$25.00 postpaid

Order from:
CSG Book Publishing, 460 Black Lick Rd., Gravel Switch, KY 40328 U.S.A. (phone 606-332-7606)
Kentucky residents should add state sales tax.
Quantity discounts are available.

P.S. CLOSED LOOP, Volume 2, Number 2, Spring 1992, is expected to be mailed, with the CSG NEWSLETTER (including information about the August meeting) this week.

Greg Williams

Date: Thu Apr 16, 1992 6:09 am PST
Subject: NEYNEWS GATEWAY

FROM CHUCK TUCKER 920416.09:07

I would really hate to see this List become cluttered with posts asking a bunch of inane and silly questions like: What are you talking about on this list? I have noted (as have others) that our List is quite different in quality and quantity (yes, there are list with greater volume, like Active-l) than others that I belong to; I would not like to lose that character. If what you propose, Gary, can be done without losing the present features of the List, fine - but I would be surprised if you could promise that - it would take brute force to get it done. Remember, control theorists don't favor the control of others but rather self-control (which is all there is, right).

Regards, Chuck

Date: Thu Apr 16, 1992 7:35 am PST

(from Dick Robertson) Does anybody know of any catalog of most commonly used sentences in various languages, like the lists of most common words? I'm most interested in French right now, because of my own struggles with it while lec-turing on PCT here in Belgium, but I wonder if it might not be a reasonable mod
e of learning in any language. I have been talking with a linguist here, Peter
Kelley, who is in charge of teaching English to science majors at Un. Notre Dame, in Namur, Bel. who made a manual based on whole sentence learning that seems to get good results. He has shown a lot of interest in PCT too, having decided early on that S-R doesn't work in his field.
Reason I got interested in the whole sentence approach came from noticing that that is the way you get it chunked in conversations. I couldn't follow a con-versation even when I knew all the separate words because of the time it takes to assemble them. Thanks for any comments & info on this topic, Dick Robertson

Date: Thu Apr 16, 1992 8:32 am PST
Subject: Adaptive Control

[from Gary Cziko 920416.0830]
Rick Marken (920415 08:00) said:

>This makes me think that gain control systems probably had to emerge
>very early in the evolution of living control systems. They must
>be an very important aspect of our own nature.

Rick, since you brought this up, this might be a good time to say something about "adaptive control." It seems the engineers use this term a lot. Is adaptive control what you are talking about--varying internal loop gain to compensate for variations in the environmental loop gain (this latter can be less than one, I suppose?). Or does adaptive control refer to a lot more than just varying loop gain, perhaps all kinds of filtering and leaky integrating and other fancy stuff that I have only pretty vague ideas about.--Gary

Date: Thu Apr 16, 1992 8:32 am PST
Subject: Two-Way or One-

[from Gary Cziko 920416.1022]

Taylor, Lansky, and Lubin favor (or at least prefer) a one-way (read-only) access to CSGnet via NetNews (Usenet).

It may turn out that this will be the best way to go. This way anyone with access to NetNews could read CSGnet but could not post unless they subscribed.

But we could change to one-way at ANYTIME. So why should we start off this way? Why deny access to many people who may profit from and make valuable contributions to CSGnet for fear that they may be some jerk out there with electronic spray paint just waiting for CSGnet to show up in the NetNews list so he can spray us all with obscenities? Here we are in a situation where we don't have to predict what will happen because the feedback will allow us to adjust the accessibility of CSGnet as necessary. It surprises me that on this network I am having to argue here to use feedback and not feedforward!

Museums remain open to the general public in spite of the fact that every 50 years or so someone tries to improve Michelango's sculptures with a hammer. The risk we run doesn't even compare, and no permanent damage can be inflicted.

I have seen no convincing arguments why we shouldn't give two-access to CSGnet via NetNews (Usenet) a try.--Gary

Date: Thu Apr 16, 1992 8:40 am PST
Subject: NETNEWS FEED

>Alan E. Scrivner (920416:0825)

Based on the following experiences, I vote for at most a one-way channel.

For the past several months, I have had essentially a read-only channel out of CSG-L. This due largely to our crippled mail system (which I have been told is fixed). This has helped me greatly by tempering my thoughts and forcing me to think out what would have been my responses, rather than acting them out and destroying your brilliant discussions. Often the light bulb came on a few posts later in a response from another CSG member. As getting mail out was difficult, I choose to send letters to the listserver so I could get past discussions, rather than attempt to enter discussions, the thread of which would have passed by the time my mail ever got off site.

This has served to deepen my understanding and appreciation of control theory and find novel uses for it in both software and hardware, building what my soon to be two year old calls Daddy's ROWDOT (Robot). (Yep, Greg. I'm working on those Ants I told you about). Midnight Engineering is fun and serves to strengthen my understanding of PCT. Hopefully I'll have something interesting to report in the near term.

Alan

P.S. Our technical library here was giving away what they considered to be "Useless" books. I managed to latch onto a 1948 copy of Norbert Wiener's _Cybernetics_. One person's junk is another person's Gold!

=========+=========+=========+=========+=========+=========+=========+=========
=+

Alan E. Scrivner ms54aes@mercury.nwac.sea06.navy.mil

There was a footpath leading across fields to New Southgate,
and I used to go there alone to watch the sunset and
contemplate suicide. I did not, however, commit suicide,
because I wished to know more mathematics.
Bertrand Russell

Date: Thu Apr 16, 1992 8:42 am PST
Subject: Re: NETNEWS GATEWAY

[from Gary Cziko 920416.1050]

CHUCK TUCKER 920416.09:07 states:

>If what you propose, Gary, can be done without
> losing the present features of
>the List, fine - but I would be surprised if you could promise that - it would
>take brute force to get it done.

You are going to surprised, Chuck, because I can easily promise that CSGnet would retain its current positive features because I want to keep them, too, and all I need to do is change the link to one-way and, voila, no more problem. But I cannot promise that there will not be a minor disturbance or two along the way (remember, control systems need disturbances for them to earn their keep). And it will not require brute force. Just changing the link to one-way--IF a problem develops.

--Gary

Date: Thu Apr 16, 1992 8:44 am PST
Subject: Re: Adaptive Control

>[from Gary Cziko 920416.0830]
>
>Rick, since you brought this up, this might be a good time to say something
>about "adaptive control." It seems the engineers use this term a lot. Is
>adaptive control what you are talking about--varying internal loop gain to
>compensate for variations in the environmental loop gain (this latter can
>be less than one, I suppose?). Or does adaptive control refer to a lot
>more than just varying loop gain, perhaps all kinds of filtering and leaky
>integrating and other fancy stuff that I have only pretty vague ideas
>about.--Gary

The distinction that is normally made between deterministic and adaptive control is that deterministic control uses only existing knowledge, while adaptive control involves continuing collection of information and modification of the control strategy in light of new information.

A classical example is delivering a bomb to a foreign country. An ICBM uses deterministic control and is fully programmed from the moment of firing. A piloted bomber can monitor weather conditions and modify the trajectory to correct for winds and other factors.

The tradeoff is that information collection and use adds to the complexity of the system. Feedback isn't free!

Bill
--
Bill Silvert at the Bedford Institute of Oceanography
P. O. Box 1006, Dartmouth, Nova Scotia, CANADA B2Y 4A2
InterNet Address: bill@biome.bio.dfo.ca

Date: Thu Apr 16, 1992 9:27 am PST
Subject: Re: Two-Way or One-

The argument you give is equally good for starting with one way with the first message being you can contact X to get in to the net. Or make that part of any one way message. Seems to me less risky to start with one way.
len

<< Leonard M. Lansky Internet: Lansky@UCBEH.SAN.UC.EDU or >>
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<< FAX: (513)556-1904 >>

Date: Thu Apr 16, 1992 10:05 am PST
Subject: Gateway

[From Bill Powers (920416.1030)]

Rick Marken called today to see if I'm all right. I'm all right. I'm just pushing to get this arm model finished, and the way I work (trial and mostly error) I need long gobs of time without interruptions to get anywhere. So not much gets sent to CSGnet.

I vote for the one-way (out) NetNews connection. CSG-L is for people who seriously want to learn more about PCT or who already know a lot about it. I don't mind answering elementary questions as many times as they're asked, but I echo Chuck Tucker's distaste for inane -- uninformed and self-important -- comments and questions. Mostly I want to avoid getting tangled up with people who think they're so brilliant that they can jump into the middle of a conversation without knowing what it's about -- dilletantes. Dilletantes killed cybernetics almost at birth.

Bruce Nevin is quite right about the conflict between promulgation and getting on with the work. I decided some time ago, however, that the teaching part is more important to me, even if it means I don't get around to some of the projects that would be very nice to do. Obviously I can't follow up every idea that comes to mind and even if I did nobody else would be learning how to do the same thing.

Bruce also mentioned and I agree that if we go one-way, we should frequently post a piece of boiler-plate saying that when you feel you've learned enough about the basics of PCT to ask real questions and/or make contributions, you're welcome to join CSG-L in the usual way. It should also be mentioned that we welcome dissenters and critics if they're willing to get substantive AND KNOW WHAT THEY'RE DISSENTING ABOUT OR CRITICIZING. The only real criterion for membership on CSG-L (as in the CSG itself) is a serious interest in control theory, and this criterion merely defines who is likely to get something out of membership and to feel accepted in the group. The CSG is non-exclusive and non-snobbish.

It's crossed my mind that a "learner's list" might be appropriate. We wouldn't all have to monitor it all the time, but if the CSG-L subscribers

looked at it once a week or so at random and took it on themselves to answer questions they feel competent to answer, we might bring more people into the circle in an atmosphere where they would feel freer to ask for help. There has to be a place for people who want to ask questions like "what's feedback?" or even just "What the heck are you guys talking about?"

Speaking of you guys, thanks for the nice remarks. I don't need a lot of that, but a little love really hits the spot. Ditto to you.

Mary's announcement of the next CSG meeting will be out soon; I'll post it next week. I hope that some of the active contributors (and listeners) on the net who haven't been to previous meetings (as well as those who have) will plan now to attend. These meetings are not like any other scientific meeting you have ever yawned through. The only yawning is done by those who stayed up all night in bull sessions.

Best to all,

Bill P.

Date: Thu Apr 16, 1992 10:30 am PST
Subject: Comments on NewsNet

From Bill Cunningham :920416.1204:

1. Evangelism is a value expressed by a number of net members. Do we control for evangelism or for the avoidance of evangelism's penalties?

2. The quality of committment and discussion are recently expressed values, mainly in opposition to NewsNet and some of its undesireable features. Do we control for this quality? How would we react to a disturbance? Suppose Rand M. Nuisance or Otto Bounds joined CSG-L and became disruptive. Would this disturbance differ from one inserted by NewsNet.

3. Initial postings in the Beerbug discussion looked a lot like restoration of status quo ante, following a disturbance. That got sorted out with restoration of values expressed above. In fact, it would appear we control for both evangelism and quality, seeking a balance of the two.

4. Evangelism, like teaching, involves introducing new ideas to others. If the idea is new, we should expect it to disturb whatever they control for. That means we seek their reorganization so they control themselves. (No violence.) I think that means we have to put up with some questions that seem silly/stupid to many of the old timers, but not to the asker. Some of this might be forestalled by a good introductory package available at the push of a button. The ratio of contributors to readers in CSG-L suggests this is a pretty intimidating forum to join. Do we control for that?

5. Bottom line: Is there a TEST to determine what we control for?

Bill C.

P.S. This net IS addictive, as somebody mentioned. I'd not like to see it ruined. However, I retain control over my delete key.

Date: Thu Apr 16, 1992 11:24 am PST
Subject: re NetNews (Usenet) Link

[re: Gary Cziko 920415.1530]

Usenet sounds like a good vehicle for PCT to a wider audience. I agree with Martin and Joe that we should begin with one-way (read only) access, permitting write access only to those folks who have requested to be on the mail list or met some other such criteria.

Date: Thu Apr 16, 1992 12:22 pm PST
Subject: NetNews (Usenet)

[from Gary Cziko 920416.1430]

Wow, I must be a pretty lousy convincer. Nobody yet out there has unambiguously come to my aid in supporting a two-way link to Usenet.

I don't want to see any more time spent on this, so I will concede defeat and pursue a one-way link, unless somebody out there wants to start arguing for the two-way link.

The only change anybody on the net will see will be a periodic (probably monthly) standard post describing CSGnet and how to subscribe. You can just toss it if you've seen it already.

Let's get back to what CSGnet is supposed to be for.--Gary

Date: Thu Apr 16, 1992 1:20 pm PST
Subject: NewsNet, Adaptive control

[From Rick Marken (920416)]

Many good points have been made about going to a NewsNet connection. I am inclined to go along with Gary's suggestion that we try the two-way connection, see what happens and act if necessary to make things better. I think that the potential gains from a 2-way connect are worth the risks. But I think the reservations expressed about the 2-way connect are well founded. It's nice to see that so many people enjoy the discussions on CSGNet as much as I do. I think this shows that we all have very similar references for the kind of interaction we like to see here -- and if we make the two-way conncetion and see something happening that constitutes a disturbance, I can imagine that we would work cooperatively and skillfully to bring the interaction back to our collective reference level.

So I vote (cautiously) for the two-way NewsNet connect.

Regards Rick

Date: Thu Apr 16, 1992 1:24 pm PST
Subject: Concession

From Bill Cunningham :920416.1645:
Gary Cziko--

I take your "concession" as more positive than that.

You have decided on a course of action and are executing. That's better than screwing with it. If that disturbs equilibrium, you will have initiated THE TEST.

Re my earlier comments, which may or may not make it onto the net, it seems to me that net responded to imagined disturbance that did not occur. Interesting PCT question--what's difference between response to imagined disturbance and response to an actual one?

Bill C.

Date: Thu Apr 16, 1992 1:44 pm PST
Subject: my vote

Gary made some good points. I vote for trial 2-way: It'll be an interesting experiment.

Joseph Lubin

Date: Thu Apr 16, 1992 1:50 pm PST
Subject: Re: Gateway

I follow the discussions on CSG-L with interest, but don't contribute. I would appreciate only a one-way link in order to keep the number of messages that are not contributing to any further development of the theory/ demo's etc but are merely questions for clarification/flames etc to a minimum (at the moment virtually zero).

Jan Talmon

Date: Thu Apr 16, 1992 6:41 pm PST
Subject: USENET VOTE ANNOUNCEMENT

[from Gary Cziko 920416.2100]

Despite my earlier concession (which I hereby unconcede), I am calling for a vote on whether we should set up a TRIAL global TWO-WAY link to Usenet (NetNews) as a "Bit**" group. Send your vote to my PERSONAL address of G-CZIKO@UIUC.EDU and NOT to CSGnet. Please put your vote in the SUBJECT HEADER of your message in the form "Yes to Usenet" or "No to Usenet." No message or commentary is needed or desired.

I am not considering the one-way option at this time, but may consider it later if the "No" votes carry.

Votes will be accepted through April 24. This announcement will probably not be repeated. See below (if you wish) for more information concerning some advantages of a two-way link.

I don't want to waste any more time discussing this now. We all have better things to do. Just send in your vote. It's quick, painless, and we'll get to see how many people out there care enough about CSGnet to cast their vote.

===

MORE DETAILS

My concession to set up a one-way Usenet link was premature.

I have now found out that in order to establish a one-way global link, I will have to cut the local two-way link that has existed for the benefit of students on this campus.

The standard student account on this campus comes with a mailbox that overflows after a day or two of moderate CSGnet traffic. At that point, all messages are returned to my mailbox and students can no longer receive any more e-mail until their mailbox is cleaned out. This is why I set up the local Usenet link. They don't get their mailboxes stuffed and I don't get all the returned messages filling up MY mailbox. While most of these students are listen only, some have made us of the two-way link to send messages. These include Joel Judd and Mark Olson.

Concerning returned messages, I get these whenever a post does not make it to its destination because of node problems. Usually there are at least one or two nodes out at any time. Sometimes more. When the latter coincides with high CSGnet traffic, I get lots of returned posts cluttering up my mailbox.

So access via Usenet is much preferable from the list manager's point of view. No returned messages. No need to subscribe or unsubscribe people or change addresses. It also much more efficient in terms of computer resources (both network and personal). And also much preferable for those with Usenet access who have small electronic mailboxes.

At this point I am not willing to abandon the local two-way link for a global one-way link. I am therefore calling for a vote for either a trial two-way link (YES) or no Usenet link at all (NO). If we decide on the trial link, I will conduct another vote (if anyone wants one) after no more than one month has passed with the two-way link.--Gary

----

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Date: Fri Apr 17, 1992 8:14 am PST
Subject: what lapses?

[From: Bruce Nevin (Fri 920417 08:11:31)]

Little snippets of time are popping up between checking and printing files for a final ms. See how the appearance of continuity emerges from a discontinuous world.

Just saw a review of Daniel Dennet, _Consciousness Explained_ (reviewed by David Cohen, "psychologist and film maker," in _New Scientist_ for 28 March, p. 47).

Dennett argues that the "stream of consciousness" metaphor due to James is misleading because consciousness appears to be sporadic and patchy. It seems that either Dennett or Cohen (the reviewer) overlooks the distinction between consciousness and attention, which we have found useful here.

This does not vitiate his general view that consciousness must have been a late evolutionary development in a brain not "designed" for consciousness

> . . . let alone consciousness of self. These were late by-products of evolution. Instead, the human brain is designed to spot food and danger. Its priorities are the four Fs--feed, fight, flee and mate--and not a fifth, to fictionalize.

He argues that consciousness emerged sufficiently far into the incipient stages of language for the following hypothetical scenario:

> A crucial moment came when a hominid uttered one of these signals, thinking there was another hominid nearby. But there wasn't. That lonely hominid was the first person to talk to him or herself. At first, she didn't understand herself, but, instead of at once going into proto-therapy, she persisted with the habit and, slowly, became conscious.

Of course, when an oriole chirrups a feed, fight, flee, or f!ck signal in the expectation of another oriole being present, and finds no other present, consciousness does not emerge. Still, there is something satisfying about the notion that the reflexivity of awareness (and attendant paradoxes) is due to the reflexivity of language as used in the stories we tell ourselves. The illusion of continuity and permanence is in the story.

Whereby may depend the motivation for evolving this capacity for story telling. Control systems control continuously (except when they do so by sampling, and that may be only in parallel with other ECSs that control in a continuous way, as in visual vs. kinesthetic systems in the new arm-pointing model). Discontinuity of sensory input to an ECS is OK (no error) only with corresponding discontinuity of reference input, which in turn is represented by a continuous signal (perception of continuity and permanence in the perceptual world) on a higher level. The character of continuous control as an attribute of control systems imposes continuity and permanence as apparent attributes of the perceptual world.

Close attention to perceptions (e.g. the vipassana meditation described, taught, and practiced by various Buddhist traditions) discloses impermanence and discontinuity at lower levels of perception, masked by the continuity of perceptions at higher levels. The direct experience of aniccha or impermanence plays an important role in the progress of one following this path. From it arises the realization that there is no permanent self or ego, and with the dissolution of that fundamental construct comes what is called enlightenment, or an initial stage of it.

I would suggest that one of the "priorities" of the brain (at least the mammalian brain) is precisely to "fictionalize," in the sense that higher levels of control in a sense substitute the perception of continuous constructs for discontinuous lower-level perceptions.

If a part (glimpse of yellow, smell of banana) can be taken for the whole (I'm going to get that banana before someone else finds it), it is no large step to taking an exemplar for a set (maybe there's a banana grove nearby), and taking an exemplar for a class (banana peel--somebody found food). It seems to me that associations such as these are the first steps to symbolization and symbol manipulation. The stories we tell ourselves and others represent perceptions of continuous, stable constructs over the discontinuities and instabilities of lower-level perceptions. By "represent" we mean, I think, this associative process of taking a perception which happens to be handy in some sense as the representative of other perceptions that are less so. Fold a finger every time a deer comes through the opening. Cut a notch in the stick for every full moon since the salmon run. Let x=distance and y=time.

This accounts for the curious disproportion between "objective events" and subjects' awareness of them.

> Dennett argues introspection still has its uses but it fails particularly when we attempt to ascribe a time to the brain's activities. We cannot say precisely when awareness of a perceived stimulus emerges. For example, trying to pinpoint the moment when you are aware that you have seen a red light ... lead[s] some experimenters to the conclusion that you become aware of the light before it was switched on. Dennett proposes a model in which events and awareness of them are matched in a rough fashion as, for example, a soundtrack is matched to a film. It is counterintuitive, but understanding timing is crucial.
>
> Dennett reviews the evidence showing that there is no precise instant when the brain becomes aware of a stimulus. . . . [Hence, no humunculus is possible.] Dennett reviews research on speech production. . . . The experimental facts do not fit a theory which posits a "central meaner" who decides what "I think" and then orders the mouth to utter the desired words. The novelist E.M. Forster was there long ago when he sniped "How do I know what I think until I see what I say?"
>
> . . . In an intriguing suggestion, Dennett points out that the brain may be a parallel processor but our experience of consciousness is anything but parallel: it is serial. We are conscious of one thing or experience after another.

The review concludes with the suggestion (Dennett, Ornstein, John Rowan, and now David Lodge's novel _Nice Work_) that personality, self, ego, is a tissue of narrative discourse. I would say rather such a narrative discourse would be a representation of something equally artifactual, namely, perception on higher levels and their relationships to perceptions on lower levels. But who can say?

Bruce Nevin
bn@bbn.com

Date: Fri Apr 17, 1992 10:05 am PST
From: Dag Forssell / MCI ID: 474-2580

TO: csg (Ems)
EMS: INTERNET / MCI ID: 376-5414
MBX: CSG-L@VMD.CSO.UIUC.EDU
Subject: Starter document
Message-Id: 32920417180523/0004742580NA1EM

As best I understand, Gary has been sending a packet of introductory papers to new subscribers to CSG-L.

I am aware that any subscriber can request a file with a weeks worth of CSGnet by sending a message to Listserv.

To meet the conflicting objectives of:
a) maintain quality, conciseness and courtesy
b) disseminate information to anyone interested
c) welcome people who know nothing about PCT, but may be interested
I would like to propose / ask:

Can we assemble a "Starter document" which can be available directly from list server without Gary's active involvement. What should a starter document contain?

This document would be mentioned and instructions on how to download would be mentioned in a weekly "boiler plate" message.

The boiler plate should also contain a short statement about what PCT is, along with the standards and objectives for CSGnet.

How about one of those standards being a request for any correspondent to preface any question by stating:

My professional interest (reason for writing) is:
I have read "Starter document" and thought about it.
I have studied the following references:
I have read CSGnet for (at least a month) time.

(A standard I would like to propose is that when someone has asked an open question and a CSGnet member answers, the answer is acknowledged with a comment on its clarity (being understood) being valid and addressing the question asked. When I answer questions, I do so both to answer and to test my own understanding and my ability to spell it out.

To spend time to answer a question someone asked with apparent sincerity and then hear nothing makes you wonder if you were considered lucid, obnoxious, off the mark or unintelligible. It does nothing for your own growth to hear nothing at all. We work with all kinds of feedback on this net).

Questions by people who "just listen in" and do not bother to read and ponder our "starter document" can and should be ignored by all. This way we exercise "social control" of the kind that Bill has described.

The content of our starter document becomes important. Length, lofty visions with revolutionary zeal and down to earth explanations /definitions of feedback have to be carefully balanced. References carefully considered. Perhaps it would be sensible to have two. An introductory one to spell out what this is about. A second one for those who like the first one. The first one can be 5 pages. The second 40.

Perhaps the net would benefit from a discussion about these matters, regardless of the outcome of our vote on Usenet.

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Date: Fri Apr 17, 1992 10:06 am PST
From: Dag Forssell / MCI ID: 474-2580

TO: Gary   (Ems)
          EMS: INTERNET / MCI ID: 376-5414
          MBX: G-CZIKO@UIUC.EDU
Subject: Yes to Usenet
Message-Id: 95920417180659/0004742580NA1EM

Date: Fri Apr 17, 1992 10:38 am PST
Subject: Miscellaneous

from Ed Ford (920417.11:30)

Rick - From President "Mundane" Ford

For newcomers to the CSGnet - Our annual conference is Wednesday, July 29th through Sunday, August 2nd in Durango. More on that from Mary soon. As in the past, talks, sessions, demos, etc. are scheduled Wednesday evening. What is customary is a 20 minute talk followed by lengthy discussions. If the presenter wants to talk longer, our custom is to let her/him write a paper that can be read before hand. The key to our various presentations is group discussion and interaction. Our presentations are in the morning and after the evening meal. Afternoons are for informal get-togethers, around demo on computers, etc.(or for taking a nap) I got Closed Loop from Greg yesterday and it is at the printers. Mary's portion is in the SnailMail. The newsletter should be in the mail by the 22nd at the latest.

Gary - I vote for 2-way with you in control. You seem to have a good sense about how to deal with that project. I also vote for a gold star for you for working all of us through this discussion. Your continued mothering of us netters is much appreciated. Also, I would like a copy of your accepted article.

Dick Robertson - Got your note. Can't be of much help to your student friends. Right now I'm almost exclusively working in corrections/schools/treatment center areas. Arizona State U's School of Social Work dropped me from their schedule both this spring & next fall (I teach there this summer) so my guess is my time there will be limited to summers only and perhaps not even that. I suspect control theory overwhelmed them too much during some student oral presentations. Thus, not much help there as well. Life goes on.

Joel - congratulations.............

Ed Ford ATEDF@ASUVM.INRE.ASU.EDU
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Date: Fri Apr 17, 1992 12:15 pm PST
Subject: CT cleans up a fuzzy act

[From: Bruce Nevin (Fri 920417 15:31:32)]

A little more waiting for printers and between telephone calls. Sarah and the kids are coming about 4:30 for the long drive down to her mother's in NY for the Easter weekend.

Musing on some Japanese applications of fuzzy logic in video cameras, microwave ovens, and now a vacuum cleaner puts me in mind of possibilities for hierarchical control systems on a chip. Could be a source of funding--the CSG could clean up!

A blip in the March 28 _New Scientist_ (p. 28, the Patents page) describes a vacuum cleaner from Matsushita that shines a light beam through the incoming stream of air and dirt onto a sensor which it is said estimates the quantity of dirt per second, how light it is, and whether it is evenly spread or in patches. The fuzzy logic processor then adjusts the suction power purportedly to suit the type of dust, its depth, and its distribution. Blinking LEDs impress the user with how hard the vacuum cleaner is thinking.

What sensors and ECSs would be required to do this job? Seems like you would want to sense if dirt remains on the floor and increase suction enough to pick it up. Motion detector? What about rugs? Experimentally pulse harder if nothing detected in the air stream? Would that wear the moving parts out sooner and contravene a mechanical design imperative? If you were a vacuum machine, what perceptions would you need to control to satisfy the perceptions that the user is controlling? (Telling the user how hard you're working is not at all a bad idea, so long as the floor is getting clean in the bargain.)

Enjoy the weekend. We had snow last night, but it's all washed away during a day of rain.

Be well,

Bruce Nevin
bn@bbn.com

Date: Fri Apr 17, 1992 2:05 pm PST
Subject: Gateway; Testing Models

[From Bill Powers (920417.1100)]

How interesting. Bill Cunningham and Gary Cziko have tipped the balance, and I now vote for a two-way NewsNet connection. Gary's practical difficulty introduces a new controlled variable, and Bill's comment on imaginary disturbances was unsettling. I think the imaginary disturbances got to me the most. An imaginary disturbance is part of a mental model, isn't it? So I was faced with my mental model of all those unknown others out there to whom we would be opening the floodgates. In this case, when I looked at my mental model, I found a big red stamp across it: PREJUDICE. Open the doors, sez I.

Ed Ford mentioned his monthly meetings with people interested in using control theory in the real world. In a phone call, he asked me to say something that would help his group "make models." I replied that what his group can do is probably better called "testing models," so that is what I'm writing about here. I'll digress at the start to introduce some background on the concept of prediction. The first part of this development is intended to amuse experimenters; the second part gets to practical matters.

On testing models:

Part 1.

Some time ago I remarked that the most common model in psychology is a cause-effect model in the form of a regression equation. The hypothesis is that the effect depends on the cause linearly, as in y = ax + b. To test this model, you'd take the values of a and b determined from a formal study, and try to predict new values of y from new observations of values of x.

David Goldstein commented that this concept of using a model for predictions is not the way such findings are used in psychology. Once the regression line is drawn through the data points, that's the end of it. The model equation describes the data, but isn't then used for predictions.

On thinking this over, I agree that no formal use is generally made of the regression equation, but the findings are certainly used to predict individual behavior. Suppose the dependent variable y is a clinical measure of depression, and the independent variable x is a depression-factor score on a personality test. In computing the correlation between the test score and the clinical measure (in a study of many people), a regression equation of the form y = ax + b is the basic premise behind the correlation calculation. If the correlation is positive and statistically significant, the conclusion drawn is that depression is predicted by the test score. Then the test is administered to a new individual (presumably from the same population), and if the depression-factor score is high, the person is diagnosed as depressed.

This isn't a formal application of the regression equation: you don't say that a test score of exactly 7 predicts a depression of exactly 25 units on the clinical scale, even if that's what the regression equation says. But a person who measures 15 on the test score would be judged as more depressed than a person who measures only 3. So while the slope and intercept coefficients aren't explicitly used, the general trend is implicitly used, and there are semi-quantitative judgements made.

The scatter in data of this kind is so great, of course, that literal application of the regression equation would be silly. The prediction for any individual when the correlation is as low as 0.8 would be seriously wrong most of the time, often even getting the sign of the relationship wrong for one person. The only correct way to make a prediction would be to begin with another equally large sample of the population and do the whole study again. You would predict that the same regression coefficients would be found.

But there is an urge to predict for individuals, and the form of the urge follows the regression line: a higher clinical score ought to predict a more severe depression. While it is folly to give in to this urge when the data are so bad, the motive behind doing so is consistent with the principle of modeling.

If the principle of modeling were followed through formally, the regression line would indeed be used to predict behavior. If the line has the equation y = 3x + 5, and the depression-factor test score for a new individual is 4, the model predicts that a clinical evaluation of depression will come up with 17 on the clinical scale for that person. To follow the test through, one would then submit the person to the same clinical evaluation as used in setting up the model, and see what number actually results.

Suppose the actual depression measure is 12 on the clinical scale. This is a deviation of -5 units from the value of 17 predicted from the test score, for an error of -29 percent. Is that good, or is that bad? The answer depends on how important it is to get the evaluation exactly right.

Of course in this case we know the clinical measure of depression, and if we believe it we can just ignore the test score and the prediction. But what if we want to make the diagnosis on the basis of the test score alone? Now the generally expected error for an individual prediction becomes relevant. If you're going to prescribe electroshock therapy that will most likely severely disturb the person's life for many years, maybe even permanently, you might decide that a 29 percent error is too large to allow. Perhaps even an error of 5 percent would be too large if the person is a borderline case. On the other hand, if you're going to prescribe a tranquilizer that won't do any permanent harm even if the person isn't really depressed, then perhaps you can allow errors as large as 29 percent.

I've gone through this to illustrate that prediction errors can't be judged as good or bad without taking the context into account. But what if the context is that of testing a general model of behavior? Now the actions taken as the result of a diagnosis are no longer in the picture. All we want to know is which theory is better. Now the errors of prediction under different models are judged not against practical standards, but against each other. The smaller the expected error, the better.

I have also tried to show that even in standard approaches, the method of modeling is there just beneath the surface. It's probably not mentioned much because the predictions made from literal application of the model -- the regression equation -- are so poor. But the model is there. It's that model that we have to compare against the control-theory model, and the way we do the comparison is through making quantitative predictions using the actual form of the model.

Let's look at the rubber-band experiment. Suppose we just measure the position of the experimenter's end of the rubber bands and of the subject's end, designating the positions as e and s. Let's confine the experiment to a line, so we consider only one dimension. The zero point on the line can be chosen arbitrarily, with all measurements made relative to that zero.

If we now measure the positions e and s over a long series of movements by the experimenter, we will obtain a data set consisting of pairs of values of e and s. We can do a correlation between e and s. From the normal calculations, we can derive a regression line.

The regression line will have the form s = ae + b. The position of the subject's end will depend on the position of the experimenter's end. If the rubber-bands are identical, the coefficient a will be very close to -1. Half of the intercept b will correspond to a position on the line. That position will be the average position of the ends of the rubber bands: with a = -1, we will have (s + e) = b, or (s+e)/2 = b/2.

In fact, half of the intercept b will turn out to be a position nearly underneath the knot where the rubber bands are connected. The knot, as it will turn out, remains very nearly at the position b/2 all during the experiment.

There's a moral to this story, but it's not quite obvious yet. The first part of it is that when you do an SR experiment in the usual way, to get a regression coefficient, you can SOMETIMES translate it directly into a control-system experiment. If you find that the intercept b corresponds to something in the experimental situation that's remaining nearly constant at that value, you've found a controlled variable -- actually, by finding its reference level first.

The second part of the story concerns the accuracy of the prediction. The SR prediction will be accurate only if the two rubber bands have identical characteristics, or strictly proportional characteristics. If their characteristics are different, the correlation coefficient you derive from the data corrected for the different rubber-band properties will be very much higher than the one derived from the model s = ae + b, which assumes identical rubber bands.

Part 2.

In testing the control-system model, the basic procedure is to assume that all behavior without exception is control behavior, predict behavior on that basis, compare the prediction with the appropriate data, and let the match or mismatch decide the issue. You can never prove that a particular control-system model is the only correct one, but you can show that it is incorrect.

Considering the low correlations that are found in S-R experiments, it might seem hopeless to substitute a PCT model for the linear regression model. When the data are that noisy, how can any clear decision be made? This objection, however, assumes that the SR experiment has correctly represented the data. While we can't prove that ALL SR experiments could be translated into relatively noise-free PCT experiments, there are excellent reasons to think that this can be done in a significant number of instances, maybe even most instances. To do this, however, can require some changes in viewpoint that may be hard to achieve.

An SR "fact" is expressed as an effect of a cause. Doing something to a person results in that person's doing something else. If the relationship expressed in this "fact" isn't clearcut and quantitative, then the control theorist has to start asking questions about the data.

The basic question is, what is it that was affected by the "stimulus" that was also affected by the "response?" If you utter encouraging words to someone, and that someone then shows added efforts to achieve something, you have an SR relationship. Now you have to try to guess: what did the encouraging words affect that was affected EQUALLY AND OPPOSITELY by the increased efforts?

Equally AND OPPOSITELY? There's the rub. You would like to think that there is something you said that helped this person do better. But control theory says that if your words of encouragement had some regular effect on the person's behavior (apparently), that behavior was aimed at COUNTERACTING your influence. If this is true, then you don't have the control over the person's behavior that you thought you had, even for the good. You are seeing yourself as helping the other person to do better. The other person, however, is seeing the situation differently: you're disturbing something, and the other person is acting to cancel the effect of the disturbance.

This may not be true, but if you're going to test the PCT model honestly, you have to pretend it's true and try to make sense of it. You can't test a model if you don't follow its logic faithfully and literally as far as you can. You can't look ahead and think "If PCT is right, then I haven't been helping people the way I thought I was -- so PCT must be wrong." You have to be prepared to change your ideas about anything at all. Otherwise your reasoning is just a sham.

Let me give you a real example from my high-school days. We had a coach, named Coach, who was tremendously popular, a great guy. We all loved him and wanted his approval above anything else. Coach would say "You can do better than that, I know it -- just give it one more try and you'll make it." And by golly, we'd give it one more try and we'd make it, sometimes.

Now it would seem that his encouragement and belief in us caused us to try a little harder than we thought we could, so we achieved something we couldn't do before (sometimes). I suppose that Coach looked at it that way, as any reasonable person would. But I can tell you that from inside at least one person (and at the time I guessed this was true of a lot of the others), it wasn't all that nice.

The basic problem was that Coach went around all the time saying to people, "What you're doing isn't good enough to please me." That's what "You can do better" says. I was already doing better than I thought I could, in number of pushups, speed of climbing a rope, time in the 40-yard dash, or whatever. And I was damned tired and hurting, and not necessarily interested in doing any better. I liked physics a lot better than physical education. But here's Coach telling me that he doesn't like what I'm doing. That mattered to me. So I got myself together and made it REALLY hurt, and I felt great -- because now Coach wasn't displeased with me. Not because I'd achieved something I wanted, but because I'd done something to counteract his disapproval.

From Coach's point of view, he had helped me put out that extra bit of effort to surpass my previous achievements. No doubt if I had continued to go along with this, worked out, built up a lot of strength, learned the football playbook by heart, and all of that, satisfying the coach more and more all the time, I might have achieved even more. I might have been a college football star; I might even have become a professional football player and ended up as a coach myself, by now. I might be bold, aggressive, commanding, and rich. But I certainly wouldn't be writing this. I also wouldn't be the Bill Powers you know.

What actually happened was that many of us simply gave up on pleasing Coach because we didn't buy the goal. It wasn't pleasant to do that -- to decide we were trying as hard as we cared to try toward that particular end, and that we would simply endure the disapproval. We still loved Coach, and we tried to fend off his disapproval by seeming to try harder. But the price was too high to really do it. When Coach was called into the Navy and left in 1944, there was a huge tearful farewell ceremony for him, and I'm sure that amid the sorrowful participants there were many hearts filled with relief.

To apply the PCT model, this is the sort of thing you have to think about. It's especially difficult when the hoped-for effect on a person is beneficial. There's an almost-inescapable tendency to suppose that what you think of as beneficial is also considered beneficial by the other person; that what you consider harmful is also thought harmful by the other. Coach would have been completely baffled by the present discussion. He would have said "Well, you did try harder, didn't you? And you did do something you thought you couldn't do, didn't you? What's so bad about that?"

The SR viewpoint encourages this sort of naive projection of one's own goals onto the behavior of others. I shouldn't even call it the "SR" viewpoint. It's really this viewpoint, adopted innocently by well-meaning people who have never heard of stimuli and responses, that led naturally into SR theory.

To test the PCT model in real life, you have to be prepared to follow its logic all the way. Forget about whether the "response" is good or bad. The question is how to find the controlled variable, the thing that is disturbed by what is done to the person, and is protected against more disturbance by the action that the person takes. If you find such a controlled variable, you will understand that person far better than you did before. If you want to help that person, you might even find out what he or she really wants and figure out ways that person could get there.

It's possible that you won't find any such controlled variable in a given circumstance. But if you don't look for one, you will certainly not find one even if it's there staring you in the face.

The basic message here is that to test PCT, you have to make predictions from it and from nothing else. You have to follow out the logic even when it seems to say things you don't believe. Then you have to look carefully to see whether, in fact, the prediction holds true. This requires being consciously open-minded and willing to take a chance. You simply have to trust that if the theory does predict correctly, you'll be better off knowing what it predicts than not knowing, letting the chips fall where they may.

Best to all   Bill P.

Date: Fri Apr 17, 1992 2:57 pm PST
Subject: Adaptive Control

[From Rick Marken (920417)]

I forgot to talk about adaptive control in my last post. So:

Gary Cziko (920416.0830) asks:

>Is
>adaptive control what you are talking about--varying internal loop gain to
>compensate for variations in the environmental loop gain (this latter can
>be less than one, I suppose?). Or does adaptive control refer to a lot
>more than just varying loop gain, perhaps all kinds of filtering and leaky
>integrating and other fancy stuff that I have only pretty vague ideas
>about.--Gary

I think that "adaptive control" in PCT can refer to a lot more than adjusting loop gain; so I go with your second proposition. I think of adaptive control as any situation where variable aspects of one control system (CS1) are controlled by another (CS2). The variables controlled by CS2 could be things like 1) integrated error in CS1 2) variance in output of CS1 3) variance of a variable that is affected by (but not necessarily controlled) by CS1, etc etc. The means by wheich CS2 achieves control is by varying parameters of CS1 -- input function, output amplification, allocation of CS1 outputs to lower order systems, allocation of lower level system inputs to the CS1 input function, CS1 gain, etc. Some aspects of adaptive control are delt with as special functional aspects of the hierarchical model -- reorganization is certainly adaptive control where it is intrinsic variables (influenced by the outputs of perceptual control systems) that are controlled by CS2 (which is the reorganization system). I think "imaginative" control is a type of adaptive control (planning ahead, considering alternatives,etc). Gain control is certainly an example of adaptive control. I was proposing that gain control systems are a common component of the control hierarchy; special control systems that just monitor control system stability (I don't know what they might be perceiving but this is a place where neuroanatomy might by highly suggestive) and adjust gain continuously to keep the system in ship shape.

Thanks to Greg and Ed for the info about publications and meetings (Ed, you're not mundane; you are out of this world).

Best regards Rick

Date: Fri Apr 17, 1992 5:28 pm PST
Subject: Re: USENET VOTE ANNOUNCEMENT

[Martin Taylor 920417 2130]

I sent a "No to UseNet" vote, because already 1 Meg per month is too much when most of it is serious. If we can't have a one-way link, for the reasons you mention, then we should have no link. I, for one, would probably quit reading the group, and go with personal mail to Bill. Maybe the rest of the group would feel that would be a good idea, but I would be sorry to lose the interchange that we have.

Martin

Date: Fri Apr 17, 1992 6:25 pm PST
Subject: USENET VOTE ANNOUNCEMENT

(Martin Taylor 920417 2130)

>I sent a "No to UseNet" vote, because ...

I would regret that a lot, but think that we should try out the principles we profess to be interested in, and if the results are such that Martin feels inclined to bow out, cut the link.

Avery.Andrews@anu.edu.au
(currently andrews@csli.stanford.edu)

Date: Fri Apr 17, 1992 7:51 pm PST
Subject: Re: vote yes

VOTE YES for network connection. The discussions are too closed as they stand,
and could be enriched by open access to people whose background is in `pure' control theory. Also, since control theory was introduced in domains where the notions of `current state' and `desired state' exist on well-formed metric spaces, CSG has yet to address the criticism of whether or not it is applicable in domains where it is not entirely clear that such a metric space exists (ie. problem solving, behavioural strategy selection, linguistics,
etc.)

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Date: Sat Apr 18, 1992 6:00 am PST
Subject: Dennett and consciousness

[From Bill Powers (920418.0800)]

Bruce Nevin (920417a) --
>
>Dennett argues that the "stream of consciousness" metaphor due to James
>is misleading because consciousness appears to be sporadic and patchy.
>It seems that either Dennett or Cohen (the reviewer) overlooks the
>distinction between consciousness and attention, which we have found
>useful here.

I think you're right. As far as I can tell, consciousness (awareness of something going on) is continuous during waking hours, although attention to particular things changes. There's always something in attention. Of course what I'm attending to may not be what you're attending to, so from either point of view, the other's attention is "patchy."

>This does not vitiate his general view that consciousness must have >been
a late evolutionary development in a brain not "designed" for
>consciousness ...

The stories people make up about the "dawn of consciousness" and such things tell more about their beliefs than about consciousness. Basically everyone seems to be trying to "prove" (with fairy tales) that humans are in some essential way different from animals, some of them at the same time trying to "prove" that they're the same.

Dennett is quoted as saying

>Instead, the human brain is designed to spot food and danger. Its
>priorities are the four Fs--feed, fight, flee and mate--and not a >fifth,
to fictionalize.

From what premises (they can't be facts) does Dennett draw such self-confident conclusions? Is a bird watching a piece of ground with a small hole in it not fictionalizing a worm? I don't believe Dennett or anyone else knows what the human brain is designed to do. We know some of the things it CAN do NOW, but by no means all of them. We know it can imagine experiences not occurring in present time. We can guess, roughly, at the kinds of circuitry required to imagine, or "fictionalize." I don't see a thing that forbids such circuits to exist in animal brains or prehuman brains, or even in chemical control systems. I can see a lot of things the brain does that are not covered by the four Fs, and that don't seem confined to human brains. Note that Dennett doesn't mention perceiving or controlling. In fact, his four Fs don't even talk about behaviors: they're all goals.

The following is a piece of nonsense and confusion, not to mention gratuitous imagination:

> A crucial moment came when a hominid uttered one of these signals, thinking there was another hominid nearby. But there wasn't. That lonely hominid was the first person to talk to him or herself. At first, she didn't understand herself, but, instead of at once going into proto-therapy, she persisted with the habit and, slowly, became conscious.

Notice that this hominid was "thinking there was another hominid nearby" and THEN "slowly became conscious." How did this "thinking" occur without consciousness? The identification of consciousness with language is commonly made, and is one of the main reasons people seem to pay so little attention to nonverbal happenings at high levels of organization. The use of language is a SYMPTOM, not a CAUSE, of consciousness. There are many other symptoms accessible to anyone who examines experience closely. When you look, taste, listen, and feel you are conscious, even when you're not describing the experiences to yourself or anyone else.

>Of course, when an oriole chirrups a feed, fight, flee, or f!ck signal
>in the expectation of another oriole being present, and finds no other
>present, consciousness does not emerge.

Well, I guess I can agree with that, because consciousness, quite possibly being present in all living systems, doesn't have to "emerge." My premise for that conclusion is simply the cosmological principle: that it's unlikely for us to have a special or preferred position in the universe (of animals), and we're likely to make the fewest mistakes by not assuming such a preferred position. No matter how much such an assumption would puff up our egos. It's hard, by the way, to imagine a non-conscious "expectation.

>Still, there is something satisfying about the notion that the
>reflexivity of awareness (and attendant paradoxes) is due to the
>reflexivity of language as used in the stories we tell ourselves.

What's going on here? What paradoxes? If you examine the self, you immediately find that what you're examining is NOT the self doing the examining. There's no reflexivity of awareness. You're NEVER aware of your current point of view -- only of a previous point of view. Language seems reflexive only because we can make up sentences like "This sentence is about itself." That sounds as if the sentence, all by itself, can be "about" something. It can't. We MAKE it be about something, and if we just look at it as a sentence, it ceases to be about anything, least of all itself. You have to look at the MAKER of the sentence, not the sentence, to see what this "aboutness" is about.

>Discontinuity of sensory input to an ECS is OK (no error) only with
>corresponding discontinuity of reference input, which in turn is
>represented by a continuous signal (perception of continuity and
>permanence in the perceptual world) on a higher level. The character of
>continuous control as an attribute of control systems imposes >continuity
and permanence as apparent attributes of the perceptual >world.

[Sampled control systems usually have continuous reference signals: only the perceptual function is a sample-and-hold device.]

I think this is backward. The lower levels of ECS are continuous; only the higher ones introduce sampling and discrete variables. Heck, just look around you. Is the room you're in present only intermittently? When you hear someone speaking, are the words really separated? When you attend to your actions, do they ever actually cease? Are they really packaged into "acts?" Is the world of experience really divided into categories with distinct boundaries, sequences with non-overlapping elements? I think that it's only at the category level and higher than we begin to treat the world as if it were discontinuous -- and even then, the edges are fuzzy.

>Close attention to perceptions (e.g. the vipassana meditation >described, taught, and practiced by various Buddhist traditions) >discloses impermanence and discontinuity at lower levels of perception, >masked by the continuity of perceptions at higher levels.

I think that "discloses" is the wrong word: I would say "presumes." I dispute the claim with the counterclaim that it is just the other way around. The lower levels are those we have the least choice about, and are the most continuous and seemingly permanent. It is the higher levels that represent the world as things separated into categories and occurring in discrete sequences. Even to contemplate "a flower" rather than "this" (or "That") is to categorize and discretize. Programs consist of discrete operations conducted one after the other, with abrupt transitions between states. "Impermanence" and "discontinuity" are higher-level perceptions (or imaginings), as are their opposites, "permanence" and "continuity." I think the Tao comes closer to the truth: unevaluated perception is a river that never ceases to flow. It's only when you start symbolizing and talking about it that you start to separate out the eddies as something individual. The eastern philosphers did a LOT of talking.

>The direct experience of aniccha or impermanence plays an important >role in the progress of one following this path. From it arises the >realization that there is no permanent self or ego ...

Perhaps that is one route to the realization, but it is by no means the only one or even the quickest one. The quickest one is the Method of Levels, which doesn't get into philosophizing at all. You just keep looking. The thoughts that go through your head while you're looking are irrelevant. It's the looking that counts. Just keep looking at yourself, and now and then remember to wonder "who's looking?" I don't think that categories like "impermanence" help at all. I think they're conclusions one reaches AFTER the realization comes. Of course those who haven't had the realization think that the conclusion is the means of reaching the realization. It isn't. When you put a motor on a windmill, it doesn't make the wind blow any faster.

>I would suggest that one of the "priorities" of the brain (at least the >mammalian brain) is precisely to "fictionalize," in the sense that >higher levels of control in a sense substitute the perception of >continuous constructs for discontinuous lower-level perceptions.

And I claim that you're not looking directly at the world of lower-level perceptions when you say that, but at DESCRIPTIONS of that world. The descriptions are discontinuous. The world isn't. There's no reason why the imagination connection can't exist in any brain at any level (but the lowest).

>If a part (glimpse of yellow, smell of banana) can be taken for the >whole (I'm going to get that banana before someone else finds it), it >is no large step to taking an exemplar for a set (maybe there's a >banana grove nearby), and taking an exemplar for a class (banana peel-->somebody found food). It seems to me that associations such as these >are the first steps to symbolization and symbol manipulation.

At last we agree. Any perception can be a symbol for any other perception. But a perception isn't a "part" of another one until the concept of the whole perception (a category) exists. A sensation of yellow is just a sensation of yellow, until it comes to symbolize the category named "banana." Only then could it be perceived, at a higher level, as "part of a banana."

>The stories we tell ourselves and others represent perceptions of >continuous, stable constructs over the discontinuities and >instabilities of lower-level perceptions.

But the "stories we tell" are themselves composed of discontinuous elements: words. If there's anything continuous we get out of the stories at a higher level, it must be the persistent perception of programs/strategies, principles, and system concepts. So in a sense I can see that the higher levels are more continuous than the lower -- as long as you consider the lowest levels to consist of words and the categories that they name.

Perhaps we're thinking of different kinds of continuity and discontinuity. Below the level of words we have (by my count) six levels of perception that are fundamentally continuous -- although I am looking more and more askance at the so-called "event" level. Above the level of logic or programs we have two levels that are continuous in the sense that the same thing can continue to be perceived over a collection of discrete programs, sequences, and categories (the principles themselves, however, are discontinuously distinct from each other). If you take apart exemplars of principles, you find finite programs made of finite sequences composed of discrete categories. But if you take apart the categories, you find continuous perception of a continuous world. You go from block letters to cursive handwriting, which wriggles and writhes in familiar patterns without any break between one pattern and the next into which it flows.

> Dennett argues introspection still has its uses but it fails particularly when we attempt to ascribe a time to the brain's activities. We cannot say precisely when awareness of a perceived stimulus emerges.

What Dennett fails to grasp is that it's ALL introspection. It's ALL perception by someone. If Dennett recognizes only discrete stimuli, then of course it's hard to say precisely when awareness of them emerges. Awareness was there all along, continuously, with events passing through its scope. If only discrete stimuli exist, then between them there are no perceptions in awareness. A blank. This is an imagined scenario, which one can believe only if direct experience is somehow being ignored. The only way I know of to do that is to focus exclusively on the world of words, shutting out the external world and confining reality to the realm of descriptions.

> . . . In an intriguing suggestion, Dennett points out that the brain may be a parallel processor but our experience of consciousness is anything but parallel: it is serial. We are conscious of one thing or experience after another.

To me, this indicates only the extent to which Dennett lives in a world of words. My goodness, if you could be aware of only one thing at a time, you'd be seeing only one letter at a time while you were reading this -- you wouldn't even realize that these letters are on a piece of paper or a screen, or that there are lines of letters above and below the one you're reading. In the world of words, things happen serially, one experience after another. But in the rest of the world of perception, multitudes of perceptions coexist and overlap and flow into each other, in parallel. Think of an artist, adjusting form, color, arrangement, shadows, highlights, and so on, working, to be sure, on only one tiny piece of canvas at the tip of the paintbrush but continually aware of the whole structure and all the relationships of sensory experience within it. That's not a serial world. The halfback running for daylight isn't experiencing a world in which one thing at a time happens. The conductor of an orchestra doesn't deal with one thing at a time. Dennett is so taken in by his own idea that he lets his words dictate what he notices about the world.

>The review concludes with the suggestion (Dennett, Ornstein, John >Rowan, and now David Lodge's novel _Nice Work_) that personality, self, >ego, is a tissue of narrative discourse.

What else would a person conclude who sees the world entirely as it is described in words?

Best Bill P.

Date: Sat Apr 18, 1992 9:49 am PST
Subject: Re: Starter document

[Martin Taylor 920418 13:40]
(Dag Forssell 17 Apr 92 16:53:40)

If we go to a Usenet link, as seems to be the concensus, there's no way to deal with people who don't read the "starter document". They will just be answered by others who have not read it. Words of wisdom from the likes of Bill and Rick will be treated as equally uninformed stupidities by those answerers. We will lose what makes CSG-L great--the idea that we can and do develop something that really progresses and is important--to the turbulent discussions that characterize every Usenet group that I know of (I subscribe to about 100 such, but usually only dip into them from time to time).

But Dag's idea of a 2-level starter document is a very good one, all the same. It might mitigate some of the problems. Who writes it?

Martin Taylor

Date: Sat Apr 18, 1992 10:15 am PST
Subject: Tension, Coach

[Martin Taylor 920418 13:50]

Yesterday (I don't have the exact reference) Bill wrote an interesting post about "social control" using the example of his kindly coach who induced him to try so hard that it hurt, but produced great performances--and induced him to hate the coach because of it. Bill argued that all attempts to control were just introducing disturbances that were resisted in maintaining reference percepts, and that inevitably such resistance was accompanied by resentment or other bad effects (sorry, not having Bill's message at hand, I may exaggerate, but that's what I got out of it).

I quite agree with the first half of the conclusion, but not with the second. As I have argued before, there is no technical distinction between the alteration of the error signal in an ECS by changing the reference as compared to changing the percept. Each results in a determinate error signal that results in behaviour that reduces the error (assuming a well-organized control system hierarchy). The coach played on this by assuming that Bill had a reference to be liked and admired by the coach, and causing Bill to perceive that this was not the situation, though it could be. The coach also presumably assumed Bill had another reference (shared by athletic overachievers) that he should do as well as his body would permit. Bill asserts that he did not share that reference. If he had, then the coach's behaviour would have induced percepts that caused errors with respect to each reference that the same behaviour would have satisfied. But since Bill did not have the "excellence in athletics" reference, the "hurting" behaviour helped to satisfy only the "find favour with coach" reference, and conflicted with the reference most people hold "feel good in my body."

I don't think it is necessarily true that this sort of conflict leads to resentment and bad feelings. I go back to my comments of a few weeks ago that I wanted to expand on--about the zeroing of the errors in a system totally under control. The better a system is controlling, the lower the errors within it, almost by definition. Rick pointed out that the errors don't go to zero, because of non-orthogonality within the hierarchy. To some extent, behaviour that helps reduce on error increases another. Such conflicts
are almost inevitable in a complex hierarchy, especially one in which there are fewer final degrees of freedom for control than ECSs in any one level. The human muscular system provides a good example--some 400-800 muscles (I don't know an authoritative number, but that's the range) control around 125 degrees of freedom for joints, face, voice, and so forth. There are two ways of resolving the conflict: mutual control, such as in opponent muscle pairs (one zeros its control while the other works), or tension (each tries to achieve its reference, and a balance between then is achieved).

I think that tension and conflict is desirable, if it is not overdone. It enables the control system to react promptly to changes in the perceptual situation. It is analogous to the temperature of a thermodynamic system. Zero conflict means a system perfectly organized for the disturbances the environment presently provides--the system is frozen and will not necessarily be able to respond well to new types of disturbance. Some tension means two things: the system is ready to move fast in many directions, and, equally important, it is prepared to reorganize if Bill's notion about reorganization being driven by accumulated error is correct. So a system with tension and conflict will be more robust than one that is placidly content.

The end-point of this line of thought is that we should have evolved to be happier with some level of disturbance and internal conflict different from zero than with a bland, disturbance-free environment or an environment that we have totally under control. Bill's coach was right, but perhaps went too far. Mild social control of that kind is what we like. We want to do well for other people, but we do want to find that we can reach the reference level of satisfying them without at the same time working too hard (diverging from other reference levels). I suspect that many marriage problems arise from a perception of inability to satisfy the partner despite excessive efforts (which might be in the wrong direction, demanding reorganization).

Thus: tension, conflict, and uncorrectable disturbance are good, but not in excess.

Martin

Date: Sat Apr 18, 1992 11:51 am PST
Subject: CSG Meeting Announcement; Mary comments

[from Mary Powers]
Here it is!

CONFERENCE ANNOUNCEMENT

The 8th annual meeting of

THE CONTROL SYSTEMS GROUP

July 29 - Aug 2
Fort Lewis College
Durango, Colorado

This is an interdisciplinary conference on the application of Control Theory to the behavioral, social, and life sciences.

The conference is small and informal. There will be 7 plenary sessions (Wed. evening, morning and evening Thurs., Fri., Sat.). Afternoons and Sunday morning are unscheduled, and are available for more specialized and technical one-on-one and small group meetings as interest dictates, or for rest and recreation.

While all participants share the common language of Control Theory and therefore communicate unusually well across disciplines, it is probably a good idea to keep in mind the wide diversity of fields represented when choosing discussion subjects for the plenary meetings.

Our best sessions take the form of a brief presentation of research, problems, insights, future plans, etc., followed (and frequently interrupted) by questions and open discussion. We encourage participants to bring papers for distribution (25-30 copies) which can be read by participants before any presentation in order to have an informed discussion. We discourage the reading of a long paper or a faithful reproduction of presentations to one's classes, clients, or dissertation committees! For those who need academic brownie points, a distributed paper is considered as presented. No one has to give a talk, and a number prefer simply to listen, learn, and comment.

For those with computer demos, there will be an AT and (we hope) a MAC. There will be an overhead projector with (again, we hope) a computer projection plate. There will also be a VCR and monitor.

Durango is in southwestern Colorado at an altitude of 6500 feet. It is a major vacation area, with the San Juan Mountains to the north, Anasazi Indian ruins, a narrow-gauge steam railroad, river rafting, etc. July-August weather is usually hot and dry, with afternoon rain common, and cool nights. Participants and guests can stay over at the college for up to three nights following the meeting.

Registration

The cost of the meeting is $220 for a single room, $180 for a double. This includes the $40 membership in CSG which supports the publication of Closed Loop, plus all other expenses including meals from Wednesday dinner through Sunday breakfast. Sunday lunch is extra, and so is the airport taxi ($12 each way). The student rates are $185 and $145. We can waive the fee for three students (see below).

The fee for a guest sharing a room with a participant and including all meals is $125. The charge for a guest without meals is $60, and meals can be bought on campus (breakfast $4.50, lunch $5.50, and dinner $6.25) or off-campus. The no-meals fee does include dinner Friday night - the banquet/business meeting. The charge for staying over after the meeting is $20 per night for a single person in a room, $12.50 per person sharing a room. No meals included. For more than one guest, or any other special arrangements such as tourist information, day care for children, etc. please contact Mary Powers (address below, or 303-247-7986 or at powers_w%flc@vaxf.colorado.edu (the symbol following powers is an underscore and the symbol following w is a percent sign). For those specifying in advance that they are attending half or less of the meeting, any refund from the full cost will be returned after the meeting.

Registration is $50 ($15 for students), and MUST be received by July 13. Registrations after this date CANNOT be accepted. There will be no refunds of registrations except for students applying for assistance (but membership will be paid).

The balance is due July 27. Please do not plan to pay at the meeting.

Make checks out to The Control Systems Group and send with the following form to

Mary A. Powers
73 Ridge Place
Durango CO 81301-8136

* * * * * * * * * * *

The Control Systems Group
8th Annual Meeting
July 29-August 2, 1992

NAME____

ADDRESS____

PHONE (evening)____

will attend (circle) WED(pm) THURS FRI SAT SUN(am)

will also stay (circle) SUN MON TUES nights
room: SINGLE DOUBLE (share with_____________________________)

GUEST(S) name____

____

guest meals (check) ALL__________ BANQUET ONLY___________

Banquet drink preference (iced tea provided - main course will be roast beef and vegetarian lasagna):

WINE red_____ white_____ BEER_____ POP reg_____ diet_____

Coffee break preference (majority choices to prevail)

morning: COFFEE_____ TEA hot_____ iced_____ FRUIT JUICE_____

evening: COFFEE reg_____ decaf_____ TEA hot_____ iced_____

LEMONADE_____ FRUIT JUICE_____

EQUIPMENT NEEDS:

AT computer_____ MAC ______ Overhead Projector_____________

Projection Plate___________ VCR/Monitor__________

OTHER (specify)____

STUDENTS (including 1992 graduates): I am unable to afford this conference and hereby apply for financial assistance to cover all fees except CSG membership. (Enclose $15 registration. $10 will be refunded if you receive assistance or if you do not and are therefore unable to come. Assistance available for three people)

signature____ approved by sponsor/professor/CSG member

signature____

* * * * * * * * * * *

To Roy Eagleson:

What is a "pure" control theorist? One of the main strengths of this group's explorations is Bill Powers' years of designing and building control systems. The difference in his approach from that of other control engineers is that he would take the point of view of the control system he was designing, rather than focussing on the desired (by the engineer) output of the plant being controlled. But using the same "pure" theory, and successfully (while simultaneously working on PCT). (Were you on the net during the discussions with Iszak Bar-Kana - if not I can send you Closed Loop #3)
Would you explain what you mean by "metric space" and why it is necessary?

To Dag Forssell:

I think if everybody had to acknowledge everybody else's messages we'd drown.
Comments not commented on are either perfectly

acceptable or too stupid to be bothered with (very few of those). I think the author of an unacknowledged message can figure out which. People comment on whatever message gets the creative juices flowing - that may leave some senders in the dark occasionally, but that's because nobody is responsible for answering any particular message. Would you have it any other way?

I'll leave it to Gary to consider the feasibility of Dag's suggestions. One filter that occurs to me is to suggest that anyone who wants to talk on the net might back up their interest in and support of this conversation by contributing to the CSG. Last time I looked, only about 20 netters are actual CSG members.

Date: Sat Apr 18, 1992 2:34 pm PST
Subject: Coach; conflict

[From Bill Powers (920418.1500)]

Martin Taylor (920418.1340) --

You've sort of taken off at right angles to the line of thought I was developing. The "Coach" example was meant to illustrate how an apparent SR relationship (encouragement --> doing better) can lead to quite a different interpretation when explored from the viewpoint of control theory. I wasn't trying to generalize from the particular way I and probably others dealt with Coach's urging us to overachieve. With another person or in another circumstance, a similar encouraging remark leading to improved performance could work in a different way. But it will never be a cause-effect way. My point was that to test control theory you have to think of possibilities other than the surface appearances.

Since I'm into high school stories, I remember another instance with a mathematics teacher. I didn't much like or dislike this teacher -- he knew his stuff but wasn't strong on making things clear. The class was doing an exercise, each person trying to prove a trigonometric identity. I was stuck -- something was wrong and I didn't know if I was even getting close. The teacher was going around the room seeing how everyone was doing. When he got to me, he said "That's fine, you're almost there."

This told me that I hadn't made any mistakes so far and was headed in the right direction. So I stopped worrying and went ahead and finished the proof, my first one. That felt nice. The 60th proof didn't feel so nice.

Apparent SR relationship: he said what he said, I then went ahead to reach the goal. Cause and effect? No. Information. I wanted to know if I'd made some stupid mistake, and he told me (in effect) that I hadn't. With that information, I could stop looking for a mistake and devote my efforts to something more productive. I didn't finish because I liked the teacher or in order to please him. I finished because I wanted to be able to prove the identity. His remark wasn't a disturbance of something I was trying to control; it provided a missing perception so I could get unstuck from looking for a nonexistent error.

My Coach example was one in which the apparent stimulus actually did disturb something I was controlling for, and my response opposed the effect of the disturbance. The result was to put a very different light on what seemed like a simple S->R chain. That's all I was trying to show -- not that there's something inherently bad about encouragement or that being as pushy as Coach was necessarily leads to resentment and bad feelings. In fact I never resented Coach; not many did. He was a nice guy. I just resisted him. I regretted not wanting to live up to his expectations, but not enough to change my mind.

Re: your comments on conflict.

Conflict doesn't "lead to" anything in particular. What it leads to depends on how you resolve it, or fail to resolve it. Most conflicts are unimportant; we just shrug and turn to something else, or go into a little fit of reorganizing and think of a different way out. This happens all the time; we have natural machinery for resolving inner conflicts and it usually works very well.

The degrees-of-freedom problem doesn't normally cause conflict because we've learned to use only those control systems that are compatible when working at the same time. The balancing of reference signals contributed by many higher-level systems isn't a conflict unless one of the higher systems is unable to keep its own error reasonably small because of the interference of other systems at the same level. The usual case is that all active higher-level systems keep their errors small despite the fact that no one lower-order system's reference signal is the exclusive property of one higher order system. The systems just find the analog solution of the simultaneous equations and they all are successful.

When opposing muscles are used to control limb position, there's no conflict. In fact there are two controlled variables that are independently adjustable: for the tendon reflex, one is the difference between the tensions in the two muscles, the other is the sum. The sum-of-tensions signal is controlled to produce a specific muscle tone. The difference signal controls the net applied force. Because the muscle is highly nonlinear, the sum (muscle tone) signal effectively alters the spring constant of the combined muscles near the zero-error condition, thus adjusting the static loop gain of the tension control system (and also the stretch control system).

Conflict is a problem only when it concerns some variable important to the organism, is severe, and goes unresolved for a long time. That's what brings the clients to the therapist or counsellor. Serious conflict destroys control or reduces its effective range to the point where it's not sufficient for the purposes normally served by the control systems.

A control system that keeps its error very small isn't likely to be "placid and content." It's able to keep the error small because it has a very high loop gain. This means that even the smallest disturbance will evoke an opposing effort, and that opposition will keep the controlled variable nailed to its reference condition. When you're driving a car along a mountain road with a washout on the cliff side, you tighten up that control system so the car stays precisely on the path you've picked to squeeze past the danger point. I don't think that "placid and content" describes that control system. But it's not in conflict, either: if it is, you have a problem because you won't be able to move the wheel as much as if there weren't any conflict.

There's a problem with your suggestion that "a system with tension and conflict will be more robust than one that is placidly content." The problem is that reorganization will start because of the chronic conflict. As a result, precise control will become impossible: the parameters of the control systems are going to be changing at random. What you get is a jittery and unpredictable control system that could literally do ANYTHING without warning.

Just because of neural response curves, I can believe that some slight amount of tension would help with rapidity of response to disturbances, because near zero signal the slopes of the functions will be very low and the loop gain will be low. But this is relevant only when the control point is set to zero and there are no disturbances. Most reference signals specify values of perceptual signals that are far from zero -- somewhere in the normal range between zero and maximum. And there's normally some amount of disturbance to raise the error signals above zero, if only gravity. In those cases, there's no advantage to conflict because conflict won't raise the sensitivity or speed of the system and will only reduce its range of control. I think that the best state to be in for possible action is one of alertness and calm. You should feel just a little zingy, but you certainly shouldn't be in white-knuckle conflict with yourself. You want everything working in the same direction.

So I guess I agree with your concluding remark: tension, conflict, and uncorrectable disturbance are good, but not in excess. I would figure something like 5 percent of the range of control. The rest of your reserve you would want to save for affecting the environment.

Uncalled-for remarks on social conflict.

In the background I suspect is an idea that competition is good for us (if not in your mind, then in others). Up to a point, while it's fun, I agree. We like to set problems for ourselves and solve them, and get better at solving them. But competition as a way of life doesn't work that way, except for a few winners. A social system based on serious competition is just a step from violence (in the US, a very short step). The losers vastly outnumber the winners: we end up with a society of losers, winners being an anomaly. In situations where the terms of the game determine that only a few can win, chronic losers can get very nasty; in fact, they tend to abandon whatever social principles there might be that make civilization better than life in the jungle. I don't think that the price is right. Competition -- interpersonal conflict -- is the lowest level of social intelligence. I don't like to admit that even a little conflict can be a good thing, because we've accepted a HUGE amount of conflict as good and natural for far too long. It's time to get smarter.

Best Bill P.

Date: Sat Apr 18, 1992 2:41 pm PST
Subject: Re: Starter document

[from Gary Cziko 920418.0820]

Dag Forssell (920417) said:

>Can we assemble a "Starter document" which can be available directly from
>list server without Gary's active involvement. What should a starter
>document contain?

If we go with the Usenet (NetNews) link, I would plan to periodically post a short document about PCT and CSG and CSGnet, including information on how to retrieve more information from the listserver maintained by Bill Silvert. Unfortunately, everyone the Listserv list would also get this post periodically, but it would be short and identifiable and could just be ignored by the old-timers.

I would be happy to send the current "Intro Package" to anyone who wants to work on this or just provide ideas on what should be said to newcomers.

>How about one of those standards being a request for any correspondent
>to preface any question by stating:
>
> My professional interest (reason for writing) is:
> I have read "Starter document" and thought about it.
> I have studied the following references:
> I have read CSGnet for (at least a month) time.

I don't agree here. I would hope that the CSGnet veterans could politely direct individuals to appropriate references when that is indicated. People wouldn't do all this anyway. It's hard enough to get everybody to always identify and date their posts before the text of their message (Dag, you didn't even do this on your post!).

>(A standard I would like to propose is that when someone has asked an
>open question and a CSGnet member answers, the answer is acknowledged
>with a comment on its clarity (being understood) being valid and
>addressing the question asked. When I answer questions, I do so both to
>answer and to test my own understanding and my ability to spell it out.
>To spend time to answer a question someone asked with apparent sincerity
>and then hear nothing makes you wonder if you were considered lucid,
>obnoxious, off the mark or unintelligible. It does nothing for your own
>growth to hear nothing at all. We work with all kinds of feedback on this
>net).

I've been guilty about this, too. I try to get back to people, but sometimes other things get in the way. For example, I still haven't responded to Bill Silvert's reply on adaptive control. Sometimes I just need time to ponder a response and after a couple of days or a week other things have intervened. And I don't think it makes sense to post a "Thanks Bill for your reply" to the net if that is all that is going to be said. But such courtesy replies could be sent directly to the addressee, and I think that this is a good idea.

>Questions by people who "just listen in" and do not bother to read and
>ponder our "starter document" can and should be ignored by all. This way
>we exercise "social control" of the kind that Bill has described.

You can ignore whomever you want to ignore. But why should you tell others whom they should ignore?

>The content of our starter document becomes important. Length, lofty
>visions with revolutionary zeal and down to earth explanations
>/definitions of feedback have to be carefully balanced. References
>carefully considered. Perhaps it would be sensible to have two. An
>introductory one to spell out what this is about. A second one for those
>who like the first one. The first one can be 5 pages. The second 40.

Dag, you're more than welcome to work on this for us. Thanks for your concern about the net and its evolution.

--Gary

Date: Sun Apr 19, 1992 10:59 am PST
Subject: re: testing models and conflict

To: Bill Powers and other CSGnet people
From: David Goldstein
Subject: testing models and conflict
Date: 04/18/92

Can we follow through on your coach example. What was the controlled variable for you? The degree to which Coach was pleased? The comment lead to your perception that Coach was not pleased. The increased effort was designed to increase the degree to which he was pleased. However, by making more of an effort you caused yourself some physical uncomfortableness and took some time away from activities which you enjoy more. So you really in conflict: you wanted Coach to be pleased more and you didn't want Coach to be pleased more since that meant hurt and time away from more interesting activities. Suppose that you said to Coach: I am doing as well as I want to for myself. If that really bothers you, I will be glad to quit. The Coach could say: I want you to quit, it really bothers me. The Coach could say: I don't want you to quit, it doesn't really bother me. In either case, you would be pleasing the Coach. What would you have done with each answer? I don't believe that you would have quit even if Coach gave the first answer. This suggests to me that you played football for several reasons other than to please Coach. I think you would have felt more relaxed about not putting out more effort if he gave the second answer. But you might have tried a little bit harder just to show Coach that you care about his opinion even after you made your remark.

What was the controlled variable for Coach? I guess it is a principle level generalization something like: Never accept the initial effort, always prod the players to do better, and accept whatever additional efforts they make. Underlying the comment, perhaps, is the thought: I think very highly of you. I can see potential in you which you don't see in your self. Coach is probably controlling for increased efforts beyond the ones a player can make comfortably. No pain, no gain. Effortfulness. Committment. The Coach's comment lead to your raising the gain. When a player does this, the performance is probably close to potential. The Coach wants each player to do the very best that he can.

Here is a clinical example which just happened and illustrated the difficulty of pinning down the controlled variable: A man was caught having sex with the babysitter in his own house. The babysitter was a friend of the wife. He had been having an affair with the babysitter over a period of eight years but not in the past two months. He doesn't understand why he did it. He did not have any intention of doing it, until the babysitter invited him and said: You better come down. The man said he experienced fear and guilt when he did it. The babysitter is not especially physically attractive but is a nice person. The man has been depressed over the winter months and has been worried. His work was keeping him very busy. He wife was noticeing that something was wrong but he withdraw from her and did not talk about his state with his wife. This sort of pattern of withdrawing and acting out is typical for this man who, in the past, would turn to marijuana or food as a means of coping with bad feelings. What was the variable being controlled by the man having sex with the babysitter? The comment of the babysitter told him that she wanted to have sex with him. Was he afraid to displease her? Did he feel threatened by her remark as in blackmail and his action reduced the fear? ? Did her remarks lead him to feel more positively about himself and his actions were a way of him feeling less positively about himself? He certainly is criticizing himself for what he did. The major result of getting caught is that his wife is very upset and is thinking about separation/divorce. She has been complaining a lot about the lack of time which he gives her and the children because of his business. Did the babysitter's remarks result in a sense of excitement which reduced the depressed feelings this man was having? And the act of having sex with her was a way of reducing his depression? So far, the possible controlled variables are: too much fear, too much self-esteem, too much depression. The act of having sex with the babysitter could have been: to reduce fear, to decrease self-esteem, to reduce depression. Since the man reported experiencing fear and guilt during the act, I would guess the latter choice. Any comments/questions on this example would be welcome and might help us on the more general issue of applying HPCT in applied settings.

Date: Sun Apr 19, 1992 11:05 am PST
Subject: re: coach; babysitter
From: "William T. Powers" <POWERS_W%FLC@VAXF.Colorado.EDU>

[From Bill Powers (920419.0700)]

David G. (920418) --

You sent direct to me so I'm replying direct. You can post the exchange to the net if you like.

>Can we follow through on your coach example. What was the controlled
>variable for you? The degree to which Coach was pleased? ... So you
>[were] really in conflict: you wanted Coach to be pleased more and you
>didn't want Coach to be pleased more since that meant hurt and time >away
from more interesting activities.

The conflict is (was) expressed at the level where there are different goals for the same thing. I wasn't in conflict about liking Coach and wanting to please him, or about wanting to be doing more interesting activities. I even had some personal goals about getting stronger and getting better at athletic things. The conflict came when Coach pushed me TOWARD one of my goals, and in fact PAST it. To be more exact, he made his approval contingent on my trying for a goal of physical achievement that was a lot higher than my own goal for physical achievement. To please myself, and fit my athletics time in with all the other things I had goals about, I made a certain amount of effort for a certain amount of time, and was satisfied that I was doing pretty much what I hoped to do. Then the Coach, for his own reasons, decided that it would be good for me to try harder, spend more time in the Gym, become not just a social football player but a dedicated one, and so on. Maybe he saw some physical talents there and felt they should be developed more (of course that was what he was hired for and what he thought worthwhile in life).

The net result was that in order to maintain a good relationship with Coach, which meant mainly that this admirable guy would express approval of what I was doing, I had to reset my goal for athletic prowess at a level higher than what my own values recommended -- at least temporarily. But doing this resulted in errors in my social life (too much time at the gym and football practice, more physical discomfort than I was willing to experience, a shift in self-image that didn't fit with my picture of me as a physicist, etc.). So I wanted to try harder and become better in order to please Coach (and get whatever other benefits would come from going that way, like being a football hero, scaring off people I was afraid of, etc.), and I wanted to try less hard in order to be more comfortable, have more time for my girlfriend, be with my other friends, tinker around with "scientific" projects in my room, and so on. It all came down to wanting to try harder (for one set of reasons) and wanting not to try harder (for another set of reasons). That was the focus of the conflict: I want to try harder and I want to not try harder. I couldn't do both. My solution was probably a typical adolescent solution. I gave the appearance of trying harder without trying harder, and let Coach believe (or so I thought) that I just didn't have the talent he thought I had. At least I was convinced that he believed it, and so my conflict was resolved.

>Suppose that you said to Coach: I am doing as well as I want to for
>myself. If that really bothers you, I will be glad to quit.

I could have quit football, but not physical education, which was required. And don't forget that one reason for going out for football (among several) was to please Coach! I didn't want to please him just to get him off my case. I liked him and admired him. The only problem was that he wasn't satisfied by that -- he didn't just say "Glad to have you on the team." He did say that, to my great pleasure, but he then went on to demand more of me than I was willing to give. A great way to turn people off is to "encourage" them to do more than they want to do.

This certainly wasn't the only area of my adolescent life in which there was a conflict that rested on wanting to be approved of and liked, a conflict that led me to do things that caused errors in my self-image and self-esteem, but satisfied (or would have satisfied if there had been no conflict) other desires. I was always aware of these conflicts, but didn't really have any good ways of resolving them. Finding those ways took me another thirty or forty years of messing around at random.

One of the things I was trying to get across with my example (aside from the main one, which was reinterpreting an apparently beneficial cause-effect situation in PCT terms) was that "helping" people doesn't always help a whole lot. Like the adolescent me, most people are already in the middle of trying to fit their various goals into one coherent structure. When you try to force them toward what seems like a worthy goal, you inevitably cause conflict with other goals. You become part of the conflict situation. Of course you're trying to help, but you're forcing the person in a direction that person has probably already tried to go, or in which that person has gone far enough to meet the goal. If the person hasn't spontaneously gone farther in that direction, it's because doing so would violate other goals. If you really want to help, you'll help the person find out what is keeping that person from achieving all goals that look attractive, not urge trying for any particular goal just because, in your life, it has proven to be worth pursuing for you or for others. And helping doesn't mean urging people to go PAST their goals.
My life has been full of well-meaning people who just knew that I could achieve great things OF THE KIND THEY THOUGHT WORTH ACHIEVING. If I'd gone along with all of them I would have been a physicist, a writer, an athlete, a biologist, a neurologist, a cop, a teacher, a debater, a poet, a

gardener, an engineer, a playwright, and so on -- but just one of these things and nothing else. Of course I was smart, so I've showed a little aptitude in all of these directions. But I was never into heavy competition -- I didn't want to be THE GREATEST in any of those fields, or in general. All you have to do is show a little interest in someone's field, and that person becomes convinced that you share the same obsession, and wants to do you the favor of helping you achieve fame and fortune in that field. People are really very generous in this way. But they aren't really "helping." They're really trying to validate themselves, their own choices of goals.

>Coach is probably controlling for increased efforts beyond the ones a
>player can make comfortably. No pain, no gain. Effortfulness.
>Committment. The Coach's comment lead to your raising the gain. When a
>player does this, the performance is probably close to potential. The
>Coach wants each player to do the very best that he can.

Probably something like that. It's a common viewpoint. It's also a narrow one, because what's a person's "potential?" Potentially, I could have been a great criminal. Potentially, I could have become a Hulk with deltoids like balloons. I could have become a pro football player. I could have become one of the world's great atomic physicists (at least one of my classmates did). People who see "potential" in you aren't considering your values, but theirs. They're also communicating, in a not so subtle way, that they don't think much of what you've done already. David, you have great potential as a psychotherapist (if you'd only just try a little harder). How's that grab you?

>What was the variable being controlled by the man having sex with the
>babysitter?

Pleasure, I suppose. The problem, of course, wasn't the variable involved in having sex, but all the goals that were frustrated by doing so with the babysitter, and the ones that were frustrated by not doing so even more often.

>Was he afraid to displease her? Did he feel threatened by her remark as
>in blackmail and his action reduced the fear? ? Did her remarks lead >him
to feel more positively about himself and his actions were a way of >him
feeling less positively about himself?

Your guess is as good -- or as bad -- as mine. I don't think it helps to generalize about something like this. The man did what he did for his own actual reasons, not for general reasons. If you want to know what they are, you'll have to ask the man, or help him to figure it out. I don't believe in trait psychology -- I think people have complex structures of goals and they each work out complicated ways of getting as close as they can to satisfying them all. Usually that isn't very close, without some serious work with an outside helper. I believe in the method of specimens, not the method of relative frequencies, for dealing with real individuals.

>Did the babysitter's remarks result in a sense of excitement which
>reduced the depressed feelings this man was having? And the act of
>having sex with her was a way of reducing his depression?

What's the point of guessing? Ask the man.

> So far, the possible controlled variables are: too much fear, too much
>self-esteem, too much depression.

Those are error signals, not controlled variables. And they're just three possibilities out of hundreds. Out of all the men who have sex with babysitters, you probably won't find two who were conflicted about it for the same reasons. You'll find some who are quite happy with the arrangement and don't have any conflict at all. The only thing you can say about all of them is that they probably found the sensations pleasurable. The sex isn't the problem save for impotence or pain. The problem is all the OTHER goals, and the goals of all the other people involved. This isn't a problem about sex. It's about relationships with people. If it weren't for all those complicated relationships, female babysitters (I assume) and male employers could screw their heads happily off and there'd be no problem, with mutual assent. Sex between consenting adults is never a problem -- the mechanics are rather simple, it feels good, and the moves come naturally. What's the big deal?

The big problem is what we and others think about certain people having sex with certain other people, in the light of agreed-on relationships and rules. And of course, the physical consequences, which one may or may not be prepared to accept. If the babysitter got pregnant, this would probably require revising some lives rather drastically. Would that be OK with the man? The only way to find out is to ask.

>Any comments/questions on this example would be welcome and might help
>us on the more general issue of applying HPCT in applied settings.

To use HPCT in applied settings, you apply it. HPCT doesn't contain any specific list of most common goals, except tentatively and only by type. It certainly can't tell us what the most common conflicts are, or whether any individual suffers from them. What it does is tell us what to look for when dealing with a real live person who is interacting with us right now. It tells us what a conflict IS, and how it relates to higher-level goals. It gives us a method for exploring structures of goals. I don't think there's any useful way to generalize such structures across people, or to bypass the exploration and guess what the probably structure will be in one big lucky jump.

Ask the man.

Best Bill P.

Date: Sun Apr 19, 1992 11:26 am PST
Subject: ups and downs of conflict

[From Rick Marken (041992)]

Martin Taylor alluded to the potential value of a moderate level of conflict. Bill Powers agreed that some small amount of conflict might help in some situations. Bill says:

>So I guess I agree with your concluding remark: tension, conflict, and
>uncorrectable disturbance are good, but not in excess. I would figure

>something like 5 percent of the range of control. The rest of your reserve
>you would want to save for affecting the environment.

I'd like to point out that Bill's "5 percent" figure is based on experimental evidence. Nearly two years ago I stumbled on the fact that people can control better when the disturbance to a controlled variable is caused by the output of another control system than when it is simply the result of causal processes. I had subjects do a tracking task where the disturbance (d(t)) was the output of a low gain control system that was trying to keep the cursor at the center of the screen. This control system was in conflict with the subject (who tried to keep the cursor at another, "target" location on the screen). The subject always "won" the conflict because the opposing control system had such low gain. What I wanted to show was that the output of the opposing control system would be delt with by the human subject just as a disturbance -- as though it were simply drawn from a table of numbers in the computer, as usual. So I did one tracking session with d(t) generated by the opposing control system. I also saved this d(t) in memory. Then I did a second run using the d(t) from memory as the disturbance -- the SAME sequence of numbers that had been the disturbance during the first run. Performance (measured as RMS error or stability or whatever) was ALWAYS poorer with the replayed (or not-actively generated) disturbance. This was a VERY surprising finding; it was dubbed the "Marken effect" -- which made my kids very proud.

Bill Powers discovered the explanation of the Marken effect. It turns out to require no changes in the PCT model; just the recognition that there are transport lags in control systems (we rarely build transport lags into our simulations, but we should). The "actively generated" disturbance (from the conflicting control system) acts a bit like a spring, allowing dynamic stability and, thus, better control. Once d(t) is generated and replayed, there is no possibility of moment to moment adaptation to the subject's dynamics by the opposing disturbance. Bill (and I) confirmed that a control system with a transport lag (I forget the value -- I think 100 msec) exhibits the Marken effect -- just like subjects.

Bill suggested (and I confirmed) that you might be able to get improved control in a tracking task if you add the output of a conflicting control system to the "inanimate" disturbance in a tracking task. The gain of the conflicting control system must be low, of course -- and the optimal value of the gain produces output that contributes about 5 percent of the total variance of the effective disturbance to the controlled variable. That is, if q = h + d (where q is the cursor, h is subject output and d is disturbance) then, in the "improved control" situation, d = de+dc, where de is is the regular environmental disturbance and dc is the added effect of the active output of a conflicting control system. Adding dc to de IMPROVES CONTROL if dc contributes only about 5 percent of the variance to the variance of d.

So conflict can help people control -- but the gain of the conflicting system must be VERY VERY low. If the conflicting control system were a person he/she would be VERY unhappy because he/she would ALWAYS BE LOSING -- s/he would not have any control of the variable s/he is trying to control.

So I heartily agree with Bill (again) that it's probably best not to harp to much on the presumed value of conflict; there is FAR too much interpersonal conflict already and the kind of conflict that seems to be of any value (like the kind in the Marken effect) requires that the gain of one system be so low that people would never want to be that system themselves; weak artificial control systems would be best in that role.

Best regards Rick

Date: Sun Apr 19, 1992 7:38 pm PST
Subject: Re: Coach; conflict

[Martin Taylor 920419 23:20]
(Bill Powers 920418.1500)

I guess I went in an orthogonal direction from what you had intended with your "coach" story because it triggered things I wanted to get onto for some time. But your response is also orthogonal to what I had in mind.

You have talked about reorganization as being a consequence of continued, sufficiently bad "intrinsic error." As I understand "intrinsic error", that would make reorganization a whole-system thing. But we had got so far last month as to agree that it had to be modular, and I was working on the presumption that reorganization within a module (a fuzzy module, not one with clear boundaries) would be occasioned by the continued sufficiently bad failure of the module to satisfy its various references. Under those conditions, we don't get a jittery and unpredictable system that could do anything without warning, at least unless the modules concerned are quite high-level. Most of the hierarchy will still be quite stable.

In our Little Baby project and its related speech-recognition project, we are talking about three quite different ways in which a control system can change (and thereby learn): quasi-Hebbian changes in weights and gains, Powersian reorganization (within modules), and Genetic Algorithm based reconstruction of the hierarchy. All of them are "controlled" (forgive me--it's not the right word in this context) by the sustained error. But we have no results yet. I hope when I get back in June that at least one of them will have been tried out.

I hope also that I will have time to pursue this theme of tension and low-level
conflict when I get back in June--if CSG-L still has the character of a working
group and not a discussion fest.

I totally agree about the problems of social competition. We have far too much of it, and it is an article of faith for many in North America that competition is good. And I do believe that some level of competition is good. Without it, we have a super-stable non-evolving society such as perhaps might have been in Europe before the Black Death, or in Egypt under the middle pharaos, or in China for millenia under the stifling civil service aristocracy. Such a society is not robust against new challenges, and does not react quickly to disturbances, any more than does an undisturbed control system--I note your comment about high-gain control in a tense situation.

Martin

Date: Mon Apr 20, 1992 4:04 am PST
Subject: Apples and Oranges

From Greg & Pat Williams (920420)

Mary Powers says, in reply to Roy Eagleston:

>Would you explain what you mean by "metric space" and why it is necessary?

Continuous control systems designed by engineers have comparators which output error signals which are not simply binary (error or no-error), but can vary along a (numerical) dimension. That is, there can be (at any given time), more or less error (and often the error can be either positive or negative). But how does PCT deal with comparisons which don't appear to be made along a single numerical dimension? For example, how would "apples" (reference signal) and "oranges" (perceptual signal) be compared, and what would the error signal look like? The problem is that continuous-control-system operation seems to require that error signals always be ordered along a "more-less" continuum (with an ordering, at least, if not necessarily a measure ("metric"), of the error's "size" at one time relative to the error's size at another time). There are at least three ways to compare apples and oranges: via discrete comparators ("match" or "no-match," error or no-error), breaking up the signals into sets of unidimensionally ordered signals which can be handled by several continuous comparators, or comparing the signals without breaking them up by some sort of fancy comparators.

Questions: Are there any generative models being offered by PCTers for any of these ways (or other ways) to compare apples and oranges? We have a fuzzy idea that Bill favors the second alternative; if so, is there a model for how the signals come to be broken up into various dimensions, and for how the separate errors are integrated back into a composite error signal (if integration is assumed to occur)?

In short, we'd like to hear about speculations on control mechanisms for making "qualitative" comparisons.

Greg & Pat

Date: Mon Apr 20, 1992 7:30 am PST
Subject: Imagined disturbance, confidence

cunningB@monroe-emh1.army.mil

From Bill Cunningham :920420.1105:

Bill Powers (920417.1100)-

Your response was both surprising and illuminating. I hadn't considered the model (should have), although I now see it at the root of my comment. I was observing what I've always heard referred to as "negative fantasy", wherein the players take counsel in their fears. The explanation I've always heard is that the individual worries about his/her ABILITY to deal with the disturbance -- rather than the potential disturbance itself. But that requires a model of how oneself would perform in a given situation. By definition, that's prejudice. Prejudice is bad, I guess, when we lose the ability to test the model objectively. You sound pretty damned healthy to me: identified model, didn't like implication, sought external test of model. Thanks for the insight.

On the coaching exchange:
Sure wish I had heard of PCT during active years of coaching hockey and soccer. One of the principles in task training is to introduce a skill at a rudimentary level, with zero resistance. In fact, the teaching drill is organized into a "game" wherein the player can only perform correctly. Once the task is learned (not mastered), controlled resistance is progressively introduced so that player can successfully overcome variety of disturbances. This builds confidence, in addition to skill. The disturbances can't be controlled under match conditions, except in the gross selection of opponents. It took me a very long time to understand that the long term objective was not just to get players to extend themselves in preparation, but rather to give them the ability and confidence to over come the uncontrolled match level disturbances. I found it virtually impossible to get a player to try a new skill in match play, where it MIGHT fail. Also, the more fatigued the player, the greater the tendency to regress to "safe" moves, even if these are easily defeated. Hence a real motivator for fitness. The encouragement from the trig teacher removed a negative fantasy and provided a safe base from which to take risks--some of which might fail; but at least one of which was "guaranteed" to succeed.

Bill C.

Date: Mon Apr 20, 1992 7:57 am PST
From: Cynthia Cochran
EMS: INTERNET / MCI ID: 376-5414
MBX: cochran@clio.sts.uiuc.edu

TO: * Dag Forssell / MCI ID: 474-2580
Subject: Re: Starter document

Dag,
I got, read, and underdstood your answer to my question about rubberbands. The reason I did not reply was that I got booted off the stystem while I was reading the instructions on how to reply to an individual sender. When I returned, the mail messages I had already read were deleted.

I have been reading this board for a year, although I often skip and skim since the messages are many, lengthy, and I am writing a dissertation.

The reason I read the bulletin board is that Gary Cziko interested me in PCT when I observed his class for a study on reading and writing of graduate students. PCT is much more attractive than behaviorist theory, but shares some of its stronger points. I am trained in cognitive theory of the sort that Herb Simon describes, esp. after taking many courses in cognitive processes involved in reading, writing, and thinking in general. I also am fascinated by Rand Spiro et al. cognitive flexibility theory. Most of the work I do now follows constructivist theories of reading, especially Nancy Spivey's version.

Your last post made me cringe. The post I sent was the first one I had ever dared send, since I am somewhat shy about public conversations about things which I do not feel comfortably knowledgeable. Like PCT's myriad of operators, loops, variables and variations. I was searching through the board for your address when I came upon it, and thought you were referring to my questions and lack of follow-up response. Okay, so I may not be a n expert, having read Power's basic treatise only once, but my understanding of this board was that PCT is relatively new, and that people from a wide variety of fields come into it all the time, that "respect" was a touted feature of this board. The video demonstrations and the computer simulations I have seen of PCT tests have convincced me that one's percept control relied heavily on the quality of feedback. I erred in thinking that one perceives feedback, rather than the result of that feedback ON THE VARIABLE which your response taught me. So thank you. But your follow up reply makes me worry about the reception I may get of my other dumb questions.

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Date: Mon Apr 20, 1992 7:58 am PST
Subject: ups and downs of conflict

[From Rick Marken (920420 0800)]

A private post from Martin Taylor made me realize that you folks out there are too smart to let me get away with a mistake I made in my description of the Marken effect. I said that the conflicting low gain controller was trying to keep the cursor in the center of the screen. This was not correct (altough it was correct for the "improved control" situation where the output of a conflicting controller is added to an environmental disturbance). What I really did was have a subject try to keep the cursor on target (near the middle of the screen) while the conflicting controller tried to MOVE the cursor back and forth randomly. I made the reference input to the conflicting controller a smoothed, time varying random variable -- just like the one that we ordinarily use for the disturbance itself.

In his private post Martin said he didn't understand why the conflicting controller created a disturbance (I hope this is OK to quote Martin, because I think it's a good question)

> Presumably the conflicting controller (the low-gain
>one) would by itself reduce the variance of the cursor position by

>counteracting the disturbance. I'm not clear how the conflicting
>controller was contributing variance. Wouldn't it be reducing it?

Hopefully, my explanation about the "varying reference" in the conflicting controller explains this. You are right -- if the conflicting controller had a fixed reference then it would not be contributing a varying disturbance for the subject to counteract. By varying the reference of the conflicting control system, the system varies its output to try to get the cursor to match the reference -- and since it is low gain, does a piss poor job of it. But its varying output provides a nice disturbance to the efforts of the human controller. When this disturbance is replayed the subject's control is poorer than it was when the SAME disturbance was actively generated, often by a factor of two or more.

When the output of a conflicting controller is added to the disturbance in a tracking task (d = de+dc where de is the regular disturbance and dc is the disturbing output of the conflicting controller) then I had the reference of the conflicting controller fixed. I just realized that this means that the variance of d will be slightly less than the variance of de alone. So any improvement in control using d rather than de could be attributed to the reduced variance of the disturbance. I'll have to do some more research to show that the improvement is due to the addition of de. I think it is a result of adding dc and not just the result of lower variance of d. I think so because, if you make the gain of the conflicting controller too high then there is the expected degradation of performance that comes from being in conflict with another control system -- and when the gain of the conflicting control system is high, that control system is acting to reduce the variance of d considerably. So there is performance decrement even though variance of d is reduced. But I should do some more research on this. I should be able to do the necessary studies this weekend. I'll let you know how it comes out if you are interested. This is why I need graduate students, darn it.

Regards Rick

Date: Mon Apr 20, 1992 8:34 am PST
Subject: Reorganization; apples & oranges

[From Bill Powers (920420.1000)]

Martin Taylor (920419.2320) --

I think CSG-L will continue to have the character of a working group. After all, what can anyone say that isn't grist for the PCT mill?

I think we agree that some low background level of reorganization is a good idea. To that I could add that at the higher levels, where we are on the leading edge of evolutionary development, reorganization may be one of the main ways of groping for control. When reorganization shuts down at the highest level, creative life is finished. I suppose I harp too much on conflict (for reasons with which you evidently agree). We shouldn't forget that control can fail for other reasons, such as confusion or lack of skill or knowledge. Simply developing the hierarchy is a massive job of reorganization.

Greg and Pat Williams (920420) --

>But how does PCT deal with comparisons which don't appear to be made
>along a single numerical dimension? For example, how would "apples"
>(reference signal) and "oranges" (perceptual signal) be compared, and
>what would the error signal look like?

If reference signals are derived from past experiences with perceptual signals, the situation of which you speak wouldn't occur. The basic question that led to PCT in the first place was "how can a person repeat an action?" This led to realizing that it is not the action, but the perception that is repeated (the second action may be completely different from the first). And then, if the perception is to be "repeated", the question is how one knows it is the same perception as before, which immediately brings memory into the picture. One remembers a perception and acts to make the present perception match the remembered one. It follows that (perhaps) reference signals are derived from memories of previous perceptions, which nicely takes care of the problem of comparing apples with oranges.

My feeling is that the situation of which you speak doesn't occur. So before we spend a lot of time dealing with an imaginary problem, perhaps what we need are some nice examples of goals that are stated in units different from the units of the perceptions that are compared with them. Such an example would propose "I want X*," and show that the action that achieves X* does not produce perceptions of X to match X*, but perceptions of Y which are incommensurable with X*.

>... is there a model for how the signals come to be broken up into
>various dimensions ... (?).

No -- the process in HPCT works the other way. The individual perceptions come first (beginning with excitation of individual sensory endings). Then higher-level perceptions are derived as functions of the lower. So a FACTUAL analysis of the higher-level perception will reveal the lower-level perceptions of which it's in fact a function. An IMAGINED analysis (based on possibilities instead of examination of what appears to be the case), could propose that oranges break down into apples, Buicks, and a feeling of despair. Why not? If you're just looking at possibilities, anything goes. Making sense of such possibilities, however, is another question.

If you want to compare apples and oranges and make any sense of the comparison, then you have to talk of SOMETHING ABOUT the apples and SOMETHING ABOUT the oranges (going down a level) or SOMETHING MORE GENERAL indicated by either apples or oranges (going up a level). You can speak of the number of apples being less than the number of oranges, in which case you're comparing numbers. Or you can say "There's too much fruit in that basket," treating the configurations categorized as "apples" and "oranges" as members of a third category: pieces of fruit. Then you're perceiving and comparing in units of fruit.

I don't think there really are any qualitative comparisons. But come up with some examples and prove me wrong.

Best

Date: Mon Apr 20, 1992 11:15 am PST
Subject: Attributes: mixed fruit & metrics

cunningB@monroe-emh1.army.mil

From Bill Cunningham :920420.1300:

Pat & Greg Williams (920420)-
The following attempts to summarize where a colleague and I are headed on this point. Best described as PCT with a string matching algorithm drawn from "Sparse Distributed Memory", Pennti Kanerva, MIT Press 1988. Bottom line for the metric is Hamming distance between multidimensional reference & input.

Starting with description of the sensed objects (or events) using Aristotelian attributes, the attributes can be grouped into various classes--apples, oranges, fruit, things to juggle, etc. The objects have more attributes than necessary to define a particular class and the objects generally belong to more than one class. The attributes may be orthogonal, especially those attributes defining class distinction. The problem is not one of matching the attributes exactly, but finding a "best fit" into a class. The reference attribute set includes weighting of individual members based on previous experience. Said weighting would discount attributes not significant to that particular class. Hamming distance between the two is calculated on basis of 3-level logic (yes/no/indeterminant).
Class assignment based on minimum Hamming distance, subject to an error threshold that accepts the match as "good enough" or requiring recursive search. Original motivation was the need to deal with noisy sensory input, something not discussed on the net. I should add that that attribute sets form can form very large (n=256) bit strings.

Principle reason for class distinction is progressive stricture of variety (which is why Martin's degrees of freedom comment was of extreme interest). Moving up a hierarchy in a Kanerva scheme implies discarding those attributes not relevant to the matching process at the next level.

My friend's name is John Gabriel. He works at Argonne Nat'l Lab, and can be reached by e-mail at 'gabriel@eid.anl.gov'. I've been screening and forwarding CSG traffic to him for about 6 months, but haven't been conned into subscribing on his own. Here's a good chance.

Bill C.

Date: Mon Apr 20, 1992 11:16 am PST
Subject: Re: USENET VOTE ANNOUNCEMENT

[from Gary Cziko 920420.1150]

Martin Taylor (920417 2130) said:

>If we can't have a one-way link, for the reasons you
>mention, then we should have no link. I, for one, would probably quit

>reading the group, and go with personal mail to Bill.

Of course, personal messages should be sent personally, but your comments have been appreciated by other CSGnetters and so I would hope that you would continue to share them with the net (and Bill his responses).

I can imagine only two ways in which a two-way Usenet link would disturb you: (a) people reacting to your posts when you wish they wouldn't (solution: don't read their posts); (b) too much mail for your system (solution: read CSGnet via Usenet and read only those things (and those authors) that you find interesting.--Gary

P.S. I am still accepting votes on the two-way Usenet link at my personal address through Friday, April 24. Simply send a message with "Yes to Usenet" or "No to Usenet" to G-CZIKO@UIUC.EDU before the end of the week.

Date: Mon Apr 20, 1992 11:44 am PST
Subject: Re: Bill P. on Fruit

From Greg & Pat Williams (920420)

Bill Powers' answer to our post about qualitative comparison appears to reflect our lack of clarity in posing our questions. Here's another try: Suppose you are writing a paragraph and decide, upon reflection, that it doesn't fit your goal for how it should "sound" to you. How far along are PCT models for how the error-correction process can happen in such a "qualitative"-looking piece of control. Quantitative error is obviously easily dealt with by a control system; how is qualitative error dealt with? Forget the misleading "apples" vs. "oranges"; we want to hear about comparing signals with SIMILAR QUALITATIVE (naively speaking) forms, like the "styles" of two different pieces of writing. If you think that "qualitative" is really complex (maybe multi-dimensional?) quantitative, fine. Regardless, what do PCT models look like for, say, controlling for the "right" "sound" while writing a story, as opposed to controlling for the desired number of units of something or the desired position of something?

At any rate, we do seem to make decisions on the basis of multi-dimensional comparisons involving entities with parts which are difficult to quantify, for example when buying a car (unless the price is the only basis for comparison!). This one has an air bag, but that one has antilock brakes; this one is green and that one is blue; etc. -- but we end up buying one.

Greg & Pat

P.S. Please send the Foreword for Rick's book at your earliest convenience.
P.P.S. that P.S. was to Bill P., of course.

Date: Mon Apr 20, 1992 11:51 am PST
Subject: Re: Starter document

[from Gary Cziko 920420.1400]

Cynthia Cochrane (920420) said:

>A starter document, perhaps the same one I've seen periodically on this net,

>would be helpful even to those of us who read the board regularly but need
>periodic reinforcement. I lost my "starter documents" long ago in a
>computer glich. It would be nice to see the "Intro' package now and again.

If anybody would like this sent to them, just send me a personal note at G-CZIKO@UIUC.EDU and I'll send it on its way. Cindy will get hers in a few minutes.

What I think should appear periodically on the net is a reminder that such information is available, rather than send a long document out to everybody. Eventually, "offically CSG approved" starter info will be available on the fileserver and so I will just need to remind people how to get it.

>But requiring people to preface their remarks with proof of their legitimacy
>seems a bit territorial. Some of us readers are shy.

I agree.--Gary

Date: Mon Apr 20, 1992 12:07 pm PST
Subject: Quality fruit

cunningB@monroe-emh1.army.mil

From Bill Cunningham :920420.1530:

Greg & Pat Williams (920420.2nd post)

From the Kanerva viewpoint, a "qualitative" judgment would be a multidimensional match attempt where the attribute weighting is based on personal preference. The "quality standard" lies in both the weighting choice and in the acceptable error (Hamming distance). Given that interpretation, I see no reason not to consider part of (or perhaps extension of) PCT.

Bill C.

Date: Mon Apr 20, 1992 12:51 pm PST
Subject: Re attributes post

>from Alan E. Scrivner (920420:1305) Bill C. Writes:

> Starting with description of the sensed objects (or events) using
>Aristotelian attributes, the attributes can be grouped into various classes--
>apples, oranges, fruit, things to juggle, etc. ....

This post sounded a lot like a problem I had worked on in the past on aircraft track correlation. i.e. How to identify a single aircraft's flight path in the midst of noise from other aircraft tracks, birds and missing or misleading position information. My solution was to use a self-organizing neural network. It finds the N-dimensional "ball" of best fit around a collection of points in parameter space. Perhaps these ideas could be useful in the apple & oranges discussion. The axes for which would

be color, texture, size, deviation from spherical, etc. Backing off and looking
from this problem from this higher-dimensional space, I think apples and oranges
would easily be identifiable and would occupy quite distinct regions.

Alan E. Scrivner ms54aes@mercury.nwac.sea06.navy.mil

Date: Mon Apr 20, 1992 1:38 pm PST
From: Dag Forssell / MCI ID: 474-2580

TO: csg (Ems)
EMS: INTERNET / MCI ID: 376-5414
MBX: CSG-L@VMD.CSO.UIUC.EDU
Subject: Starter document
Message-Id: 43920420213834/0004742580NA4EM

From Dag Forssell (920420)

Mary Powers (920418) Gary Cziko (920418.0820)

>I think if everybody had to acknowledge everybody else's messages
>we'd drown.

I certainly agree with you, Mary. This is not what I meant.

The issue I have raised about my desires is not major, but since I have raised it, I will follow through.

Where I send an unsolicited message, I expect no acknowledgement from anyone. If it starts the creative juices somewhere, fine.

If I ask for specific information from anyone out there, and Joe Smith answers with more than a trivial bit of data, I feel that I personally owe Joe Smith a response. Requests like this have been made on the net at most once a week, but I anticipate that the frequency will increase with a two way Usenet connection.

If I answer a request from someone, I do so for several reasons. Like everyone else, I have my own background and angle on PCT. I am hooked on PCT. I want to see PCT grow and gain acceptance. I benefit greatly from reading the net, and want to contribute. I also want to grow myself. I may try a new perspective or way to explain. I may not be so sure of my own understanding or express myself as well as I might. To me, it seems quite reasonable to suggest that ideally - see below - someone who asks a specific question that requires more than a trivial answer enters into a social contract to let me know how my answer was perceived in exchange for the effort I put in to make a contribution to this someone's growth.

I know that this understanding or opinion of mine and my sensitivity to it is a reflection of the conflicts I have experienced with other individuals since I was born. Blame my Swedish culture if you wish, or exposure to Californians who don't keep appointments. Just blame me. Swedes and Californians are not responsible for me. Certainly I have my prejudices. Other netters have different understandings, and do not have to agree with me.

PCT teaches us that we re-create perceptions all day. We call them references or "wants." To get along well with others, the best thing is to discuss your wants, not to focus on actions.

I am influenced in my thinking by an article by Dr. Kosaku Yoshida: Deming Management Philosophy; Does it work in the US as well as in Japan? Columbia Journal of world business. Volume XXIV Number 3, Fall 1989. In this article, Dr. Youshida discusses the difference between Desirability and Acceptability.

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         Acceptability      Desirability
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With acceptability, everything inside the border is acceptable, everything outside is unacceptable. Defining the border becomes the major issue. This is very difficult. It requires defining conditions that you are not willing to accept under many different circumstances .

With desirability, the center is most desirable. Conditions progressively farther away from the center are progressively less desirable. Defining the center becomes the major issue. This is relatively easy. It requires defining what you think is ideal.

To exercise control where the reference is defined by acceptability is awkward. Whenever the perception crosses the boundary, an error signal appears. This is a discontinuous process. It will appear arbitrary to observers who do not know where the border lies. Particularly if the border has not even been discussed. It is easy not to discuss the border, particularly in a culture where almost anything goes - or you are accused of discrimination.

To exercise control where the reference is defined by desirability is easy, once the ideal has been defined. Control is smooth and continuous. You can respond gently to small error signals.

When I propose standards, I am proposing a statement of what would be ideal, realizing that it will rarely be met, even by me. Still, it will serve as a guide and reference.

>If we go with the Usenet (NetNews) link, I would plan to periodically
>post a short document about PCT and CSG and CSGnet, including
>information on how to retrieve more information from the listserver
>maintained by Bill Silvert. Unfortunately, everyone the Listserv list
>would also get this post periodically, but it would be short and
>identifiable and could just be ignored by the old-timers.

Ok, I have understood you correctly. Bill Silvert's listserver can hold several documents which anyone can access without bothering you. The periodic document will not bother me.

>>How about one of those standards being a request for any correspondent

>>to preface any question by stating:
>>
>> My professional interest (reason for writing) is:
>> I have read "Starter document" and thought about it.
>> I have studied the following references:
>> I have read CSGnet for (at least a month) time.

>I don't agree here. I would hope that the CSGnet veterans could
>politely direct individuals to appropriate references when that is
>indicated. People wouldn't do all this anyway.

If we soften the meaning of the word "standard" to be "desirable ideal" perhaps the intent of my proposal would be more palatable. It certainly would set the stage for a polite direction to appropriate references. Some people just might do it. Some have as I recall, when they make a first posting. An important question is: Would it be beneficial if they did?

>It's hard enough to get everybody to always identify and date their
>posts before the text of their message (Dag, you didn't even do this on
>your post!).

Guilty as charged!

>>Questions by people who "just listen in" and do not bother to read and
>>ponder our "starter document" can and should be ignored by all. This
>>way we exercise "social control" of the kind that Bill has described.
>
>You can ignore whomever you want to ignore. But why should you tell
>others whom they should ignore?

You are quite right. I am going overboard.

>>The content of our starter document becomes important.
>
>Dag, you're more than welcome to work on this for us. Thanks for your
>concern about the net and its evolution.

Please send the current "Starter package" direct in its entirety. While you are at it, I will really appreciate your paper in its final form.

I will ponder this and post a suggestion by April 27. I take for granted that you, Gary will be the arbiter on this, and hope that anyone else who feels inspired will post their suggestions.

Dag Forssell
23903 Via Flamenco
Valencia, Ca 91355-2808
Phone (805) 254-1195 Fax (805) 254-7956
Internet: 0004742580@MCIMAIL.COM

Date: Mon Apr 20, 1992 1:42 pm PST
From: Dag Forssell / MCI ID: 474-2580

TO: Cynthia (Ems)

EMS: INTERNET / MCI ID: 376-5414
MBX: cochran@clio.sts.uiuc.edu
Subject: Starter document
Message-Id: 34920420214243/0004742580NA4EM

[From Dag Forssell (920420-2)]

Cynthia Cochran (920420) Direct

Cynthia,

Thanks for your thoughtful response this morning. Your post is a beautiful illustration of the issue I have raised. I need to be told that you cringed. I thought my post was strictly technical and quite non-personal, but now that I see your more personal introduction and commentary, I can learn to be more careful.

I trust you realize that I have never before voiced the point I made to you in my post. It just came to me from my understanding of PCT in response to your question, which to me was an intellectual challenge. I don't mean provocation, just something I wanted to understand.

Just like I did not know who you are, you have no idea who I am, unless you happen to have caught the one post in the past year, where I touched on that.

My background is in mechanical engineering. I have industrial experience for 25
years. I know nothing of the people you refer to. I have seen some computer demos of cognitive experiments at Stanford during an open house. I have not studied Behaviorism or Cognitive Psychology in depth.

I have read many, but not quite all of the PCT references and am about to go public with a seminar for business executives on PCT and better management.

I am vitally interested in how to explain PCT and show how to use it to live better.

We respect each and every person as an autonomous, living perceptual control system in this group. We sometimes fight tooth and nail on what we perceive as important issues of understanding, application, logic or whatever.

Your question was not dumb. It was not dumb since you are seriously interested and got stuck on that point. It was also very useful to me in that it attracted
my attention to an interpretation I had not thought of.

There are no dumb questions. There may be careless questions, insincere questions and lazy questions. - I think. The latter is what I perceive CSGnetters to be wary of as we consider a trial period with Usenet.

If you like, I will mail you a few pages from my presentation that illustrate the difference between studying a control system (with some eight boxes and arrows connecting them) and being one (just four of those boxes with arrows). Snail mail address required.

Please read my reply again and consider the possibility of perceiving it as an attempt to provide a supportive technical dissertation in response to a valid technical question.

(One of the delightful insights of PCT is that we choose our interpretations and can select the ones that suit us best).

Please put all this in context of my post to the net today. Your post to me is of interest to the net, and I hope you will choose to post it along with this reply.

And welcome to CSGnet. All the best.

Dag Forssell 23903 Via Flamenco Valencia, Ca 91355-2808 Phone (805) 254-1195 Fax (805) 254-7956 Internet: 0004742580@MCIMAIL.COM

Date: Mon Apr 20, 1992 3:14 pm PST
Subject: Re: Starter document

I prefer Dag's "softer" version of why people should introduce themselves in prefatory remarks to their questions. His original post (not your answer to my original question, Dag) made me cringe enough to send him a personal reply complaining that, essentially, his post about the need for new readers to familiarize themselves with a Starter Document and then identify their interests, backgrounds, etc., was asking a bit much of the shy types out there.

Nonetheless, his direct reply to me convinced me of the value of ENCOURAGING new or perifory netters to introduce themselves, since doing so will help others contextualize their posts. Here goes:

Although I am in an English Department (at Carnegie Mellon, as a PhD candidate and at the University of Illinois, as a visiting instructor), I am vitally interested in PCT for three reasons:

1. it takes the best of behaviorist theory and dumps the rest.
2. it seems consistent with goal-directed problem-solving theory but seems broader in scope because it
    a) incorporates feedback more formally into its models
    c) it provides a possiblity of nesting social, cognitive and motor goals (this should be (b))
3. I liked Gary Cziko's lectures and demos on the subject when I observed his class.

Someday I hope to be able to understand the technical information associated with model-building sufficiently enough to start a model on the socio-cognitive
variables involved in reading-writing acts. Right now I am working with a constructivist framework, which essentially says that people use information from texts interactively with information they already have to select, combine and organize the texts they read and write. I am currently trying to find out how peoples' intended audiences and their contextual "discour
"discourse communities" (e.g., your academic field and all the people in it) play a role in their reading-writing behaviors.

And I have been reading the board (including the basic information posts) for a year, although I skim and skip.

Hello.

Cynthia Cochran

P.S. And Dag -- Your response was extremely articluate (re: Rubber Bands)

Cynthia Cochran
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University of Illinois
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Date: Mon Apr 20, 1992 3:24 pm PST
From: Cynthia Cochran
EMS: INTERNET / MCI ID: 376-5414
MBX: cochran@clio.sts.uiuc.edu

TO: * Dag Forssell / MCI ID: 474-2580
Subject: Re: Starter document

Dag,
Again I lost your other piece of mail, so I am responding to this one instead.

I introduced myself to the net, as you can see.

I did not mean that your response to my question made me cringe. Rather, it was succinct and clear. What made me cringe was the implication behind your first Starter Document post, particularly the idea that new or perfory readers prove their merit. I like your softer version better.

Thanks for your concerned reply. I very much would like your document since one of the problems I see with using PCT for work on qualitative differences between peoples' behavior is that it is often difficult to separate the quality rater from the action being rated. This happens when we try to rate papers that students write -- and when in the context of research into writing, this often means that the findings of the study are suspect or at least susceptible to difficulties in replication.

My snailmail address is:
Cynthia Cochran
3610 E. University Ave. #2
Urbana, IL 61801

Thanks, Cynthia

Date: Tue Apr 21, 1992 2:09 am PST
Subject: Qualitative control; methods of steep descent

[From Bill Powers (9204.2300)]

The late batch of mail today was most interesting. The original question that Greg and Pat Williams posed -- how does qualitative control work -- evoked answers very much like the ones I would give, but obviously from fields that have tackled these questions from a different standpoint. This is encouraging. I think that as workers in these other fields become acquainted with HPCT they will see the relationship, and the fundamentally new approach that the concept of control provides (which, intuitively, they may already grasp).

Bill Cunningham's precis of his work rang bells with me in every sentence, especially with his helpful translations into CT terms. The idea of dealing with large numbers of attributes and finding a function of them that minimizes error is especially familiar to me. I used the same underlying concept to develop an image-sharpening program back in the 60s. The same principle, in a different context, became, 15 years later, a program for non-analytic calculation of the impulse-response of a control system; following that was an "artificial cerebellum" that continuously constructs a transfer function that will stabilize a control system, even as the load characteristics change. Now this principle is getting into the Little Man arm demo. I think this same principle underlies the "back-propagation" of neural network fame, and most similar approaches that use measures of outcomes to reach back and adjust the processes leading to those outcomes. Behind all of these approaches is the concept called the "method of steep descent" by which a large set of equations is solved by progressively adjusting coefficients on the basis of measures of nearness to a solution.

And even further in the background is the basic concept of control, applied, often unawaredly, in all these contexts. The basic scheme rests on having some way to specify a desired outcome, and then continously comparing the actual outcome with the desired outcome and translating the error into changes that make the actual outcome different in the right direction.

In my image sharpener, the method was to create a fuzzy image of a point source, and then to use this fuzzy image as a mask that spread the light from each point of a artificial sharpened picture into a two-dimensional intensity distribution. The method adjusted the intensities of the points in the artificial sharp picture so that the fuzzy result matched the fuzziness of the original photograph to be sharpened, as nearly as possible. When this artificially-created fuzzy image matched the real fuzzy image at every point, the artificial sharp picture was (in theory and sometimes in practice) a sharper picture of the original scene. This whole process employed (as I came to understand some years later) a huge array of simple control systems operating in parallel, each one concerned only with matching the intensity of one point in the artificially-fuzzed imaged with the corresponding point on the image to be sharpened.

The transfer-function algorithm worked the same way, but with serial inputs and outputs in one dimension. A trial transfer function was used to transform the real input into a modeled output, which was continuously compared with the real output. A set of control systems each dealing with a progressively more delayed version of the input tried to keep the model's output matching the real output for a particular lag time. When all the control systems were experiencing as small an error as possible, the model's output matched the real output quite closely, and the controlled variables of the control systems, plotted against lag time, were a picture of the impulse response of the system. This method was probably planted in my mind when I learned that spectrograms have been sharpened in one dimension by a similar method that corrects for the finite width of the spectroscope slit.

These are all boostrap processes by which a solution is assumed, transformed into an output, and then slowly corrected by comparing the output of the computation with either a desired output or the actual output of a system being modeled. The logic is exactly that of a control system (or, as it turns out, hundreds or thousands of control systems acting independently in parallel).

I haven't really studied neural network methods of solving problems, but a remark by Hopfield and Tank (or about them, I forget which) has stuck in my mind. They likened their solution of the 7 bridges problem to an analog solution in which the 7 nodes were attached to the solution through rubber bands. The analog circuits, in effect, found where the junction of all the rubber bands would come to rest. Don't ask me how that solved the 7 bridges problem.

Looking on these problems as multidimensional control problems, the concept is very similar. Each degree of freedom is represented by a control system. The least error in all the control systems is reached when the solution is as close as possible to the one that satisfies the reference signals in all the control systems. The corrections might be applied to a complex environment with simple fixed perceptual functions in the control systems, or it might be applied to the perceptual functions in a fixed environment, or both. The possibilities are endless, and I suspect that somewhere in the process of exploring them we are going to find out how human HPC systems become organized.

In the arm model I'm (still) working on, I've succeeded in making the kinesthetic control systems do a moderately good imitation of real human arm movements, including major features of the trajectories of rapid movements. I've found, however, that if I try to make the visual control systems guide all kinesthetic movements while they are happening, the trajectories that result are importantly different from the real ones. The visual systems work with a geometry that's just too different from the kinesthetic geometry. So I've decided to try to incorporate an adaptive map that will correct the kinesthetic report of arm position to make it match the visual report. This requires -- guess what -- a set of control systems that find the best map by a method of steep descent.

Given the kinesthetically perceived arm position and the visually perceived fingertip position, the object is to make these two perceptions the same at all points in the working space. The difference between the two kinds of perception of what is presumably the same thing is used to correct a map so that the difference gradually becomes smaller.

There are two places where this map could be inserted. It could be placed between the output of the visual system and the reference input of the kinesthetic system (which is used by the visual system to position the arm). This would correct the higher system's output effects on the kinesthetic systems.

Alternatively, it could be put in the perceptual function of the highest kinesthetic control system, to alter the way the kinesthetic system perceives arm position. In this case, the visual error output would serve as the kinesthetic reference input directly, but the kinesthetic perception of position would be altered, through the map, to become consistent with the visual perception of position, before the comparison takes place.

This latter way is the way I want to do it, for several reasons. First, it enables the system to make moves toward a kinesthetic goal position on the basis of visual information even when visual feedback is removed during the arm motion. Real people can do this; it would be nice if the model could do it, too. The other main reason for wanting to do it this way looks ahead to a more complex model in which touch information (easy to add) is used as another way of identifying object positions. This would introduce another degree of freedom, and now corrections to the map could be made to achieve consistency among the visual, kinesthetic, and tactile perceptions of arm position in relation to external objects. What I would love to see happen would be that after enough of these different ways of making independent representations of positions agree with each other (altering all perceptual maps with none being preferred), we would end up with a linear perceptual space common to all exteroceptions. So when you tap on the table, the visual position of the tap agrees with the kinesthetic position of the fingertip, which agrees with the tactile location of the bump, which agrees with the auditory location of the sound. When you trail your fingertip across a flat surface, the perceptions of position lie in a perceptual plane surface. And so on. I'm convinced that this is how it's going to work, but I probably won't live to see if it will.

The basic correction algorithm is very simple. The visual perceptual signals (three of them) for fingertip position are compared against the CORRECTED kinestic perceptual signals for arm position, and the difference is used to alter an additive constant at an address in the map selected by the three uncorrected kinesthetic perceptual signals. These constants, one for each degree of freedom, add to the uncorrected kinesthetic signals to produce the corrected signals which are compared against the visual fingertip positions. As the fingertip moves around in space, the control systems doing the correcting gradually alter the constants until in every position, the corrected kinesthetic signal agrees with the visual signal in each degree of freedom.

This system, of course, will be able to recover from having prisms placed before the eyes, and so on.

So it's the same principle again. The output or outcome is compared with the desired outcome. The error is converted into a change in the process that leads to the outcome. The correction method is set up so convergence is guaranteed, eventually. The assumed correction is bootstrapped to become the right correction; the initially assumed outcome gradually becomes the right outcome.

To answer Greg and Pat more specifically: (920420)

>Regardless, what do PCT models look like for, say, controlling for the
>"right" "sound" while writing a story, as opposed to controlling for >the
desired number of units of something or the desired position of >something?

The "right" sound is the minimum-error sound. This is the form of the paragraph in which all the independent control systems, each controlling for one aspect of the paragraph independently of all the others, experience as little error as possible. The overall sense of error, which isn't specific to any one degree of freedom because it's the sum of them all, is what you feel as the vague and general difference between what you have and what you want. This is the "qualitative" sense that the paragraph isn't right yet. But this qualitative sense of wrongness results from a multitude of individual quantitative error signals in many systems, each of which controls one attribute of the paragraph "for the desired number of units of something or the desired position [or state] of something". The nonspecific sense of wrongness can't be corrected without quantitatively correcting the specific perceptions that are in error in specific dimensions.

The reason that the actual control systems have to be quantitative and work on a contiguous number scale is simple. There has to be a systematic relationship between the direction and amount of a correction and the direction and amount of error, or no systematic control is possible. A mathematician could probably state this requirement more rigorously. But intuitively, it's not hard to see that if there's a slight change in the error, this must lead to a slight change in the action and a consequent slight change in the perceptual signal. It's a continuity requirement, and it must also be a monotonicity requirement, at least piecewise. Neighborhoods, or words like that. Why doesn't someone who knows about this stuff pipe up? I'm sure that the same requirements show up in all applications of methods of steep descent.

Best to all, Bill P.

Date: Tue Apr 21, 1992 4:13 am PST
Subject: Quality fruit, indeed!

From Greg & Pat Williams (920421)

>Bill Cunningham 920420.1300 >Bill Cunningham 920420.1530

>Alan E. Scrivner (920420:1305) >Bill Powers (9204.2300)

To summarize these enlightening posts with regard to Roy E.'s original claim that PCT had not addressed how "qualitatitive" (our word, not Roy's) control can work where there is apparently no metric defined: That there is no metric (or at least ordering) for "qualitative" perceptions is simply how it appears naively; metrics (or at least orderings) along independent dimensions are necessary for continuous control; control of a "qualitative" perception is modeled as control of that perception's independent constituent perceptions, each of which can vary only in a single dimension with a metric (or at least an ordering) defined.

The posts also point toward plausible models for how "qualitative" perceptions are related to their unidimensional components. This is getting into the perceptual transformation issues which nobody yet has gotten very far on, but we think the notions of orthogonality in n-space, computation of distance measures, and such could be very fruitful. (No pun intended. Well, maybe.)

Also, Bill's latest work on the arm model is very important because I (Greg) suspect that it will avoid all of the objections which I had previously raised (acting as a surrogate reviewer) -- in spades! Keep at it, Bill!!

Greg & Pat

Date: Tue Apr 21, 1992 8:40 am PST
Subject: program that generates coherent texts

[From: Bruce Nevin (Tue 920421 11:41:23)]

This certainly sounds like it is on the right track. I have sent for a preprint of the paper.

This is part of an extended furor over the "psychological reality" of grammatical rules, or lack thereof--an ancient controversy, of course, though changes of terminological dress seem to have obscured that for some. At one point in the late 1950s the battle line was drawn between "Hocus-Pocus" linguists and "God's Truth" linguists, and Harris was considered the quintessential example of the former type "playing games" with the data. It is only in the '90s that Harris has attributed some of his findings to language and to language users; before, he only claimed that his results concerned properties of grammar (grammatical description). He would agree with Sibun's views expressed here, I think, though it remains to be seen if the "coherence" in Sibun's output encompasses the range of information structures that Harris discloses in texts produced by humans.

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From: Linguist Digest 3.276
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6)
Date: Fri, 20 Mar 1992 15:16:35 PST
From: Penni Sibun <sibun@parc.xerox.com>
Subject: in whose heads are grammars

i previously sent a message to this list stating that grammars are in linguists' heads but there's no reason to suppose that they are in speakers'. despite a poster's assumption that i was being sarcastic, i mean this very seriously, though perhaps i expressed myself too succinctly.

grammars may well be a useful tool for linguists to describe and theorize about language and how people use it. however, just because grammars are a good *descriptive* tool, it doesn't follow that grammars play any role in language *use*. rob stainton's waltz analogy illustrates this point very well.

one way to think about the locus of grammars is to try to model language use and see if grammars (syntactic, discourse, etc.) are necessary or sufficient for producing (or understanding) language. within computational linguistics, there is a small ``natural language generation'' community which builds or designs computer programs that produce (presumably intelligible) text. of course, a computational model doesn't prove anything about the insides of people's heads;

however, if a program can generate text without representing and using grammars, this suggests that maybe people can too. further, if we can agree that we can't find a grammar anywhere in the program, yet we can discuss the output language in terms of a grammar (eg, decide that the output is a grammatical sentence), then we have an example of the grammar being in *our* heads, as linguists talking about grammatical sentences, but not in the ``head'' of the producer of the grammatical sentence under discussion. it is reasonable and appropriate for us to talk about the grammaticality of the sentence; it would not be reasonable or appropriate to assume that it follows that there is a grammar in the program.

for my just-completed phd thesis, i wrote a generation program that produces coherent texts that are up to a page long, without representing or using either a discourse or a sentence grammar. the program's job is to talk about something, and it is continually deciding what to say next. it makes this decision based on things like what it's already said, what it hasn't talked about that is closely related to what it's just mentioned, and what words and linguistic patterns it knows about that might express what it chooses to say. when it has decided what to say it says it, and goes on to the next choice. while the program does not concern itself with syntactic or discourse-level grammaticality in making its choices, its output can be judged as to whether the discourse structure is coherent and whether the syntax is grammatical (the program usually does ok on both counts).

this work and the arguments i present here are described in more detail in an article in the next issue of _computational intelligence_; i'd be happy to send preprints on request.

--penni sibun
sibun@parc.xerox.com

Bruce
bn@bbn.com

Date: Tue Apr 21, 1992 9:07 am PST
Subject: A puzzle about the hierarchy

(From Dick Robertson)
I just came back from a visit with Frans Plooij where he showed me his latest data on confirming the development of a sample of Dutch children through the levels. It's beautiful data, there are no overlaps between regression periods for different kids BUT his data is showing 10 reorgan-ization periods in the first 18 months to two years. QUESTION Then what happens in subseqent development? Frans recalled that some other etholo-gists (or child psychologists, I forget) claimed that development goes through a second round when children begin to symbolize - Might that mean that the hierarchy is first "roughed out" in what Piaget called the sensory motor period and is subsequently re-created a second time in a more abstract form? (For those who don't know Frans, he was the first one to document that chimpanzee & child development goes through Bill's hierarchy stages) His new data also confirms that kids get sick at the reorganization points.

Date: Tue Apr 21, 1992 9:40 am PST
Subject: sci.systems

[from Gary A. Cziko 920421.1220]

Here's information on a new Usenet newsgroup that may have overlapping interests with CSGnet. I don't know if it's up and running yet, but should be soon. I will post to them to let them know CSGnet exists.--Gary
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NAME: sci.systems

STATUS: Unmoderated

CHARTER:

Sci.systems provides a forum for the discussion of the theory and application of systems science. In the broadest sense, systems science is the study of the nature of systems. Such systems can be physical, chemical, biological, sociological, economic, etc. Systems science and system theory can be applied to systems of all types. Systems science as defined here includes mathematical systems analysis, systems engineering, general systems theory, etc. This definition is intentionally vague in order to encourage discussion on all aspects of the study of sytems.

Discussion might include, but is not limited to:

- An overall discussion of the various aspects of the study of systems.
- Discussion of the particular methods for analyzing systems.
- Application of different methods to particular systems.

RATIONALE:

While many existing newsgroups address particular aspects of systems science (such as sci.math, comp.theory.dynamic-sys, or comp.simulation) or discuss the application of systems techniques to particular fields (sci.econ, sci.bio, or sci.engr, for example), no existing newsgroup provides an appropriate place for people from different fields to discuss different approaches to the analysis and design of systems of all types. Sci.systems serves this purpose.

Date: Tue Apr 21, 1992 9:43 am PST
Subject: intros; stats; social/personal

[From: Bruce Nevin (Tue 920421 12:27:51)]

Talking of introductions (and thank you for yours, Cynthia), I wonder if it wouldn't be both useful and pleasant to have a collection of such intros archived so that any of us can go look and say "oh yes, that's who that is." We get a sense of one another over time in the wash of traffic on this net, but I expect that an explicit bio would afford surprises and perhaps new perspectives even for old timers.

On the other hand posing this as a requirement could sure be off-putting to newcomers. Volunteer basis? Older hands have canned bios used with journal articles that could be "personalized" a bit for this purpose.

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Some Looooong time back Martin picked up on my remark about control theory justifying my lack of motivation to study statistics. Let me try to pull that phantom foot out of that phantom mouth that has my name on it. The only place I was exposed to statistics in anything more than the most rudimentary terms was psychology courses. I had no interest in behaviorist psychology as it turned out to be studied and taught at Florida State (Tallahassee) in 1962-3. When I returned from living for two years in Greece, I got into Penn and went right into linguistics, where statistics was rarely a blip on the horizon.

Statistical methods have a role now in Labov's brand of sociolinguistics --what one ABD student of my acquaintance facetiously calls "urban statistical ethnolinguistics". And there are some controversial applications in historical reconstruction. But linguistics deals generally with individual language users rather than populations.

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Or linguistics purports to deal with individual language users rather than populations. There are essential problems in the standard step of abstracting to an idealized speaker-hearer (Chomskyan phraseology, but the step of abstraction is virtually universal in the field for anything above the level of phonetic detail). I think this step is found to be necessary precisely because the structure that can be described in language is social property, not private property, which any given individual knows only in part.

In other words, this step of abstraction to an idealized language user reflects the paradox that I have been trying to understand (and perhaps belaboring), namely, individuals' adoption and control for socially maintained norms or conventions.

To be sure, these norms or conventions only exist insofar as individuals as living control systems maintain them and control for them as internalized reference values for the relevant perceptions. Processes of language death (perhaps more properly "language abandonment") make this obvious. Contact with speakers of another language who happen to own what people are controlling for. Then, in just a generation or two, all that elegant complexity, embodying countless generations of experience, gone like snow on water.

For most speakers of English at least the possibility of such loss is incomprehensible. Yet what is lost? Surely something is. But not the speakers. Even in cases of outright genocide there almost always are individual fluent speakers in the generation or two after their language has ceased to serve as social coin. What is it that was there previously, and is not there for them?

Assuredly, that whatever-it-is (controlled perception of language choice for accomplishing purposes requiring coordinated action with others, whatever) only exists insofar as individuals as living control systems maintain them and control for them as internalized reference values for the relevant perceptions. But more than one person must control for the "same" perceptions, in fact, more than two or a few, and those who do must control a great many social conventions as their individual, private reference values for their individual, private perceptions, or there is not community. There are only individuals, isolated, and feeling a very great loss, the more poignant for being unnameable.

This is like the sea in which we live and move and have our being, as difficult for us to comprehend as water must be for fish to comprehend.

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I will be going back into Pit River country next September for a month of fieldwork on Achumawi, if I can find any speakers. I guess I'm gearing up for the pain that exists in that community. Don't mind me.

Back to work.

Bruce
bn@bbn.com

Date: Tue Apr 21, 1992 10:25 am PST
Subject: grammar w/o rules

[From Rick Marken (920421 11:00)

Bruce Nevin (Tue 920421 11:41:23) gave a reference to a linguistics paper that seems to demonstate one of the points we were discussing some time ago -- that grammar a consequence perceptual constraints rather than an inherent constraint in itself:

>This certainly sounds like it is on the right track. I have sent for a
>preprint of the paper.

It certainly does! I sent for a preprint as well and invited the author (Penni Sibun) to join the list. The description of the program is VERY tantalizing. Thanks so much for the reference Bruce.

Language seems to be creeping back into my life again. There is a two part series in the New Yorker (I think it's the late March, early April issues) on this kid in LA who had been kept isolated from language until she was 12. I am just starting the series -- but it is obviously all about how grammar is innate. I mention it for the sake of the language buffs on the net. If I can stay up for more than a paragraph a night I should be able to finish the article soon. But if I can't, I'd appreciate it if someone who knows more about this stuff (like you Bruce) could summerize the article, critique it and maybe give some background on the major players and their theoretical agendae.

By the way -- for those who are interested -- I tested my "improved control" system (where I add the output of a conflicting controller with a fixed reference to the disturbance to a variable being controlled by a human controller) and controlled for the overall variance of the disturbance with and without the conflicting controller. I did this by simply replaying the disturbance that had the conflicting output added -- so during the first run the disturbance, d, was de+dc (passive environmental disturbance and active conflicting disturbance) and during the second run d was just a replay of de+dc (but now dc was not actively generated -- the sum was the passive disturbance). RMS error with the active control was half the size of the rms error with the replayed disturbance. So the addition of mild conflict really does improve the ability to control. I will try to spiff up the programs a bit for a demo at the meeting. It is an actual practical application of PCT; at least it was discovered accidentally while I was trying to demonstrate another phenomenon of PCT -- conflict.

Regards Rick

Date: Tue Apr 21, 1992 10:50 am PST
Subject: Re: sci.systems

[Martin Yaylor 920421 14:15]

The tide of opinion seems to be running in favour of having a Usenet group tied to CSG-L. If it must be, so be it. But I do hope that the Usenet group is properly constituted, and has a name starting with sci.xxx, such as sci.percept.control. The last thing we need is to be an alt.xxx group or a comp.xxx group, both of which are possibilities.

If Gary does decide to go ahead with the Usenet link, there is a voting procedure on Usenet that must be followed to establish the new group. I don't know the details, but I think there must be a fixed voting period within which 100 more "Yes" votes than "No" votes to create the group.

Martin

Date: Tue Apr 21, 1992 10:54 am PST
Subject: Merging perceptions, etc.

cunningB@monroe-emh1.army.mil

From Bill Cunningham :920421.1415:

So many light bulbs switched on all at once! It's hard to know where to start. It's going to take a while to sort out. I'm a tortoise, not a hare.

Greg Williams--Glad you found fruit appealing. Beware of green persimmons.

The Kanerva approach is flat--no hierarchy implied. Nothing precludes this approach as mechanism for transition between levels in hierarchy. John Gabriel

and I have had lengthy discussions on this. I think we are agreed that PCT provides the construct and that the Kanerva approach provides the best implementing mechanism (or model thereof) we know. Our application requires merger of multiple (sensory) perceptions within an organization to achieve a collective perception at the higher levels. I'm convinced that process mirrors how human actually does it. We (are you there, John?) are about to attack current process model that doesn't include human perception
in either the organization or in the customer receiving organization's product.
Our audience is almost totally unperceptive in that regard. Tracker-correlator
systems, as described by Alan Scrivner, are well known with this crowd--but they tend to define the problem around the mechanics of tracking an aircraft rather than an overall perceptual problem. The good news is that feedback is a politically acceptable term.

Bill Powers (9204.2300)--

Your whole post is a complete turnon. Expect to be quoted liberally. Your preferred crosslink between independent (arm) perceptions is absolutely right! The ability to add/delete sensory percepts is essential and the result you seek will be best able to handle noisy input.

> Behind all of these approaches is the concept called the "method of steep
> descent" by which a large set of equations is solved by progressively
> adjusting coefficients on the basis of measures of nearness to a solution.

Exactly! However, the steepest descent can also be a slippery slope when the input attributes are few (or noisy) relative to the reference set. It is
possible to assign a match in the wrong stability basin, which of course gets reinforced. Now THAT is prejudice. Easy to say get more info before making assignment, but what's enough? Related question is how to recognize slow buildup of evidence that says maybe early assignment was not so good, and then test for alternate matches.

No question that PCT is goint to have major impact on other endeavors, especially if extended to include multiple variables and qualitative judgements. Charge!!!

Bill Cunningham

Date: Tue Apr 21, 1992 10:59 am PST
Subject: Plooijian reorganizations

[From: Bruce Nevin (Tue 920421 14:08:15)]

(Dick Robertson (Tue, 21 Apr 1992 18:21:3))--

<Plooij's work on child development>

I find this very interesting. I see I am going to have to mount another expedition to a library to read some of the things in Greg's bibliography. (Still haven't been able to plunk down cash for the CSG intro to psych book.)

>his data is showing 10 reorgan-
>ization periods in the first 18 months to two years. QUESTION Then what
>happens in subseqent development? Frans recalled that some other etholo-
>gists (or child psychologists, I forget) claimed that development goes
>through a second round when children begin to symbolize - Might that mean
>that the hierarchy is first "roughed out" in what Piaget called the sensory
>motor period and is subsequently re-created a second time in a more abstract
>form?

Don't you mean "began to use language socially" rather than "began to symbolize"? If not, what can you tell me about symbolizing in terms of perceptual control? I am interested because I think this is critical for my getting a handle on how to model language use.

Is it even possible that the second set of reorganizations recapitulates the first set? For example, are there even the same number of reorganizations (10)? That would suggest a kind of mirroring or "shadowing" of non- language perceptions by language perceptions (morphemes, linguistic constructions). That seems unlikely to me, but I have little more than hunch to go on. Or are they perhaps reorganizations in the control of language per se, presuming control of non-language "referent" perceptions? Or some interdependent combination? What do the data show or suggest?

I have this feeling that I am only reiterating your own questions, using some different words. But it sounds like I wouldn't get the findings of these other investigators by just going off to read the 1990 ABS article (sometime).

Bruce Nevin
bn@bbn.com

Date: Tue Apr 21, 1992 12:52 pm PST
Subject: Plooijs

[from Gary Cziko 920421.1305]

Dick Robertson (920421) said:

>I just came back from a visit with Frans Plooij where he showed me his
>latest data on confirming the development of a sample of Dutch children
>through the levels. It's beautiful data, there are no overlaps between
>regression periods for different kids BUT his data is showing 10 reorgan-
>ization periods in the first 18 months to two years.

Is there some way we can learn more about this? Has he written something up that can be mailed or put on the network (he could send me a floppy). I'd love to have some child developmental data which makes sense from a PCT perspective.

Why aren't the Plooijs on CSGnet? Will getting on Usenet make us more accessible to them?--Gary

Date: Tue Apr 21, 1992 1:24 pm PST
Subject: Re: sci.systems

[From Gary Cziko 920421.1544]

Martin Yaylor [sic] 920421 14:15 writes:

>The tide of opinion seems to be running in favour of having a Usenet group
>tied to CSG-L. If it must be, so be it. But I do hope that the Usenet
>group is properly constituted, and has a name starting with sci.xxx, such
>as sci.percept.control. The last thing we need is to be an alt.xxx group
>or a comp.xxx group, both of which are possibilities.

I sent out information on 920415 about these issues. I think I also mentioned in my vote announcement that we would be called bit.listserv.csg-l, a special new category for listserv groups (which is what CSGnet now is) with a gateway to Usenet.

>If Gary does decide to go ahead with the Usenet link, there is a voting
>procedure on Usenet that must be followed to establish the new group.
>I don't know the details, but I think there must be a fixed voting period
>within which 100 more "Yes" votes than "No" votes to create the group.

This does not apply to bit.listserv.* groups, as described in the information I sent. If CSGnet votes "yes" and nobody on bit.admin raises serious objections, we can have a gateway.--Gary

P.S. Martin, if you don't have the info I sent, I would be happy to send it again to you. Let me know via personal e-mail.
Gary A. Cziko
Date: Tue Apr 21, 1992 2:53 pm PST
Subject: Re: Qualitative control; methods of steep descent

[Martin Taylor 920421 18:30]
(Bill Powers 9204.2300) 920420?

>These are all boostrap processes by which a solution is assumed,
>transformed into an output, and then slowly corrected by comparing the
>output of the computation with either a desired output or the actual output
>of a system being modeled. The logic is exactly that of a control system
>(or, as it turns out, hundreds or thousands of control systems acting
>independently in parallel).

The analysis seems rather like that of a super-resolving antenna, which works fundamentally because of assumptions about what could cause a particular field. I think it is also what people do, making assumptions about the kind of percept that they will receive, and using data that are theoretically inadequate to produce a percept that is usually pretty accurate, but is sometimes horribly wrong.

>In the arm model I'm (still) working on, I've succeeded in making the
>kinesthetic control systems do a moderately good imitation of real human
>arm movements, including major features of the trajectories of rapid
>movements. I've found, however, that if I try to make the visual control
>systems guide all kinesthetic movements while they are happening, the

>trajectories that result are importantly different from the real ones. The
>visual systems work with a geometry that's just too different from the
>kinesthetic geometry. So I've decided to try to incorporate an adaptive map
>that will correct the kinesthetic report of arm position to make it match
>the visual report. This requires -- guess what -- a set of control systems
>that find the best map by a method of steep descent.

Interesting that you would make the mapping in that direction. I would have thought it should go the other way, since adaptation to visual distortion is quick when active kinaesthetic checking is possible and essentially does not occur when only passive movement (e.g. being wheeled around in a wheel chair) is possible. The control is in the possibility of acting upon the environment. The eyes can't do that, but the fingers can. I should think that if you want to develop the Little Man toward a model of live perception, then you might want to take this into account for future developments in which the Little Man can feel and push the target rather than just seeing it and pointing to it.

I do believe that it is the control of perception that allows us veridical perception (such as our philosophical predispositions permit us to claim). You can do a little of this in vision if you move your head, but you need some active calibration (kinaesthetic, I would guess) that allows you to perceive where your head has gone to, independent of vision, so that you can correct any visual distortions. (I'm really parrotting JGTaylor, as you can guess, but I think he's right; it makes logical sense as well as being experimentally verifiable).

Martin

Date: Wed Apr 22, 1992 3:23 am PST
Subject: Plooijs work & language learning

(From Dick Robertson) (sorry I cant download to quote your posts)

--Bruce Nevin: you asked about whether I meant "begin to use language socially rather than symbolize," I'm not sure I have an accurate conception of "symbol-
ize." I don't see it restricted to what we externally regard as signs for con-
cepts. I hunch that the earliest kind of "symbolization" might be a perception
of a whole process (felt, seen, heard-imaginatively) as the RS of program call-
ing for a new value of some sequence-, category- or relationship-component.

You see babies seeming to make an "Oh yes, that" recognition and reach for an object you've shown before and then brought back--and then whimper and wave the
hand in the direction you made it disappear, if you again remove it (around 3 months, I think). I've always imagined that eventually words come to replace that hand waving, straining toward which I take as the first indication of some
thing being internally represented - symbolized. Maybe I'm way off the mark.

Anyway, I thought your idea that the second round of reorg might recapitulate

the first round - seems to make sense, but I have no idea how many levels would
be involved, if that were the right idea. There is a powerful lot of important
research waiting here for some bright young PH. D candidate and it should tie together Plooijs kind of ethological developmental psych with linguistics.

-- Bruce Nevin #2, I'm going to repeat my request to you, Bruce, about whether there are lists of most-common-sentences, like the lists of most common words that have been developed for various languages. I didn't get a response to my first request-maybe it got garbled-but as I watched my own struggles in trying to deal with French here, I thought I noted that a major problem was in not hav
ing time to recognize sentences by putting the words (which I knew) together.

Time after time, what the person had said came to me after they had walked away.

I met a linguist here, Peter Kelly, who has developed a manual using equivalent
French and English sentences, instead of words to teach English to French science students with good results, but it doesn't necessaarily include the most common everyday sentences from both languages.

--Gary Cziko: You asked why Plooijs arent on the net. Believe me, they want to be. Frans has just received a one-day a week chair at Gottingen and hopes there to be able to do email. They dont provide it to him at Amster Pedagogish
But tomorrow, in my last post from Belgium, I'll give the references to the 2 new articles he send along with me. They are dynamite in my opinion.

Best to everyone Dick R

Date: Wed Apr 22, 1992 8:13 am PST
Subject: Dick R

[From: Bruce Nevin (Wed 920422 08:35:07)]

(Dick Robertson (Wed, 22 Apr 1992 12:34:22)) --

Agreement about the importance of symbolizing and that we don't know how to say much more about it than that, yet.

I apologize for not responding earlier to your query about lists of most common sentences. It came when I was most snowed by work here. Certainly there are dictionaries of idioms, but they are problematic because not all idioms qualify as common or frequent, and in the nature of idioms they are to a degree anomalous, that is, they are exceptions to the most productive structures of sentences.

I think you need to focus on the most common sentence types, and practice with examples of them using vocabulary that *you* (i.e. the student) need to control. Lots of older pedagogical texts (especially of the Charles Fries era) worked with substitution of words in frames.

However, their choice of vocabulary/subject matter rarely coincided with perceptions that I was interested in controlling (the old la plume de ma tante problem).

A fruitful way of working might be something like the following:

> Record a brief conversational exchange, the controlling of which matters to you (say, asking for directions).
>
> Abstract by replacing key words with blanks
>
> > Ou est/sont l__ __________?
>
> Try out a rendition on someone willing to help you.
>
> When your rendition is corrected, determine by further substitution whether both constructions are productive (over complementary sets of vocabulary), or one is restricted to certain vocabulary or idioms.

There are three concepts to distinguish here: frequent, productive, and important. You would expect that an exceptional, nonproductive construction or idiom would by definition be "less frequent." But if you must control it in order to control other perceptions that matter to you, then for you it is more important. I suppose you could say it is in more frequent demand for your speech as an individual, but no book based on statistical studies of usage can possibly help you there.

If a construction is productive, you can substitute a wide range of vocabulary. A productive sentence type like the passive:

    The ___ was ___-en by the ___

Compare this with an unproductive idiom like:

    take the bull by the horns

Surely you can substitute "goat" for "bull" or "tail" for "horns" but by so doing you lose the idiom by participating instead in a productive construction:

    take the <noun> by the <noun>

Productivity is a measure of word combinability, not of frequency. You can say that the passive is used more frequently in scientific reports, and that is completely independent from the notion of productivity. Either frequency or productivity may have a bearing on what is important to you and what you therefore should select for practice. Subject matter determines what vocabulary is important for you. Idioms and frozen expressions are in a sense types of vocabulary items (the more so, the less they participate in productive sentence-differences).

Yes, it is best to work with whole sentences rather than individual words. The reason is that you learn what *kind* of word to expect next. One way to look at this is in terms of a stochastic process.

The set of possible next-successor word types becomes progressively restricted as more words are input, going up at sentence boundaries (less so at clause boundaries and phrase boundaries). Another way to look at it is in terms of word dependencies. This verb can only be said if argument words of appropriate classes are also said, or if conditions for elision of those words are met. A third way is to see these dependencies as reflections of dependencies among nonverbal perceptions associated with the words. I have argued that it is a combination of linguistic and nonlinguistic dependencies.

Again, I am sorry I don't have a specific reference. Perhaps this indicates why I am skeptical that you will find a completely satisfactory list of "most common sentences" analogous to lists of most frequent words. I could be wrong. Joel may have some references, he certainly knows the literature far better than I.

One last thought that is related to the whole-sentence idea. An old friend and classmate, A. I. Moscowitz, developed Charles Ferguson's idea that adults should learn in the same stages as children, beginning with babbling nonsense syllables using the foreign phonemes. Important is that the babbling is a vehicle for learning and practicing intonation contours and segmental contrasts before grappling with individual word substitutions in the sentence-frame provided by the intonation contour. This is precisely where foreign accents show up, because adult learners lock in on incorrectly calibrated reference values for these lower-level perceptions, for the sake of controlling words and meanings. Also, because of focussing on words and meanings, they stay with native-language reference values for word order, ranges for word combination, and idioms. For these latter problems, the approach outlined above might help.

Bruce
bn@bbn.com

Date: Wed Apr 22, 1992 8:18 am PST
Subject: straw man?

[From: Bruce Nevin (Wed 920422 09:37:36)]

(Greg Williams (920409)) --

I like your analogy:

Paley (Design) : Darwin (selection) : William Bateson (Mendelian mechanism)
Watson (S-R) : Skinner (operant) : Powers (PCT)

>Darwin and Skinner got part way there [from Paley and Watson, resp.],
>but the (underlying!) mechanism was missing in their theories.
>Bateson and Powers publicized the missing (and essential) "pieces."
>Note that Bateson emphasized the importance
>of the "piece" he rediscovered for COMPLETING the partial theory of Darwin,
>and neo-Darwinism took off quickly.

I won't quote the rest of this brief post, only say that it seems to me to be a critically important point. If Greg is right, then to paint with the S-R tar brush all psychologists who don't understand control is to paint yourself into a corner. A cozy corner, maybe, and by that I mean to suggest that the most insidious conflicts are those the accommodations to which go unnoticed.

Bruce
bn@bbn.com

Date: Wed Apr 22, 1992 10:08 am PST
Subject: intro;foundations

[From J Francisco Arocha, 920422, 1101]

This is the second time I have the opportunity to sent a post to the net in the several months I've been a subscriber. Since the first time was long ago, I'll again introduce myself. I'm an educational psychologist whose main work so far has been on problem solving (especially medical problem solving) and a little bit on reading. I got to know about PCT through the net and became interested because of my disatisfaction with standard behavioural and cognitive research and because I completely agree with the methodological criticisms of (statistically-based, average-based) psychology made on CSGnet. During these months I have slooowly read Powers' 73 and Runkel's books as well as several articles in various journals.

One of my concerns is with the philosophical foundations of psychology. Now, I would like in this introduction, to "situate" myself by telling where I'm coming from, in philosophical matters, something which I consider necessary (spelling out the foundations of psychology or any other scientific discipline, that is) in any starting discipline (as I think psychology is). I guess the goal of this post is to suggest a topic of discussion (of interest to me) and to learn from CSGnetters about the foundations of PCT. For this I will start by describing, in a very simplistic way, my understanding of behavioural sciences in the last 70 years or so, starting from the behaviouristic "revolution". I apologize if this post extends a little bit too much.

Behaviourism was a revolution in psychology. It was a revolution because its proponents advanced psychology as a discipline that could be studied scientifically, in the same way that chemistry or physics are studied scientifically (i.e., objectively). More precisely, behaviourism was methodologically revolutionary, if not theoretically. Unfortunately, the main philosophical framework at that time (especially the 20's through the 50's) was very much influenced by empiricism and operationalism. This philosophical framework (positivism), however, was a constructive effort to develop the basis of science on solid ground. That it failed it is now widely known. But it did because of the commitment to observables as criterion for meaningfulness and the suspicion of theoretical concepts as being "metaphysical". It is also widely known that the picture of the sciences, especially physics, was inadequate and in many instances simply wrong (e.g., their suggestions that scientific concepts are observational, that there are rules for reducing theoretical concepts to empirical concepts).

Later on behaviourism was attacked because it did not take into account "internal processes"; because it left out the most important portions of mental life. Now talking about cognitive processes and strategies, and investigating them was permitted and encouraged by the psychological establishment. So far so good. Concurrently with the "cognitive revolution", Artificial Intelligence was giving its first steps. In the beginning, the relation between AI and psychology was based on the "the-computer-is-a-kind-of-brain" metaphor. But, probably because computer science developed much rapidly than psychology, the metaphor was reversed to "the-brain-is-a-kind-of-computer". So what at the beginning was conceived as simulation of brain processes in the computer, later became an "abstract identity" between cognitive processes in the brain and programs in the computer. Cognitive processes became "instantiations" of "formal machines".

What is in principle a fair criticism of behaviourism (i.e., that cognitive or internal "happenings" have a place in psychology) became, it seems to me, muddled in the confusion between formal descriptions (e. g., a program, an algorithm) and the factual referents of those descriptions; in other words, between the model of the thing and the thing being modeled. It is now common, for instance, to talk about means-ends analysis or back-propagation as "mechanisms". But these are not mechanisms but formalisms, and therefore abstractions. Some psychologists have even argued that physiological psychology should separate itself from biology and that psychology should be concerned with the study of "pure function"; an abstract discipline of the same kind as mathematics. Plato wouldn't have been happier.

By perusing several journals I sense that cognitive science, has been gaining acceptance among psychologists. With this acceptance, there may be the implicit acceptance also of these philosophical ideas as the basis for psychology. So far as I can tell from my readings, PCT is not aligned to the idea that psychological processes are instantiations of abstractions. Moreover, I can see PCT as a unifying theory of cognitive psychology that is in close contact with other biological and social sciences without failling into the Platonist confusion. However, I would like to know the opinion of PCTers about this. Again, I apologize for the length of the post and hope someone may respond to my concerns.

****
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****

Date: Wed Apr 22, 1992 10:33 am PST
Subject: intros

[From Rick Marken (920422 11:00)]

Bruce Nevin (and some others, I think) suggested that it might be worth-while for CSG-L members to voluntarily introduce themselves. I agree. I think it might be nice to know what led people to their interest in PCT. So I'll start the ball rolling (if it rolls at all):

I am trained as an experimental psychologist. I received my PhD in 1973 from UC Santa Barbara for a thesis on "Temporal integration in auditory signal detection". I taught psychology for 11 years at Augsburg College in Minneapolis and I currently work as a human factors/systems engineer at Aerospace Corp. My first encounter with PCT was in 1974 when I saw Powers' book on the library shelf. I was intrigued by the title but did not start reading the book until 1976 -- when I was trying to prepare a talk explaining what was wrong with the concept of solving human problems by controlling behavior (the Skinnerian solution). I realized that conventional psychology (including the then "state-of-the-art" cognitive psycholgy) had no scientific answer to this question. Cognitive psychology just said you can't do it because people are too smart. That's an explanation?

By 1978 I was starting to understand PCT. This was the result of a happy coincidence:1) the publication of Powers'1978 Psych Review article which described experiments illustrating the principles of PCT and 2) the invention of the personal computer. I immediately had the College purchase a couple of Apple IIs and I started doing the experiments described in the Psych Review paper (all you needed was a game paddle and the ability to program).

By 1981 I understood PCT (obviously, I'm not a quick study -- my only excuse is that learning PCT requires unlearning a lot of "conventional wisdom") and I also understood that PCT meant the end of conventional psychology (for me, anyway). Coincidentally, 1981 is the year I also published a textbook on research methods in psychology. I consider it my "swan song" to conventional psychology.

Even after developing an understanding of basic PCT, I still had trouble answering questions about PCT from conventional psychologists; questions like "How does PCT explain [put your favorite behavior here]"? I was able to come up answers -- but it seemed like something was wrong. I finally understood the problem -- conventional psychologists and PCTers are not talking about the same thing when they talk about "behavior". They are not dealing with the same phenomenon. Conventional psychologists think of behavior as caused output (where the cause can be inside (cognitive) or outside (behaviorist) the behaving system). PCTers think of behavior as

CONTROL.

I mention this point because many people are attracted to PCT because
they like the theory -- and Powers hierarchical control system model IS
a pretty theory (in my opinion). But I think there is disappointment
ahead if you like the theory but assume it is an attempt to explain
some conventional phenomenon. For example, most experiments in
conventional psychology report the effect of independent variables on
dependent variables. This "effect" is presumably the phenomenon to be
explained. From a PCT perspective, this kind of "phenomenon" is, at best,
a side-effect of control (response to disturbance) or, at worst, irrelevant.
Looking for a PCT explanation of this kind of data (which is, I'm afraid,
most of the data in the social sciences) is like trying to explain levitation
performed by a magician using Newton's laws. The problem is that you are
taking it for granted that what you see (S-R relationships, woman floating
in the air) is as it seems (S causes R, woman is unsuspended). PCT says
that what you see may not be what it seems. The application of PCT must
be preceeded by a convincing demonstration that you are, in fact, dealing
with an example of control (purposeful behavior). And the way to become
convinced of this is through the formal or informal use of the "test
for the controlled variable". This test is really the guts of PCT. My
advice, as an "old hand" at PCT is to first learn about the phenomenon
of control; learn how to demonstrate to yourself (and others) that a
variable is indeed controlled. Once you understand the PHENOMENON of
control, you can start working on the explanation of that phenomenon, PCT.
Theory is great -- but phenomena must not be neglected. The theory
helps us know what to look for -- "the test for the controlled variable"
is based on the theory -- but then you must look and convince yourself
that people really are controlling variables (even when it appears that
they are responding to stimuli or generating responses).

Hasta Luego Rick

Date: Wed Apr 22, 1992 11:20 am PST
Subject: The topology of perception

>From Alan E. Scrivner (042292:0730)

>Greg & Pat Williams (920421)

>...That there is no metric (or at least ordering) for "qualitative"
>perceptions is simply how it appears naively; metrics (or at least
>orderings) along independent dimensions are necessary for continuous
>control; control of a "qualitative" perception is modeled as control
>of that perception's independent constituent perceptions, each of
>which can vary only in a single dimension with a metric (or at least
>an ordering) defined.

>Bill Powers (9204.2300)

>But this qualitative sense of wrongness results from a multitude
>of individual quantitative error signals in many systems, each of which
>controls one attribute of the paragraph "for the desired number of units of
>something or the desired position [or state] of something". The nonspecific
>sense of wrongness can't be corrected without quantitatively correcting the

>specific perceptions that are in error in specific dimensions.

Thank you for a flash of CSG-type enlightenment and let me carry this line a few steps further. What I found in my self-organizing network experience, was that the neural network forced a metric on the input space quite different from the usual (Euclidean) metric. Points that are nearby in the euclidean sense may or may not be "close" together in terms of the induced coordinate values they are assigned by the mapping. By overlaying the input points with their assigned output values an interesting metric emerges. Also, this induced metric was more suggestive of a non-euclidean (e.g. Riemmanian) geometry in that the metric was a function of the space and generally a tensor rather than a scalar.

But, the flash that just went through my mind was that this quantitative vs. qualitative problem could be solved using Zadeh's extension principle. Trying to map "rightness" or "wrongness" onto the real number line for the sake of having continuity and therefore convergent control wasn't setting too well.

Zadeh's extension principle:

```
  Def: Let X be a Cartesian product of universes X1,...,Xn. Let A1,...An,
       be n fuzzy sets in X1,...,Xn (resp.). Let the function f be a
       mapping from X into another universe Y. i.e. y=f(x1,...,xn). Then,
       a fuzzy set B in Y is defined by:

       B = {(y,Gb(y)) : y=f(x1,...,xn), (x1,...xn) elements of X}

       where

       Gb(y)= { sup min{Ga1(x1),...,Gan(xn)} if f inverse not = 0;
              {                              (x1,...,xn) in f inverse of y
              { 0                            otherwise

       here sup is the least upper bound. Gb(y) is the membership
       function of y in B, Gan(xn) is the membership function of xn in
       An.
```

Recall that a fuzzy set is an ordered pair consisting of an element and the value of its membership function. Also, sup min{...} is the fuzzy metric and with it we can satisfy all of the requirements for a metric space and do the kind of calculus required for control systems to converge, etc.

I found myself a footpath between the worlds of the psychologists and the mathematician. Thanks All!
==
Alan E. Scrivner ms54aes@mercury.nwac.sea06.navy.mil
==

Date: Wed Apr 22, 1992 2:57 pm PST
Subject: Re: intros

[Martin Taylor 920422 18:00]

OK, I'll unmask as well...

I started as an Engineering Physicist at the University of Toronto, in Communication Engineering (1952-6). I wasn't very satisfied with engineering, because it seemed that after one job you were back to square one and could build on nothing. My Bachelor's essay was on applications of Information Theory, with the main sections being on Art history and on Economics. I think that what was known then would have been enough to prevent the current disaster that Western economists have caused in Eastrn Europe, but as far as I am aware, present-day economists totally ignore the informational constraints
on an economy. They live in a fantasy-land in which they can declare PI to be 3 if they want, and then they declare that the Russians ought to believe that PI is 3 in the real world.

A friend of my father, having read my essay, thought I might like to become either an Industrial Engineer (Operations Research) or an Experimental Psychologist, and said that Industrial Engineering was closer to what I knew. So I tried that and didn't like it too much (M.S.E. in Operations Research, the Johns Hopkins University, 1958) and gave psychology a whirl, coming to this laboratory, where I now am, as a summer student in 1957. I found that my Engineering background was perfect for perceptual psychology, because what are the sensory systems doing but manipulating information and extracting the useful messages out of it? So I finished as a psychologist (Ph.D. Johns Hopkins 1960). During this 2 year period I met my current wife, a psycholinguist from Korea, which started my interest in psycholinguistics (another information-processing problem, right? When you have a hammer, you've
got to hit nails!). I also had Wendell Garner as advisor, a man I consider a genius in the analysis of information and its application in perceptual psychology.

While I was an undergraduate, I got involved in computers, and spent one summer as maintenance technician on the first one the University had (an English Ferranti Mark 1, called FERUT). Computers have been my other thread since then. When I got bored with psychology, I became a computer scientist, and at one time was international chairman of DECUS (the DEC computer users' society). In recent years, I have combined the two threads in an interest in human-computer interaction. That's (officially) what I do now.

Shortly after I started work full-time here, I was asked to review a book: "The Behavioural Basis of Perception" by J. G. Taylor (1962) (no relation). I thought this a seminal book, and my view of psychology has not been the same since. Taylor claimed that we only learned to perceive those things that we could affect and control through behaviour. Distortions of perception induced by, say, prism spectacles, would be quickly compensated in perception if and only if we could behave with respect to the objects in the distorted field. Anyone, even a sighted person, could learn "blind-sight" (actually using sound echoes), provided that they could manipulate or move with respect to the objects in the world. And his experiments demonstrated the validity of his claims.

In my current view, JG shows how ECSs are developed.

About 10 years ago, I started to develop what I called the "Layered Protocol" theory of communication, which has much the same background premise as PCT: one partner has a goal that the other should have some piece of information, and uses whatever means is appropriate in a hierarchy of virtual message systems to send it. Each virtual message subsystem uses feedback to determine what the partner has received and to control the ongoing form of its message so that the correct message is eventually received and incorporated into the ongoing higher-level message.

Also about 10 years ago, my wife and I wrote a book on the Psychology of Reading (Academic Press, 1983), in which I proposed a theory that integrated symbolic and distributed processing as parallel streams, rather than having one as a front-end for the other. This theory colours my current view of PCT, as I regard the "single ECS to control a single percept" approach as analogous to symbolic AI--crudely functional but brittle. I prefer to see it embedded into a "many ECS to control a small range of percepts" distributed hierarchic control system. Such a dual system would, I think, be both robust and powerful.

How did I get involved in CSG-L? Cliff Joslyn suggested on the basis of something I wrote in another newsgroup that I would enjoy the CYBSYS-L list,
and on that, someone mentioned CSG-L, which I joined last February (1991).
I do think PCT is a "correct" approach, because it builds on a principle that has to be true.

For me, CSG-L is a working group, in which serious problems are discussed and the state of the art advanced. This is why I oppose(d) opening the group to free discussion on Usenet. I do not see how advances can be produced unless the participants can understand and agree to successive steps in the development, and I do not see how that mutual understanding can progress in a large Usenet group. But we shall see.

Martin

Date: Wed Apr 22, 1992 3:26 pm PST
Subject: Re: The topology of perception

[Martin Taylor 920422 18:50]
(Alan E. Scrivner 042292:0730)

> What I found in my self-organizing network
>experience, was that the neural network forced a metric on the input
>space quite different from the usual (Euclidean) metric. Points that
>are nearby in the euclidean sense may or may not be "close" together
>in terms of the induced coordinate values they are assigned by the
>mapping.

Are you talking about Kohonen nets or something else when you talk about this mapping?

I am not sure that categories can be embedded in a metric space at all, inasmuch as the category membership is dependent on more than the incoming data. There is not a one-to-one mapping from the (metric) sensory data space to the category space. I model the category space as a sequence of cusp catasrophes, in which each branch of the cusp may itself branch into a subordinate cusp. I know this is an oversimplified view, but in itself it makes the notion of metric mapping a little suspect.

I have no real notion how to formalize this.

Martin

Date: Thu Apr 23, 1992 8:20 am PST
Subject: Plooijs references, more on re-reorg & thanks

(From Dick Robertson)
First, here are the references to the Plooijs recent work: Infantile Regression
s: Disorganizing & the Onset of Transition Periods - Plooij & Plooij, Jour of Reproductive & Infant Psychology, v 10, 00-00, 92

- Distinct Periods of Mother-Infant Conflict in Normal Development: Sources of Progress & Germs of Pathology The Journ. of Child Psychology, in press.
- Frans is still writing the third in the series on his confirmation of infant-
ile illnesses in the first two years coming mostly at the transition periods.
TO BRUCE NEVIN Thanks for your suggestions about the language learning. I in-tend to keep working on my French, for the fun of it and because there is some interesting stuff that has never been translated AND because I work on my own understanding of PCT as I watch myself learn new stuff. That brings me to the next topic

ANOTHER THOUGHT ON RE-REORGANIZING - I felt a little stupid yesterday after I recognized that I might have sounded like I was saying I thought symbolizing
s
starts before the age of one. What I was trying to express is the idea that maybe symbols start like icons - recollection of a perception consciously as it
is called up as a reference signal as the course of action is being changed. It
might be the power of suggestion, but after discussing the idea that maybe reor
ganizing recapitulates the hierarchy, or some of it, in a more abstract way, after the first round, I began recalling instances of the hierarchical form that my perceptions seemed to take as I learned to drive between here and Brus-
sels. At first I followed my guide J F Botermans - I think he recognized that I would often see the road signs too late in rush hour traffic to change lanes,
next several times I did get lost briefly but was beginning to recognize what certain critical points looked like. Still later I was amazed at the number of
details that began to fill in, in sequential order. Before that the stream had
not broken into any distinct nodes. Only after all that could I have put down directions of road signs, filling station-corners, etc to give someone else a set of instructions. If the last were fully "symbolic" then it seems to me that there was a series of successive approximations leading up to that point that at least vaguely resemble (for me) the levels in the hierarchy (and I find

new language learning fairly analogous to that process). All this reminds me that when Bill Powers and the 2 Bobs published their first version of PCT in 19
60 there were only 7 levels, but they had postulated several "modes" in the top
level that they thought resembled the previous stages in some fashion. I had forgotten that until now, but my observations of stages of reorg in the learn-ing experiences I described had seemed to me to be at the system concept level in the sense that they broadened my "theory of reality" but had to be built through the successive levels of abstration that I mentioned.

-INTRODUCTIONS I liked Rick's & M Taylor's intros, it seems a good idea, so I will add a brief one on myself. Took my Ph D in Hum Devel at U of Chicago in 1960 & had included training with Carl Rogers in Cl Centerd Cslg. Around 57 or
58 3 guys gave a lecture at Rogers place that changed my life. I can honestly say I had been a rather mediocre student in my psych courses because SR seemed internally inconsistent to me & I kept wondering what goes on in the brain when
an association is formed (I'm still wondering that-though I offered a hunch in the textbook) Anyway as soon as I got my degree I volunteered a day a week for two years to learn the theory from Bob McFarland and Bob Clark (are you there, Bob? It was good to hear from you a while back). Otherwise I worked as a clin psychologist in a rehab hosp & finally as a rehab researcher with the US Vets Q
Admin before going to teach psyc at Northeastern Il U. I found research the most exciting type of activity, but I was discouraged to find how little appli-
cation psychotherapy and rehab research have in practice. Phil Runkel's two types of research scheme was a revelation in that respect & seems to me to shoz
why that was. So I kept in touch with Bill a little over that dry spell & when
he published BCP in 1973 I started teaching it to a couple of students. I have
never been able to reach more than a handful of students at a time, but they have always been the greatest. Finally when Bill thought we ought to have a society I made my modest contribution by finding the first place we met, next I
thought we should re-write a general intro to psychology on a PCT basis. So, the textbook. That gave me the wedge to nag my department at NIU until now, we actually have TWO intro psych courses (one based on "the truth" I'm kidding a little!) Meanwhile I raised four boys, mainly thanks to my wife, Vivian, & now have four grandchildren to check out with Plooijs findings.

- SO LONG FOR ME FROM BELGIUM J F Botermans, whose account Im using, will keep on the net and I hope we see some more contact with some of these good people here. Cheers, Dick R

Date: Thu Apr 23, 1992 11:30 am PST
Subject: Two-way connection

[From Kent McClelland]

I'm registering a belated yes vote on the Usenet 2-way connection.

No time now to comment on several interesting recent posts.

Kent

Date: Thu Apr 23, 1992 12:40 pm PST
Subject: A wordy reentry to netland

[from Joel Judd]

THIS MAY BE LONG BUT DIFFERENT PARTS REFER TO DIFFERENT RECENT COMMENTS SO READ ON--YOU'RE SURE TO FIND SOMETHING INTERESTING. THE BIO'S AT THE END...

First, a cute reference before I return the anthology. I ran across this while looking for something else a while back. It's only seven pages long and refers to the disillusion many (some?) have had with the "cognitive revolution." He criticizes both Chomsky-type rule-guided behavior and the epitemological problems of linguistic knowledge from a Chomskyan/Lenneberg perspective that Mark Bickhard has taken up in the last decade. The author, Malcolm, is listed at the time of publication (1971) as being at Cornell. Anyone heard of him lately?

Malcolm, N. (1971). The myth of cognitive processes and structures. In T. Mischel (ed.), _Cognitive Development and Epistemology_(pp.385-392). NY: Academic Press.

****
D. Robertson/B. Nevin (920422):

I have used Spanish/English frequency dictionaries in the past. One of them was rather old (circa 1948). There aren't any really recent ones--the idea has sort of fallen out of favor since the decline of Contrastive Analysis. I don't have the refs handy but could get them if you want.

You might take a look at some of the Competition Model literature (Brian MacWhinney/Elizabeth Bates) and Russ Tomlin's work, since they have been doing cross-linguistic stuff and are interested in characterizing languages in part by their argument/word orderings.

The type of sentence practice Bruce mentions was the cornerstone of the audio-lingual methods which sprang up in the 50s and 60s. I am sure pedagogical materials from then, or modernized versions, would have something you are looking for. I know there have been (and still are) rather iconclastic meterials that purport to make you an overnight speaker of language X if you only learn the following 40 or 80 or 150 sentences. Something like that might be worth comparing with what you're looking for. This reminds me of the Korean supervisor of the language program I taught in in Korea who had a list of 150 English sentences. He was convinced if we woulf teach these to the students we could dispense with all the reading and discussion and other activities we were doing.
****
Re: Plooij data (920421):

From the start I have been interested in anything which sheds light on what development might be going on in children. We will never have a good handle on L2 learning without understanding the development of language in the first place (including the development of more than one language concurrently).

My first inclination in thinking about development was to assume that the levels of the hierarchy developed one at a time, until about puberty. The more I found out about the interplay of the levels, however, the more the time frame seemed to get pushed back towards birth. If one considers the evidence just for phonetic accomplishments, ECSs necessary (at least up through EVENT level) for their control have to be in place within months of birth. But if one believes that there is an overall _socialization_ process driving the infant's activities, as some child psychologists are coming to recognize (or re-recognize?), then does this not argue, by definition, for some elemental versions of high level perceptual control also very early? We have generally included such goals as 'be part of this group' as something like CONCEPT. I don't have a clue how one would ever convincingly demonstrate this. But I would not be surprised if a rudimentary perceptual hierarchy forms very early--by 3 or 4, and then is mostly re-represented (maybe not a good term) linguistically/communicatively over the several years leading to adult-like behavior.

How's that for speculation?
****
Somebody asked for an abstract last week:

Second Language Acquisition as the Control of Non-Primary Linguistic Perception:
A Critique of Research and Theory

The field of second language acquisition (SLA) has, in the last forty years, been one of the fastest growing branches of the social sciences. Today, aspects of non-primary language teaching and research are studied the world over. Advances in communications technology and geopolitical developments have served to increase public awareness of the importance and role of language in our lives.

Unfortunately, our understanding of the psychology of human language and behavior has not kept pace with these historical developments. As a result, the field of SLA has been employing research methods typical of the social sciences in general and psychology in particular, which methods are based on the statistical analysis of groups. While providing more and more data about the linguistic behavior of groups, they offer little understanding about how any one person learns another language. These methods are a result of psychology's emphasis on explaining behavior as a linear, predictable process.

In this thesis, it is argued that the main questions of interest in SLA are questions of individual language learning: How does one learn another language? How are different languages organized in the brain? Therefore, problems arise when methods of group statistical measurement are used to extrapolate to the individual. A review of studies used as evidence for key SLA hypotheses supports this claim. It is then argued that in order to determine an appropriate research methodology for SLA, a different theory of language and language learning is needed. For this purpose a current, general theory of human behavior as purposeful and goal-driven is offered as relevant to the field of SLA. With such a perspective, a more useful and accurate model of behavior and language is possible. Also, important ramifications for current conceptions of teaching are explored.

****
INTRO:

I'm still young enough I don't mind saying I was born in California in1960. Music was my thing through my first year in the university, when I took two years off to serve a proselyting mission for my church in Peru. At that time I had to learn Spanish. It was the beginning of my interest in language, language learning, and just plain understanding people.

After returning, I tested out of as many Spanish grammar classes as I could, and finished a degree in Spanish, with a minor in Linguistics. Though the school was just the local state university campus (San Jose State) it had marvelous professors, and I came into contact with an ex-international lawyer who showed me how intricate and wondrous language can be (particularly Spanish), and John Lamendella, a linguist who had been collaborating with Larry Selinker (of "Interlanguage" fame), who turned me onto the world inside the head. I've been trying to understand language in the brain ever since.

Next stop eastward was Provo, UT and Brigham Young University. I turned to a program in Teaching English as a Second Language (TESL) as a practical matter, realizing that it would be harder to market a Spanish degree than a TESL one (hmmm, wonder what he was controlling for?). My thesis was a dichotic listening study of two groups of Spanish learners--one from a classroom and one just returning from a mission. I finished this degree without much fanfare, and was able to start getting teaching experience during this time. Upon finishing and having no work, I took the first thing that came along, and ended up in Korea. The country was beautiful, but the job a nightmare, as the (foreign) teachers and (native) administrators constantly battled each other over teaching practices and philosophy. I learned a lot about what I would and wouldn't accept in teaching from this experience.

Moving eastward yet again, I found myself amidst cornfields in a Big Time (and Big Ten) university. For the first three years I tried to get an evoked potential (ERP) study of bilingual reading off the ground. But in the fall of '90 it all fell apart. At this same time, though, this professor named Gary Cziko had been looking for enlightenment and suggested I subscribe to this fledgling e-mail group and read a couple of articles about "Control Theory." Since then I have found important insights to my interests in other works by people on the net such as Rick, Hugh Petrie, Ed Ford (especially the counseling/teaching similarities), Tom, Bruce, Bill and others, as well as Philip Runkel, Jerome Bruner, Mark Bickhard and Gary's philospohical contacts, Don Campbell and Karl Popper. The rest is recent history.

I find in PCT a restoration of intention and "free-will" to human beings, 3an understanding fundamental if we are ever to get along with one another. It also gave me hope that the growing dissatisfaction I had with language teaching might end and that I could develop learning situations where teacher and students would be satisfied with one another and with the course. PCT also explains why the traditional educational power

relationships, if they persist, are not going to allow everyone equal opportunity to learn. And finally, I have come to the understanding that we will NEVER understand an individual learner solely by classifying and categorizing and correlating his behaviors. I consider myself interested in primary and non-primary language acquisition, neuropsychology, neurolinguistics, bilingualism, education, educational philosophy, and anything else I can understand well enough to argue about.
****

Date: Thu Apr 23, 1992 2:41 pm PST
Subject: intro2

[From J. Francisco Arocha, 920423; 0217]

My first introduction was not very informative regarding how I became interested in PCT. So here it goes some more. I did my undergraduate training in psychology in South America, where skinnerian behaviourism has been a big player since the 60's. So my initial training was very much influenced by it. When I graduated in 1980 (got the diploma of "Licenciado"), I did it with a lengthy monograph in which, with a couple of friends, attacked behaviourism, mainly on philosophical grounds. I had the conviction that many of the problems with it were because of their adherence to a philosophy of science that did not represent correctly what science was about. Unfortunately, if you did not like that psychology your choices were very limited, either opt out, as one of my friends did, or get into humanistic psychology or psychoanalysis or any other fantasy (as my other friend did). But I decided to get into some more practical issues and forget these theoretico-philosophical issues and thus gain at least some peace of mind.

I came to Canada in 82
and, after learning English, did my MA and PhD in Ed. Psych., where I became familar with information processing psychology and cognitive science and the works of Kintsch, H. Simon and others of the kind. However, the philosophical problems did not stop bothering me and now, with cognitive science I was faced with problems which were not the same behaviourism had but were no less "problematic".

Whereas I
saw behaviourism's problems as being the result of its commitment to positivism, with its too narrow standards of science, now I see cognitive-educational research committed to a post-positivist philosophy that encourages an "anything-goes" attitude, with no standards at all. The philosophers responsible for this attitude are well known to all: Kuhn, Feyerabend, and Lakatos. So all this time I have been looking for 2 things: a philosophy of science that truly represents the assumptions of science and a psychology that is based on, or is coherent with those assumptions. As for the former, I think I have found it in the works of Mario Bunge, probably the clearest and most important

philosopher since Bertrand Russell, but ignored by the
obscurantist philosophical intelligentsia. As for the
second, I hope I have found it in PCT. I'm still reading
the basics and I like what a read. Maybe later when I become
more knowledgable of PCT I will participate with comments
and intelligent questions.

****
J. Francisco Arocha
cybn@musica.mcgill.ca
****

Date: Thu Apr 23, 1992 3:32 pm PST
Subject: foundations

[From Rick Marken (920423 13:30)]

J Francisco Arocha (920422 1101) says:

>By perusing several journals I sense that cognitive science,
>has been gaining acceptance among psychologists.

I'd say it has been the dominant perspective in scientific psychology
(in the US, at least) since the late 1960's.

> PCT is not aligned to
>the idea that psychological processes are instantiations of
>abstractions.

I think this is the case -- but I'm not quite sure what you mean.
Not being much of a philosopher myself, I think I am eminently
qualified to try to give an answer. The main psychological process
in PCT is "control of perception". This process is postulated as
an explation of the phenomenon of control: the fact that organisms
produce consistent results in an inconsistent environment. PCT assumes
that this process is "instantiated" in the nervous system as excitatory
and inhibitory connections between neurons carrying signals in the
form of "neural currents" (spikes/sec). Neural currents are assumed to
be analog representations of environmental variables. Perceptual signals
tell the degree to which an environmental variable is represented at
the sensor(s); reference signals tell the degree to which some environmental
variable "should" be represented at the sensors; error signals represent
the difference between perceptual and reference signals -- and indirectly
specify how much effect the system should have on environmental variables
so that the perceptual signal tracks the reference signal. (Note that
an environmental variable need not be anything that we think of as "really"
out there. It could be a set of separate chemicals that are sensed in
such a way that a perceptual signal is generated that we experience as
"sweet", for example).

"Control of perception" is an abstraction when we describe the general
process of controlling perceptions. But I think that PCT would say
that for each perception that is controlled there is a real, concrete
neural circuit involved in its control. So I would say that PCT does not
look at psychological processes as instantiations of abstractions --

they are real, flesh and blood, control systems (made of neurons). But some of these control systems may be controlling what you might call "abstractions"; control systems that do "categorizing" could be said to be abstracting ("that is a good boy, that is not"). But I don't think I would want to call such a control system an instantiation of an abstraction -- but then, maybe I would.

> Moreover, I can see PCT as a unifying theory
>of cognitive psychology that is in close contact with other
>biological and social sciences without failling into the
>Platonist confusion. However, I would like to know the
>opinion of PCTers about this.

I think I know what you are getting at here -- and I think you are right about PCT solving some of these problems. Cognitive psychology seems to be trying to get away from the abstract, symbol manipulation approach to theorizing -- this can be seen in the revived interest in neural networks. But cognitive psychology has the same problem as all other "conventional" psychologies. It assumes that behavior is caused by cognition -- that behavior is output. So right off the bat they are just as far off the mark as the behaviorists, psycho-analysis, trait theorists, etc etc. They have no chance because they are looking at an illusion and taking it as fact; the illusion that behavior is caused output.

PCT shows that behavior is not caused output -- it is controlled input. This is not just a new unifying principle for cognitive psychology. It is a new unifying principle for psychology and the life sciences in general.

I hope you keep posting to the net, Francisco. I think a number of people on the net a philosophically inclined (and, unlike me, philosophically competent) and your questions are most interesting. Bien venido.

Regards Rick

Date: Thu Apr 23, 1992 3:50 pm PST
Subject: Re: foundations

[Martin Taylor 920423 17:15]
(I shouldn't be doing this ....)

(Rick Marken 920423 13:30)
>But cognitive psychology has the same problem
>as all other "conventional" psychologies. It assumes that behavior
>is caused by cognition -- that behavior is output. So right off the
>bat they are just as far off the mark as the behaviorists, psycho-
>analysis, trait theorists, etc etc. They have no chance because they
>are looking at an illusion and taking it as fact; the illusion that
>behavior is caused output.

You may be right about cognitive psychologISTS, but I think cognitive psychologY is neutral on the matter. As I understand it, it is about how we form perceptions of more or less complex abstractions. As such, it is about the perceptual functions in the control hierarchy as much as it is about the resulting actions. I think that once you get to the category level, you have to start worrying about the kinds of things cognitive psychologists worry about.

And I do think PCT is about the instantiation of abstractions. That's what the neural current that is the perceptual signal is. It represents the abstraction controlled by that ECS.

In their autobiographies, several people have said that S-R psychology made them uncomfortable long before they learned of PCT. I don't remember having been taught in that kind of behaviourist school. Nowadays, I think that we would call what I was taught "cognitive," which made sense to me and fits well into the hierarchic aspect of PCT. Information processing leads naturally
in that direction.

What I stuck on, philosophically, was the kind of statistics that leads people to quote "significance levels." That really IS philosophically (and practically) unsupportable. I wrote an essay on that in graduate school, and I had a paper rejected because I refused to cite significance levels even when the editor insisted. But I also reject that antistatisticalism that is so often repeated here. If you have information, use it, say I. If I read Bill right, he would cross a busy expressway aon foot with as much insouciance as a country lane, because it is only statistically more probable that he would be hit by a car on one than on the other. Not everybody gets hit by a car on a country lane, but some do, so looking at the class of road is of no value in deciding whether one should go there with a view to crossing.

Martin

Date: Thu Apr 23, 1992 5:16 pm PST
Subject: Grossberg model; Arm V2

[From Bill Powers 920423.1800)]

Joe Lubin (920421) --

Hello, Joe.

Sorry about not replying to the post you re-sent. I did get the Bullock and Grossberg paper, thanks. There are some useful bits in the paper. The whole approach to "velocity profiles" leaves me suspicious. I can easily move my finger from point A to point B in a helix, a broken line with straight segments, or along a straightish line with MINIMUM velocity at the MIDDLE of the trajectory, or in the shape of my handwritten name. What makes the profile bell-shaped in the experiments everyone is doing? (I also have Atkeson and Hollerbach on "Kinematics of unrestrained vertical arm movements," from Greg -- same problem). I think everyone is madly looking for "invariants" instead of trying to see how the system works. Even if you find invariants, what good do they do you? They don't tell you how the system is designed. And they don't explain behavior that deviates from the "invariant" form, which is pretty common (pick up a glass of water and put it down in a different place -- an arch-shaped trajectory). And what about very slow movements from start to finish, taking, say, five seconds? Surely they wouldn't show a bell-shaped velocity profile unless the subject knew that this profile is expected.

The bell-shaped velocity profile looks like the behavior of a damped second-order differential equation. If so, there are a million ways to get that form, and it will be "invariant" over a lot of different conditions, just naturally. Especially if you normalize to duration and peak velocity (which removes all the interesting information). I'm more interested in how the waveform varies under different conditions.

One thing in B&G's favor is that they are trying to get the various movements to emerge from the model instead of designing them in as fixed programs. That's progress over the Kelso, Bizzi, etc. approach.

Basically I think the B&G model is still too ad-hoc, and tries to explain behavior that are suspiciously stereotyped. There are some glimmers of control theory in the various kinds of feedback considered, but outflow feedback doesn't get you much if there are disturbances in the environment (like switching gravity on and off). And tailoring the onset delays of synergist contractions will let you produce just about anything at all. Finally, I think that B&G are trying to explain too much. They'd be better off just to focus on getting a basic behavioral model to run. But if they'd rather leave that to us, I'm game.

By the way, in their Fig. 6, B&G show a monkey's pointing trajectories when the target switches positions 50, 100, and 300 milliseconds after the start of the movement. Unless I misunderstand the time scale completely, something's wrong. The monkey is anticipating the jump of the target by starting the move in a direction toward the displaced position instead of heading for the first target and being surprised. Or is it that for delays of 300 milliseconds, the first motion goes to completion, but for the shorter delays it's corrected in progress (very near the origin)? Without some kind of time scale this diagram doesn't make sense.
In my modeling I try to get things to happen "naturally" -- with a minimum of outside intelligence helping the model behave right. I try to avoid models where the timing has to be just right to get the right result, or where the exact amount of amplification is critical, or where there's too much logical busy-work going on in the background, switching this circuit in and that one out just when necessary. If you give yourself too much leeway in such matters, you don't really get a model; it's more like an animation, or like picking up the robot toy and pointing it in the right direction when it's about to get in trouble. Of course some ad-hoc patchwork is unavoidable; I'm just against doing it too much.

ARM progress:

Conflating the extensor and flexor systems can still give control of compliance if you use a table-lookup representation of the muscle nonlinearities (the approach Greg and I took during preliminary tests). You get the effect of two nonlinear muscles without having to model them specifically. I have nothing against modeling them individually, but I'm trying to get a finite package finished, and want the result to run reasonably fast on my machine. With Version 3 you can get as fancy as you like.

In Version 7, of course, you're going to have to recognize that there are no pure agonist-antagonist pairs. You'll have to model, for the arm, something like 7 sets of muscles no two of which are orthogonal or opposed.

I've discovered why the trajectories under visual control were behaving so strangely (or at least one major contributing factor). I have the head turning and nodding to keep the target visually centered. When the target moves straight down from eye level to a lower level, the head nods forward, moving the eyes forward and down by a good distance. This tells the control system that the fingertip is getting too close, so the control systems push it away. Result: a big arc bowing outward! On the upstroke, the head lifts and moves backward, and you get an arc bowing inward.

The immediate remedy is just to freeze the head so it doesn't move. This clears up all those problems, at the expense of losing a nice detail of the model. A longer-term solution will require some extensive visual computations to provide x,y, and z perceptual signals that are unaffected by head movement. This is just part of a larger project, which is to create an internal representation that is stationary with respect to some bodily frame of reference, so the world looks stationary while the head and body move in it. I'm not ready to tackle that, so I'll just use a stationary head and a very simple geometry corrector for reaching only, in Version 2. That will allow reproducing several kinds of trajectories shown in the Atkeson and Hollerbach paper. Actually, adjusting just one integration factor changes from one subject's trajectory (with loose loops) into another's (with upstrokes almost identical to downstrokes). I think this is good enough to go with.

To get true trajectory control, we're going to have to put a specific transition-control level into this model. I've learned a lot about what will be needed. It's not just a derivative-taking level; it controls the path from one point to another. In a three-dimensional control task, you can have three functions, each describing a wave-form in one dimension. A parameter t then runs from 0 to max, causing f1(t), f2(t), and f3(t) to go through their respective waveforms, creating the desired path in 3-space. The linear speed of traversing the path is controlled by the speed with which t varies. You can even go partway through a trajectory and then slow down, stop, and reverse along the same path to the starting point, just by manipulating the one-dimensional parameter t. So this level isn't really concerned with time functions. This, too, is for Version 3.
-- May other interesting posts going by, which I appreciate even if I don't respond. I have to get this !#@%* arm thing done before much longer or I'll run out of steam.

Best to all Bill P.

Date: Thu Apr 23, 1992 5:46 pm PST
From: Hank Folson (920423)

At a time when I was observing that all the different business management theories I was familiar with probably were subsets of some master theory that covered all business situations, Dag Forssell introduced me to PCT. I now feel that the master theory i s PCT, and it covers a lot more important aspects of life than business! I am reading and rereading books related to PCT. I am on the CSGnet to learn more about the theory, with practical everyday applications my main interest.

I have a Bachelors in mechanical engineering from U. of Manitoba; a Masters in welding engineering from Cranfield Inst. of Technology, England; and a Masters in product design from Stanford (1966). I own a small business making investment cast steel compo nents for expensive custom bicycle frames. I compete against the technology of Japan and the labor rates of Taiwan, so I shouldnUt spend as much time as I do on PCT!

My vote is NO on Usenet. What are the people on Usenet controlling for? Doesnt PCT say they will be interested in learning about PCT only if they are experiencing uncomfortable error signals using their existing paradigms? How many of the Usenet subscribe rs are in a Reorganization mode, looking for something like PCT? Will these receptive people find CSGnet? Will they grasp its value immediately? And what distractions will come from those not ready for PCT?

Isnt CSGnet now set up to allow people already seriously interested in PCT to learn more? The format is pretty much undirected and random. If the goal were to attract new converts, the format would have to be very structured. Small precisely measured dose s of new information would gradually build the PCT case, and not create too great an error signal in the newcomer and turn him off.

PCT is a hard sell. If I may try my hand at a diagram demonstrating how people behave:

A Stimulus-Response diagram:

```
          ME  <-----------------------<-------------
         |                                         |
Responding to  my environment, which includes people |
          |                                         |
These people are responding to their environment    |
          |                                         |
Which includes-------->------------------------->---
```

If you buy into S-R, this looks workable. But look at the same diagram in PCT terms:

```
ME (An autonomous living control system)  <---------
         |                                         |
Controlling my perceptions of                       |
my environment, which includes people               |
          |                                         |
```

```
These people are autonomous living control systems  |
controlling their perceptions of their environment  |
          |                                         |
Which includes---->------------------------->-------
```

The diagram is now unworkable because autonomous living control systems cannot be controlled externally. We try, though, because it is natural for a living control system to want to perceive the world as one where we can control others. Stress is the resu lt. Some satisfaction comes in limited situations where threats and violence control others, but the satisfaction is not mutual. And of course, as a living control system, I donUt like it at all that others are trying to control me and some may have the p ower to control me!

The point of this is that we cannot control others, although that is what we living control systems naturally want to do. The last thing we living control systems want to hear is that we canUt control other people. I think it follows that people (includin g those on Usenet) would rather not accept PCT and its implications. The initial error signal is too great to accept, although life could be less stressful and organizations and societies might function better if one does understand and apply PCT.

My vote is for the CSGnet to concentrate more on applications of PCT, as newcomers might be more open to PCT if they could see its long term benefits, rather than its immediate disturbing features.

Hank Folson, Henry James Bicycles, Inc. 704 Elvira Avenue, Redondo Beach, CA 90277 310-540-1552 (Day & Evening) MCI MAIL: 509-6370

Date: Thu Apr 23, 1992 5:59 pm PST
Subject: Say what, Alan?

From Greg & Pat Williams (920423)

>Alan E. Scrivner (042292:0730)

>Trying to map "rightness" or "wrongness" onto the real number line for the
>sake of having continuity and therefore convergent control wasn't setting
>too well.

>Zadeh's extension principle:
>...
>Recall that a fuzzy set is an ordered pair consisting of an element and the
>value of its membership function. Also, sup min{...} is the fuzzy metric
>and with it we can satisfy all of the requirements for a metric space and

>do the kind of calculus required for control systems to converge, etc.

OK, Alan, what DOES a map for "rightness" or "wrongness" look like? Can you provide an example of what you're talking about so we non-mathematicians can appreciate it, too? ("Redness" would do, if "rightness" is too hard for a simple example. Or how about "squareness"?)

Thanks,

Greg & Pat

Date: Thu Apr 23, 1992 8:11 pm PST
Subject: foundations

[From Rick Marken (920423 19:00)]

Greg -- What a beautiful "Closed Loop".

Martin Taylor (920423 17:15) says:

>(I shouldn't be doing this ....)

I know. I think I'm going blind from it too. I keep telling myself that I should only do it until I need glasses.

>And I do think PCT is about the instantiation of abstractions. That's what
>the neural current that is the perceptual signal is. It represents the
>abstraction controlled by that ECS.

See. I knew there were serious philosophers out there. I have no problem with Martin's way of putting this.

> But I also reject that antistatisticalism that
>is so often repeated here. If you have information, use it, say I.

My only objection to statistics is the tendency to treat data averaged over subjects as though it said anything about individuals. Many conventional psychologists have complained about this too -- notably B.F. Skinner -- and yet this kind of research persists. I would feel better about it if it were called population analysis instead of psychology.

My problem with conventional methodology goes a lot deeper than statistics. Conventional psychology would have problems even if statistics were banned (as they basically are in the "experimental analysis of behavior" journals, I think). The basic methodology in psychology is to manipulate an independent variable and measure its effect on a dependent variable under controlled conditions. This IS experimental psychology -- whether you use statistics or not. Thus, operant conditioners manipulate schedules or stimuli or whatever to see how they affect dependent variables like pecking rates or amplitudes or whatnot. Cognitive psychologists manipulate interstimulus intervals, stimulus information, etc etc and determine their effect on reaction time, ratings, etc etc. If the independent variable is not manipulated under controlled conditions then the research is called correlational (not in the statistical sense). But the basic goal is to see how one variable (usually one that can be seen as a stimulus or input) relates to another -- the response or output measure.

PCT shows that this approach to research tells you very little (except by accident) about the nature of the behaving organism. Not because the results are analyzed with statistics; but because you are dealing with a closed loop system that is in a negative feedback situation with respect to its environment; that is, you are dealing with a system that controls its own sensory experience. So the independent-dependent variable approach to research won't work -- because o = -kd (the e=mc2 of control theory); if oranisms are in the negative feedback relationship with their environment then apparent relationships between independent variables (d -- the only variable that can be manipulated independently of what the organism does) and responses (some measured consequence of variations in the organisms output system, ie. muscles) depends entirely on the physics of the environment, k, and has nothing to do with properties of the organism itself. This is heavy stuff -- but it has nothing to do with psychologists' penchant for statistics.

The appropriate way to study the behavior of organisms is to test for controlled variables. Actually, one of the approaches to detemining a controlled variables -- measuring the stability factor -- is a statistical approach. The stability factor is a sample statistic with a sampling distribution. We decide that a variable is controlled if the value of the stability factor is highly unlikely given the null hypothesis (of no control). There, see, even control theorists can use statistics. This is not always the best approach to determining a controlled variable; but it can be helpful if you can't be sure that the variable will be held at a fixed reference value, for instance.

Final note: I am neither anti statistics nor anti the independent-dependent variable approach to research. PCT is not on a crusade. It's just that, if people are control systems, then the IV-DV approach is basically worthless -- not because we want it to be, but because it is.

Best regards   Rick

Date: Fri Apr 24, 1992 9:16 am PST
Subject: control of behavior

[From Rick Marken (920424 08:30)]

Hank Folson (920423) says:

>PCT is a hard sell.

This is because one main point of PCT is:

>   that we cannot control others,

>although that is what we living control systems
>naturally want to do. The last thing we living
>control systems want to hear is that we can't control
>other people. I think it follows that people
>(including those on Usenet) would rather not accept
>PCT and its implications. The initial error signal is
>too great to accept, although life could be less
>stressful and organizations and societies might
>function better if one does understand and apply PCT.

I could not have said this better myself. I have tried to make this point many times, in many ways, with what I perceive as only modest success. So I am thrilled to see it made so clearly and concisely by someone else. Excellent post, Hank.

There is no escaping the fact that when the big guy created life he placed us squarely in the middle of a frustrating paradox -- we live by controlling but we cannot control what is living. As you said, because we are control systems, we cannot be controlled; and because we are control systems we cannot help trying to control.

PCT IS a tough sell because people want to understand things so that they can control better. It is difficult to convince people that things will go better (with other control systems) if they don't control (or, at least, control with a bit less skill). Still, while PCT is a hard sell, I am now convinced that it is very important to, if not sell it, at least make it available to those who might profit most from understanding it; ie., everybody.Some people will resist these ideas -- and even become rude and unpleasant in their efforts to remove the disturbance. But I think it is our responsibility to at least put these ideas out in front of people, in as clear and convincing a way as possible, without compromising in order to "sell" it. Just give it a chance and understand that nasty replies or reviews are not personal attacks but the understandable efforts of other control systems to protect principles and system concepts that they consider important. I used to feel like you do (and I still do to some extent; it's not easy listening to what are sometimes just plain mean spirited rebukes). I like going to CSG meetings because it's a lot more comfortable dealing with people who already "get it" and want to hash out the details. But I think the potential benefit of getting people to understand their own natures is so great that it's worth, I think, putting up with some possibly unpleasant resistence.

More than ever in my lifetime it seems that the world is bound and determined to solve it's problems by controlling people. It seems even more insidious now because this strategy is less obvious than it once was -- when we had nice clear cut dictators like Hitler and Stalin using this strategy to the chagrin of most civilized people. Now our enlightened society thinks its problems come from the fact that we have let people get out of control. So the proposed solutions are more laws, more police, more jails, more regulations, more death penelty, stricter moral codes -- control, control, control. The idea that it might be this orientation toward control that is causing the problem does not even seem to occur to most people. I hear very little serious talk about programs that would "empower people"; help GIVE control -- education, work training, child care, cooperative work programs, community centers, insured medical care etc etc.

The only objections I hear to solutions that involve controlling others come when controls are suggested for limiting competition; the goal seems to be to have control over other people unless this control limits conflict. This is a "kinder, gentler" society. Sheese.

Yes, I think it's worth it to try to help people understand their own nature as control systems. If people don't want to understand it then, fine, we are no worse off than before. But I think that the potential benefits of understanding PCT outweigh the potential unpleasantness associated with trying to teach it.

Regards Rick

Date: Fri Apr 24, 1992 10:54 am PST
Subject: Last Chance to Vote

[from Gary Cziko 920424]

Today is the last day I will take votes on the two-way Usenet (NetNews) connection proposal. If you haven't voted already, send your vote to G-CZIKO@UIUC.EDU and put YES to USENET or NO to USENET in the subject header. No message is required.

I will accept votes "postmarked" no later than midnight today, your local time.

I will post the results this weekend.--Gary

Date: Fri Apr 24, 1992 11:29 am PST
Subject: Genie

[From: Bruce Nevin (Fri 920424 08:32:21)]

Someone (Rick, I think?) mentioned the _New Yorker_ articles about Genie, the girl who grew to 12 years of age in social isolation.

A complex story. Setting aside the poignancy of this human catastrophe --and by catastrophe I mean the social dementia of the world of science and academic politics into which she was plunged and the exploitation of her case as much as the family dementia into which she was born--I'll focus on linguistic aspects, insofar as I can from a PCT perspective. The NYer article lays out the history, you can read it there.

The conclusion drawn from Curtiss's investigations is that normal left-hemispheric systems for what is termed grammar or syntax never developed. Genie learned vocabulary, and she learned to string operator words in appropriate sequence with their argument words in relatively short sequences. She excelled at right-hemispheric functions, could reproduce complex gestalts in detail. Her talking was mostly about the detail of objects in her physical environment; telling stories with sequences of events and social relations was beyond her verbal capacity. "It was on the gestalt tests that Genie scored higher than anyone in the literature. But her portrayal of her complex comprehension was better achieved through visual than verbal means." Tests showed brain activity in the right hemisphere not only for nonverbal things, but also for her use of language, both understanding and speaking. Many anecdotes attest that her nonverbal communication was stunningly effective.

The linguistic investigation was framed in binary terms. Lenneberg (and Chomsky) held that language acquisition must occur during the period when lateral dominance is developing, and cannot occur afterward. If Genie, now at puberty (concurrently with being toilet trained!) could acquire language, then this was clearly not the case. Chomsky and Lenneberg had both been invited to participate and had declined. The NYer writer (Russ Rymer) suggests that self interest provides sufficient motivation for this reluctance. She could at best provide only muted support for the theory (failure to learn could be attributed to her profound emotional disburbance), and if she actually did learn to control language normally it would be a ringing refutation. But that cannot be, since these are men of science for whom learning the truth must be more important than having one's theory upheld. Probably they had other commitments.

In the usual terms, Genie did not learn the movement operations for forming wh- questions.

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                 The cracker is on the shelf.
           <--------------------+
        Where is the cracker    0 ?
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In terms of Harrisian operator grammar and a PCT perspective, there is no movement here, only alternative linearization of word-perceptions that have no linear sequence at all at higher levels of the hierarchy. I have not read Curtiss's book (based on her UCLA dissertation with Vicki Fromkin), but there is no mention in the article whether she controlled such alternative linearizations

The cracker is on the shelf.
On the shelf is the cracker.
Where is the cracker?

In these terms, the wh- words in questions are reduced from a disjunction under "I ask whether," something like:

I ask whether <X> is on the shelf or <Y> is on the shelf.
I ask whether <X> or <Y> is on the shelf.
I ask what is on the shelf.
I ask: what is on the shelf?
What is on the shelf?

(Substituting words the speaker thinks likely in the situation, or in complete ignorance substituting "a thing" for X and "a(nother) thing" for Y.)

There is not always an unusual linear ordering (or "movement") required for wh- questions:

The cracker is on the shelf
What is on the shelf?

A big lack in Genie's ability, then, appears to have been certain conventional reductions. This would account for her restriction to relatively simple sentences. She lacked the reductions for combining simple sentences into complex ones.

But her problems go deeper than that. As the article says, she never progressed from the toddler stage of "no have toy" to "I not have toy," then "I do not have a toy," and finally "I don't have a toy." English requires that discourse be presented in terms of temporal sequence. This requirement is codified in the requirement for tense affixes, reduced from operators like "before" and "after" that express temporal relation of parts of a discourse, or between part of a discourse and some other part that might have been spoken (but was not) about mutually understood context or background. Without this conventional requirement there could be no motivation for the requirement in English that operators with stative meanings be accompanied by "do" as a carrier for tense. Without "do," there could be no reduction to "don't."

Then there is the problem with pronouns, and her incapacity to tell stories involving who did what to whom. Perhaps here we run against her special perceptual universe. She is described as never getting clear about the boundary of self and other. She lacked most pronouns.

> "I" was her favorite, and "you" and "me" were interchangeable. . . . "Mama love you," Genie would say, pointing to herself.

Her capacity for nonverbal communication, however, was absolutely stunning. Curtiss:

> Without a word, she can make her desires, needs, or feelings known, even to strangers.

She had a penchant for anything made of plastic. Her only plaything was a plastic raincoat that sometimes was hung in her bare room, and that sometimes she was allowed to handle, strapped in as she was on her potty seat. In the first part of the article (not before me now) is the anecdote in which Curtiss was walking on the sidewalk with her. Traffic was stopped for the light. Just as the light was changing, Curtiss heard the unmistakable sound of a purse being emptied on a car seat, and this woman opened her car door, ran over, and handed Genie her plastic purse. The light was changing, and she ran back to her car and sped off. Never a word was exchanged.

Genie's progress was hindered by her being taken from a foster home where she was happy and doing well to the home of the principal investigator in the NIMH project, which was evidently much less warm and intuitively accepting, more manipulative. (My judgments, from Rymer's account.) Subsequently, when she was moved to other foster homes and to a home for retarded adults, she retreated into dementia.

Things that I wonder about:

What was her left hemisphere doing? Just because it wasn't doing the non-semantic aspects of language doesn't mean it wasn't doing anything. Perhaps the problem is not that a critical period was passed, and that the neural matter lay fallow (Curtiss finds parallels to kids with hemispherectomies), but rather that the neural matter of the left hemisphere was already pre-empted for other purposes. Perhaps tests indicated no activity, no higher cortical functions, under a wide range of circumstances; I doubt it.

Perhaps her perceptual universe was richer than could be conveyed in language shared by people living in a more conventional perceptual universe.

Weak or poorly defined ego boundaries between self and other and boundaries between imagination and reality, which were very problematic for Genie, it has seemed to me are also problematic also for people with socalled psychic or clairvoyant abilities. I have long suspected that there is a correlation between proclivity for these perceptions that do not fit in our conventional perceptual universe and the social status of mental illness. I relate this to Genie's striking gifts of nonverbal communication. These gifts were largely lost on the linear-thinking, highly intellectual, highly verbal, literal-minded people with whom she was compelled to live. Her distress at being unable to communicate in their company (but communicating very well empathically, without a word, with the neighborhood butcher or the cook in the hospital or the woman who jumped out of her car to give her her pocketbook) seems to me like an enormously aggravated form of distress felt by multitudes of people with nonverbal (stereotypically right-hemisphere) preferences for communication modalities.

The Chomsky/Lenneberg binary question was not answered yes or no, but the terms of the question were somewhat reframed. Orthodox opinion was previously that innate mechanisms of Universal Grammar provide a set of parameters, and on exposure to a language the mechanisms in the child's brain choose for each parameter one value out of the range of possible values. Orthodox opinion now is that experience of language use is necessary to trigger the very development of right-brain neural structures for the particular aspects of language housed in Wernicke's area, etc. But though the innate mechanisms are inchoate, latent, they are nonetheless (it is claimed) a physically present component of the child's species-specific genetically inherited biological endowment.

The experience with Genie does not in itself show this, of course. Claims for innate language mechanisms rest, as always, on the standard argument from paucity of data: (a) the language use heard by the child is too fragmentary and limited and frought with error for the child to learn (b) something so very very complicated as language. Studies of the social context of language learning (e.g. Bruner's Language Acquisition Support System or LASS to accompany Chomsky's Language Acquisition Device or LAD) vitiate the (a) half of this argument, together with the observation that children are working very hard on this with very little else to do, and that it is intimately connected with development of higher levels of the control hierarchy. As to the (b) half of the argument, a comparison of Generative Grammar with the operator grammar of Harris suggests that much of the hairy complexity that seems hopelessly daunting for a child not possessing innate mechanisms for language is a property of the theory and not of language.

A number of questions for HPCT arise, which Genie's story might help illumine. There may be a requirement that certain elements of the control hierarchy be developed before or during lateralization. I have seen no discussion of hemispheric specialization on this list, and the only discussion of specialized areas of the brain such as Broca's or Wernicke's has been Bill's suggestion that they are not so specialized as they seem. Since you have agreed, Bill, that there must be control systems specialized for control of elements and relations in language, perhaps the grouping of these in particular areas of the brain now seems more plausible to you. Language deficits similar to Genie's are found in children lacking the left hemisphere of the brain, and in deaf children who learned sign after lateralization or after puberty (as opposed to those who learned younger).

I hope others read the articles and find these and similar issues worth pursuing.

Bruce Nevin
bn@bbn.com

Date: Fri Apr 24, 1992 1:56 pm PST
Subject: Misc subjects

[From Bill Powers (920424.0900)]

OK, you-all have given me another excuse to avoid working on Mr. Arm.

Alan Scrivner(920422) --

>Recall that a fuzzy set is an ordered pair consisting of an element and
>the value of its membership function. Also, sup min{...} is the fuzzy
>metric ...

Golly, how could that have slipped my mind? Alan, you've said just enough to arouse a lot of interest. How about doing a little work on making that footpath run both ways (I second Greg Williams' motion)? Is there any way the import of the mathematics can be explained to the likes of me?

Martin Taylor (920423.1715) --

>If I read Bill right, he would cross a busy expressway aon foot with as
>much insouciance as a country lane, because it is only statistically >more
probable that he would be hit by a car on one than on the other.

If I read some statisticians right, they would cross the country road without looking left and right because the probability of being hit by a car on country roads is negligible.

I wouldn't cross either kind of road without looking, with or without insouciance. If the traffic on the expressway has a substantial gap in it, I'll dash across through the gap, judging that none of the approaching cars is close enough to get me, even on purpose. If there's a lone vehicle approaching too closely on the country road, I'll wait for it to pass. That kind of data I trust.

Perhaps it's just a matter of the signal-to-noise ratio one demands before taking any observation seriously. Or perhaps it's a matter of preferring to use available data about the actual circumstances rather than relying on generalizations across many circumstances.

Different subject:

Martin, your paper "Principles for integrating voice I/O in a complex interface" is a beautiful job, both of introducing PCT to newcomers and of using its principles to advance the status of Layered Protocols as an explanatory and analytical approach to communication. It's the clearest piece of writing and thinking by you that I've seen yet. One gets the feeling that behind all of your statements about the problems of communication there is a solid base of principles that make sense. But there's more to your analysis than rhetoric. This is the sort of stuff that others can understand and apply insightfully. One gets the feeling that this is really how communication works. How about posting a citation (present or planned) so others can find and read it? Highly recommended.

J. Francisco Arocha (920423.0217) --
You not only learned English, you mastered it. Both of your posts on philosophy of science were clearly put and of great interest to me. It's good to have an "ex-Skinnerian" in this group; you will understand the problems of communication (and the possibility, if it exists) better than any of us who never went down that road very far. I look forward to reading your thoughts on just where and how the Skinnerian movement went off the tracks. As Greg Williams notes, Skinner did get as far as doing PC(T)-compatible experiments, giving the animals control over their own inputs (reinforcers). But there was obviously some deep-seated belief system that kept him from seeing autonomy in the behaving system, a belief system that I think is common to most of the life sciences. What do you think on that subject?

Your path into PCT is a very typical one, perhaps the only one that brings people into the CSG. Most people who now understand control theory began by seeing what was wrong in their own disciplines, without knowing how to fix it. This meant that they had already analyzed some very basic aspects of human behavior and had realized that the theories in their own fields didn't conform to what they could see, in one important way or another. The people who end up calling themselves PCT psychologists (or whatever) are very much self-selected, which seems to me quite appropriate.

Dick Robertson (920423) --

I hope you'll keep nagging Frans about getting on the net -- maybe after he sees some of the conversations he'll realize how much support and interest he can find here.

Your history brought back memories. Perhaps it would be appropriate to mention on the net that you are probably the first psychologist to have spontaneously self-selected for PCT. That was 1957 -- I still have a copy of the Counselling Center Discussion Paper (purple ink) from the seminar where Clark, McFarland, and I made our first public presentation of the theory (at Rogers' invitation, through McFarland I think). That was 35 years ago, gulp. I even have the original Polaroids (shown at the seminar) of oscilloscope screens showing control-system properties as simulated on a Philbrick analog computer. Remember "leading, lagging, and proportional personalities?" I think that came out of those simulations.

I really look forward to seeing you again at the meeting.

Hank Folson (920423) --

You sound a lot like me. I have never made a living doing control theory, except as an physicist/engineer and with inorganic control systems. Looks like you're doomed to the same split-level existence. Unfortunately, once you understand PCT you can't un-understand it again.

I had reservations about Usenet, too. But if we just go on as we are, all that will happen, probably, is that a few people will get the bug and join us while the rest go back to plodding down their various blind alleys. I agree about proselytizing; I think that's best done person to person and by request. The introductory classes in PCT are either self-taught (people like you) or are taught by CSGers and affiliates who are already in teaching positions.

If the Usenet thing turns out to be a mess, Gary Cziko will take care of it. And I'm sure that if persistent disturbances happen, the autonomous control systems on the net will deal with them briskly.

Happy wheels.

Rick Marken's post reminded me of something someone else said (Bill Cunningham? Sorry ..) about statistical perceptions. I think we have to distinguish between NOISY perceptions and PARTIAL OR INCOMPLETE perceptions. They're not the same thing. When you see that patch of tawniness through the grass, the problem isn't noise in the perceptions. The patch is perfectly clear and so is the grass. The noise level is imperceptible, as is true of essentially all our conscious perceptions. The problem is HOW TO CLASSIFY that tawny patch: is it just some bare ground, or is it part of something more mobile? And of course even when you decide which it is, you still have to consider higher-level circumstances: is this a picture of the African veldt, or am I standing there live? A lot of what seems like statistical uncertainty is really just being faced with a conflict: if I use the "this is a lion" perception, there are logical consequences that follow, while if I use the "this is a piece of bare ground" perception, there are different and probably contradictory consequences I can perceive. Aren't the circumstances where we have to make decisions always conflict situations? If there's no conflict, we just do the first thing that will serve our higher-level purposes -- it doesn't really matter which thing.

Joel Judd (920423) --

Nice to know you even better. I wonder if a lot of people on this net are the kind who have done many different things in their lives.

Martin Taylor (920322(?) received on 24th) --

>For me, CSG-L is a working group, in which serious problems are >discussed and the state of the art advanced.

I see it that way too, but in a broader context. The state of the art varies a lot from one discipline to another. In some areas, even basic control theory represents a giant step that is difficult to take; in others it's almost taken for granted. From my standpoint, no one area of interest is so important that others ought to be neglected. If this means that we're talking about control theory at many levels at once, so be it.

I don't think we can actually have a "working group" on this net in the sense you mean. Our interests overlap at the bottom, but what we've built on them is divergent. The best of all possible worlds is the one in which each person on the net has a local working group of people who are headed in the same direction and talk the same language. Right now this is difficult for most PCTers because so few people understand or even care about it. Lots of CSG people are isolated, so this net and the annual meetings provide the only real communication on the subject -- the rest of the time they labor alone.

The net gives us an opportunity to enjoy and learn from each other without having to go through politics and persuasion. But the really technical work we do in our own fields, while we may talk about it on the net, can't be done on the net. I see what we're doing as constructing a broad base of fundamental principles with illumination from many fields to keep it from leaving out important considerations. I hope that this base comes to be accepted by every major discipline in the life sciences, whatever they may choose to build on it. This is the only way a true science of life can come into being. I think that goal is more important than any particular thing I'm working on; in fact, the only reason I work on things like cockroaches and arm models and crowd models is to show how this new approach can be used in particular fields. I'm not trying to be a renaissance man with expertise in everything. I'm not equipped for that. I hope people will understand that, and realize that if they wait for me to make some great advance in their own fields, to prove that control theory works there, too, they're going to wait a long time. I'm just trying to point out directions -- there really isn't a lot more than that that I have the knowledge to do.

The real advances will be made by people with a solid understanding of basic HPCT principles, and by the students and colleagues they introduce to these principles. HPCT itself will be advanced by these people, provided they stay in contact with others in all disciplines and remain aware that it's a single science we're developing here, not any particular specialty. There is no present discipline that has discovered a route to understanding human nature. The present disciplines are too narrow and too isolated from each other. The ultimate basic model of organisms on which a science of life can build will not be all about statistical perception, or all about language, or all about social interactions, or all about cognition, or all about mathematical theorems, or all about motor behavior, or all about human development, or all about neural networks. These are all fields that have developed without any common thread to tie them together, without many commonly- understood principles, without even a common mode of discourse.

The control systems group comes as near as any organization to being a science-of-life organization. In it are people of all levels of understanding of control theory, from more than a dozen different disciplines, with the complete gamut of interests in human beings and animals. What ties the group together is the same thing that makes CSG-L into a coherent forum despite the incredible diversity of topics and interests. It's the realization that the topics and interests aren't as diverse as they seem, once the common language and viewpoint of control theory is assimilated. For a change, all these people begin to get a sense that they're working together within the same conception of human and animal nature, that the boundaries between their fields of interest and expertise are illusory.

I want to eliminate those boundaries altogether. This can't be done by excluding people or ignoring them while a few people talk about one narrow application of control theory -- especially when it's an application that tends to be focused on things that people have done in the past and want to develop further, control theory or no. I want to emphasize what's new, what's next, what we have or can have in common with the foundation of control theory beneath us.

When we get off the net or finish the annual meeting, each person goes away with little more in the way of knowledge useful in a particular specialty, but with a very much enhanced concept of a shared science. I like to think that the boundaries fade a little more with every encounter. When we no longer think of ourselves as biologists or psychologists or sociologists and so on, but see ourselves as simply approaching one central subject through the examination of particular details of it, the boundaries will be gone and we will have a science of life as general and as clearly unified as physics. Then, at last, after three centuries or more, we will have the beginnings of a real science of living systems.

In the light of all that, I'm content to do my own work offstage and use our public meetings to advance the more important cause of a unified science of life.

Best to all Bill P.

Date: Fri Apr 24, 1992 2:34 pm PST
Subject: Bunge; Popper

[from Gary Cziko 920424.1500]

J. Francisco Arocha, 920423; 0217 said:

>So all this time I have
>been looking for 2 things: a philosophy of science that
>truly represents the assumptions of science and a psychology
>that is based on, or is coherent with those assumptions. As
>for the former, I think I have found it in the works of
>Mario Bunge, probably the clearest and most important
>philosopher since Bertrand Russell, but ignored by the
>obscurantist philosophical intelligentsia.

I have run into a Bunge once or twice myself, but don't remember much other than his antipathy (which I generally share) for Chomsky's view of mind and language.

Could you give some key Bunge references that you think are most important? I am quite interested in the philosophy of science, with preferences for the evolutionary perspective of Popper and Don Campbell. And any comments on how you see Bunge's philosophy fitting in with PCT would also be appreciated.--Gary

P.S. Speaking of the philosophy of science, I seem to remember Greg Williams saying some "bad" things about Popper in response to Dag Forssell's presentation at Durango last summer. I never did get the chance to follow up on this. Perhaps if Greg has the time he might summarize his objections (I just hope he doesn't bring up the old naive falsificationist stuff).

Gary A. Cziko

Date: Fri Apr 24, 1992 3:21 pm PST
Subject: Statistics

[from Gary Cziko 920424]

Rick Marken (920423 19:00) said:

>My only objection to statistics is the tendency to treat data
>averaged over subjects as though it said anything about
>individuals. Many conventional psychologists have complained
>about this too -- notably B.F. Skinner -- and yet this kind of
>research persists. I would feel better about it if it were called
>population analysis instead of psychology.

I understand and agree with your objection, in spite of the fact that this realization has caused me no small amount of intellectual trauma.

But I wonder if the "input-output" model and group statistics doesn't make more sense for the study of reorganization (rather than for well-functioning control systems).

For example, let's assume I want to look at different ways of teaching swimming. I find a group of people who want to learn how to swim (but can't) and divide them into two groups. One group gets a couple of dry-land sessions first to explain the physics of swimming, stroke mechanics, etc. while the other groups heads straight for the water. Wouldn't it make sense here to use statistics (dependent variable being some measure of swimming ability of x number of lessons) to find out which of the two environments tends to lead to better reorganization for swimming? If 85% of the people in one class learn to stay alive in deep water but only 45% in the other, isn't this useful information to have? I don't see why doing the statistics to study reorganization necessarily implies an input-output view of behavior.--Gary

Date: Fri Apr 24, 1992 3:42 pm PST
Subject: lateralization and (oooh) religion

[from Joel Judd]

Bruce (920423)

Thanks for the Genie summary. For some reason when I read the earlier post I thought the NY series was a NEW case--I didn't realize it was Genie. I'm glad you found out for me. I'm also glad you tend to discount her case as definitive evidence for a linguistic critical period. I used to find the notion attractive but have since backed down--there's too much redundancy and too long a time period in language development to expect "language" use/non-use to correlate to a specific maturational change in brain state, which is what a critical period is.

Interesting you brought up lateralization, since I also used to be an avid follower of all the SLA developments in this area (e.g. Michel Paradis and aphasic studies). I don't know, for the purposes of education it just doesn't seem to make much difference where 'dog' is in my head, maybe that's why I haven't missed laterality discussions lately. They have too often led to outrageous classroom practices in the name of developing this lobe or that area, after of course one is diagnosed as being deficient in "right hemiphere artistic abilities."

Nevertheless, there is some interesting reading on this, and maybe rereading some of the older stuff will look different in light of PCT. My nomination for a reading is:

Geschwind, N. & Galaburda, A. (1985). Cerebral lateralization: Biological mechanisms, associations, and pathology (3 parts). _Archives of Neurology_, _42_, 428-459; 521-552; 634-654.

It's an extremely fascinating compendium that traces lateralization back to the developing fetus.

****

Rick's reply to Hank (920424) forces me to voice my agreement, and put in a plug for a careful reading of at least New Testament christianity. In spite of several hundred years of abuse on the part of different denominations, take a PCT look at what we have of Christ's teachings. I'll give you my sweeping summary:

Help others all you can, but be responsible for yourself.

In other words, accept responsibility for what you CAN (in large measure) control [yourself], and don't get bent out of shape for what you CAN'T [others, and environmental niceities]. Breathtaking, isn't it? Be willing to share and help and cajole and serve others, but be concerned about what YOU say and what YOU do and what YOU think.

Whew. Back to looking for work.

Date: Fri Apr 24, 1992 4:01 pm PST
Subject: Teaching swimming

[From Bill Powers (920424.1600)] Gary Cziko (920423.1900) --

>Wouldn't it make sense here to use statistics (dependent variable being
>some measure of swimming ability of x number of lessons) to find out
>which of the two environments tends to lead to better reorganization
>for swimming?

Sure it would, under some conditions.

1. You teach swimming on a regular basis to lots of kids.
2. Your earnings or reputation depend on the cumulative percentage of pupils who learned to swim or at least didn't drown.
3. You don't care if a particular pupil learns to swim, but only if a respectable percentage do so, whoever they are.
4. You're never up against a situation in which you have to teach just one pupil to swim (like your own kid).
5. It doesn't bother you, or you don't have time to worry, why any particular pupil didn't learn to swim.

The statistical approach benefits those who deal with many people and are not penalized for failures, but are only rewarded for long-term success. Success is judged from interactions not with one person, but with large populations -- the same sizes, roughly, used to determine the statistically-best procedures. Insurance companies, market researchers, road and traffic planners, psychological testing companies, and professional educators benefit from applying statistical methods to populations because their successes are measured in terms of statistical effects on populations. In these applications, individual injustices, errors, mispredictions, and failures make no difference because they are outweighed by the overall success rate, and it can be proven that there are fewer miscarriages than there would be if the methods were chosen at random.

The statistical approach is seldom of net benefit to the individual. In practice, only a slight (but significant) preponderance of favorable results is required to cause selection of one method over another. This means that rather large numbers of people subject to a uniform treatment are in fact mistreated. In many cases, the payoff matrix for the individual is such that submission to the fixed procedure is probably not warranted, if it can be helped. There is only slightly more than a 50 percent probability that the treatment will be beneficial, but the loss that is risked if the person is not in that category can be very drastic indeed: high expenses, loss of a job, failure to get into or out of college, lack of insurance or a driver's license, failure to recover from an illness, or confinement to a mental hospital. By and large it is better for an individual to avoid situations in which important questions are settled by submitting to statistically-based procedures of any kind. Unfortunately, this is not a choice we are normally given.

I would rather be judged by someone who cared about MY future and who was relying on a coherent theory of MY human nature that I thought was right.

Best, for sure, Bill P.

Date: Fri Apr 24, 1992 4:06 pm PST
Subject: Re: Misc subjects

[Martin Taylor 920424 18:30]

(Bill Powers 920424.0900)

Thank you for your extravagant praise for my AGARD paper. In response to your request for a prospective reference, the paper will be presented at the AGARD Avionics Panel 63rd Symposium, "Advanced Aircraft Interfaces: The machine side of the man-machine interface," Madrid, May 18-22, 1992. There will be a proceedings volume in which the paper will be reprinted. Meanwhile, if anyone else wants to see it, I can leave instructions for our divisional secretary to send copies on request:

Madeleine Plourde
Human Factors Division
DCIEM
Box 2000, North York
Ontario, Canada
M3M 3B9

I will be gone on Sunday, back June 2. Best to everybody.

Martin

Date: Fri Apr 24, 1992 4:14 pm PST
Subject: Statistics

[From Rick Marken (920424 16:00)]

Gary Cziko (920424) says:

>But I wonder if the "input-output" model and group statistics doesn't make
>more sense for the study of reorganization (rather than for
>well-functioning control systems).

-- Description of swimming training methods study deleted

> I
>don't see why doing the statistics to study reorganization necessarily
>implies an input-output view of behavior.

I see Bill Powers just responded to this. I just want to add one point of clarification. I don't think that the use of statistics implies an "input-output" model. The input-output model is implied by the use of IV-DV methodology. Statistics is a separate issue. As I mentioned in my post to Martin, statistical methods can be used in association with the IV-DV methodology (the "psychological statistics" that we know and love) or in association with PCT methodology (the stability factor which is 1-sqrt(var(e)/var(o) where var(e) is the expected variance of a hypothetical controlled variable and var(o) is the observed variance, is one example). There are probably even ways to use IV-DV methodology in a way that is consistent with PCT. For example, I compared quality of control in two conditions -- active disturbance and passively replayed disturbance. So type of disturbance is the independent variable and rms error is the dependent variable. The goal of the study was to analyze control in different conditions -- not to determine the effect of disturbances on output. But the best test of this effect was to use a model to see why this effect occurred. Model based experimentation is the way to go -- and you can use statistics to evaluate the model, of course.

Best regards Rick

Date: Fri Apr 24, 1992 4:15 pm PST
Subject: Re: Genie

[Martin Taylor 920424 1840)
(Bruce Nevin 920424 08:32:21)

>The conclusion drawn from Curtiss's investigations is that normal
>left-hemispheric systems for what is termed grammar or syntax never
>developed. Genie learned vocabulary, and she learned to string operator
>words in appropriate sequence with their argument words in relatively
>short sequences. She excelled at right-hemispheric functions, could
>reproduce complex gestalts in detail. Her talking was mostly about the
>detail of objects in her physical environment; telling stories with
>sequences of events and social relations was beyond her verbal capacity.
>"It was on the gestalt tests that Genie scored higher than anyone in the
>literature. But her portrayal of her complex comprehension was better
>achieved through visual than verbal means." Tests showed brain activity
>in the right hemisphere not only for nonverbal things, but also for her
>use of language, both understanding and speaking. Many anecdotes attest
>that her nonverbal communication was stunningly effective.

A fascinating posting overall.

The quoted bit reminded me very much of something I came across while doing the literature reearch for our "Psychology of Reading." It's a bit old now, and maybe the data are no longer believed, but for what it's worth... (References are included in the original, but I don't feel like typing them all here. Lazy and pressed for time.)

There is a developmental syndrome known as "hyperlexia," which is something of an inverse to dyslexia. "The hyperlexic child has general language and cognitive problems, but nevertheless learns very early to read. Usually this learning is spontaneous: the child reads avidly, even though he may be classified as autistic, not reacting much to people and things in the world around him. Most hyperlexic children have a family with some history of dyslexia. Oddly, hyperlexics may be much better than normal skilled readers
at recognizing words composed of mutilated letters....Although hyperlexics are highly skilled in recognizing words, they are poor in integrating words into sentence contexts.

The hyperlexics appear to have overdeveloped the RIGHT track at the expense of the LEFT [Note: the tracks are the core of my Bilateral Cooperative Theory of reading and symbolic operation. LEFT track processes are normally performed mainly in the left hemisphere, but RIGHT track processes have no preference. Thay are usually done more in the right hemisphere because of resource competition from LEFT track processes, not because of any special competence of the right hemisphere. Roughly speaking, RIGHT track processes provide the meaning, whereas LEFT track processes deal with the form in

language processing. (This is not to say that right-hemisphere
specialized processes don't exist. They probably do, but they are not relevant
in this context.)] Many hyperlexic children are autistic, and autistic
children tend to lack LH specialization for language....
...Resources already developed for one track may be lost in acquired dyslexia,
or resources predisposed to one track may be taken over by the other
in developmental dyslexia or hyperlexia...

What you say about Genie's abilities seem to put her as an extreme example of a case in which the RIGHT track processes have pre-empted the brain resources normally devoted to symbolic processing, leaving her super-skilled at pattern and intuitive-like processes, both in perception and in action. She never had occasion to work on the symbolic processes to which humanity has so lately come, and which presumably are fairly easily lost.

Martin

Date: Mon Apr 27, 1992 4:54 am PST
Subject: more on clinical example

To: Bill Powers, Gene Bogess, interested others
From: David Goldstein
Subject: more on clinical example
Date: 04/24/92

Recall that Bill's advice to me about the man who got caught having sex with the babysiter in the house was: don't psychologize, ask the person. Although, he did not say it, I'm sure he would suggest to use the method of levels when I ask the person.

Gene Bogess came up with some additional possilbe perceptions the man was controlling for in this clinical example. Maybe his wife was not satisfying to him sexually. Then Gene expressed doubts that it was possible in real live clinical examples to pin down the perceptions being controlled.

Well, we have had our second and third sessions. I have asked the man to some degree. Here is what I get. Before returning home on the night when the incident happened, the man and his wife went to the theatre and had a good time except for one thing. The man said his wife embarassed him publically in front of friends. His wife urged him not to buy something and seems to have been obnoxious in the way she did it. He was pissed at her.

The man admitted being angry at his wife for a few months. He says that he is afraid to argue with her. She becomes explosive. He describes himself as passive-aggressive.

Recall that the babysitter invited him to come down and said "You better." The man reported that he went to sleep with his wife. He woke up at about 1:05 am. He was sitting on the bed debating with himself. The deciding thought was: What the hell! It is there waiting for me. The debate made no reference to right or wrong or to principles of any kind. It made no reference to the possible hurt it would cause his wife. At least the man was not aware of these kinds of thoughts at the moment.

As he was walking down the stairs, the stairs made a sound. He thought: I hope she didn't hear that. He describes the experience of having sex with the babysitter as on an equal plane with maturbation. This particular night, the man did not get as far as intercourse. His wife walked downstairs, called his name, and went upstairs. He heard this but the babysitter did not. He went upstairs.

After being caught he has been experiencing tremendous guilt and fear. He says that this has been the worst time of his life. He never wants to repeat it again. It seems that his wife is willing to work things out and will be coming in for marital counseling.

The man describes himself as being an addictive personality. He has used marijuana heavily in the past and alcohol. He has had difficulty in regulating his eating until recently. He does have difficulty controlling the number of hours which he works. He can become obsessive about many things. It seems that once he starts doing something he continues. It is almost as if there is no "enough" point.

The man reports that his mother was a "speed freak" (diet pills) when he was growing up. He indicates when he was a teenager, he did outrageous things in his parents house such as having sex, using drugs and alcohol. He wishes his father would have "put his head though the wall." He plans to be very strict with his two children.

The man states that it was becoming not unusual for him to get up in the middle of the night and go downstairs to play with the computer. He has had difficulties sleeping this past winter. He was showing signs of a bilogical depression and was crying a lot. He was withdrawing from his wife. His wife had asked him what was wrong but he clammed up.

At work, there was a crisis about one month ago. A female colleage complained that he was sexually harassing her. He disagreed with her and told her to "go f___ yourself." He was very worried about this. An official investigation occurred and he was found guilty even though there does not seem to be any sexual intent involved. The man intends to appeal. The female colleague says she will withdraw the complaint but hasn't so far.

While I am not sure if I pinned down the controlled experiences, I am beginning to see the following sort of patterns: The man likes to be stimulated and frequently is curious about "I wonder what would happen if...". When he is too depressed, he takes impulsive action to excite himself. When he is too stimulated, he used to take drugs and food to calm himself. Another controlled experience seems to be: He dislikes when others boss him around and will act in defiant, challenging, oppositional ways. He was not controlling for any kind of moral/ethical principles at the moment he made the decision. He was not controlling for the imagined hurt his wife would experience, he was just thinking of himself. However, one part of him feels very guilty for what he has done.

The weight of all the experiences is pointing to the above kind of conclusions. How is this different from other therapy approaches? The main way, I think, is the focus on the experiences of the moment when the action took place. The man is looked at as controlling some experiences even when his actions seem very much out of control. If the analysis presented above is correct, then the appropriate treatment would consist of him learning different ways of controlling his depression, his anger with his wife. In addition, some work on self-image needs to be done to make him more aware of the different self-images which seem to be operating. There is at least an immature, teenage self-image and a second, more mature self-image at work here which are not integrated.
Applying HPCT to a clinical case consists of trying to identify the controlled experiences. This is the key step. Disturbances can be noted during the therapy discussion of present and past situations. Disturbances can also be introduced through the use of imagination. What if your wife gave you permission to do what you did? Would that change anything? Answer: Yes, the excitement would be gone.

By the way, the man said that what attracted him to his wife when he first met her was: she seduced me. The sex with his wife was supposed to be fine, no problem. So, Gene Bogess hypothesis about what was a controlled experience does not seem consistent with the reported facts.

Date: Mon Apr 27, 1992 4:54 am PST
Subject: bio

[Jeff Dooley 920424]

My Bio

Unfortunately, as an undergraduate in philosophy during the 1960's I learned next to nothing since I thought I already knew it all. This was a grave error, I have come to believe. Chastened, I returned to student life during the 80's for an MS in management cybernetics and immediately afixed my gaze upon a book in the campus bookstore. Yup. _BCP_. Right next to it was Bateson's _Mind and Nature_. I took them home and began to read; then my world turned forever, exquisitely insideout.

I'd worked for a while during graduate school as a mental health para-worker. I spent lots of time on the locked wards and on the streets. I also spent some time in the Santa Clara County Jail talking with staff and inmates. I was struck by the impression that, allowing for disabilities, many if not most of the people I saw appeared to be creating their own stable (if bizarre to some) reality. They were controlling for a particular self-image or interpretation of what the world would throw at them. Many were controlling for defeat and despair. This troubled me greatly; I didn't know how to suggest to them my conviction that they were free to create something--anything--else. Bill's book offered an explanation for how people seemed to be who they were deciding to be, despite the attempts of others to "stimulus" them into being something else. The first thing we started doing as PCT began to sink in was stop trying to change people.

The notion of the self-created reality has stuck with me. My thesis, finished last year, was an argument for a skeptical brand of constructivism supported by Piaget's genetic epistemology. Michael Devitt has called constructivism the most influential bad idea in the history of philosophy; and many others, including Bill Powers and Greg Williams, have gifted me with searching criticism of the apparent ontological implications of the view. I saw Piaget-plus-PCT as a cybernetic model of how it could be possible to organize regularities of perception--including disturbance--into useful signals without those signals having necessarily to correspond with any native attributes of the external world. This seemed license to dispense with at least semantic, if not metaphysical realism. But still chastened, I remain in reorganizing mode on this, inclined to accommodate to science, experience, and common sense, but with a lingering skepticism toward metaphysics.

I work as a graphics systems consultant, have a wonderful wife, Lynn, and a one-and-a-half year old daughter, Johanna, whose world is full of wonder.

jeff dooley dooley@well.sf.ca.us

Date: Mon Apr 27, 1992 4:56 am PST
Subject: Flocking Birds

[from Gary Cziko 920424.2230]

I just finished watching a TV program on PBS called "Inside Information" on the brain as an information -processing machine.

To illustrate how the brain is made of simple units doing simple things out of which emerges coordinated activity without a central processor, they showed a computer simulation of a flock of birds rising from the ground, joining in flight, and avoiding obstacles. Of course, this reminded me of the "Gather" (aka "Crowd) program. But this one works in color and in 3-D!

The credits at the end said is was developed by Rebecca Allen. It might be intersting to find out what kind of model underlies her simulation (can it be anything other than control systems in interaction, whether she realizes it or now?).--Gary

----

Gary A. Cziko Telephone: (217) 333-4382

Date: Mon Apr 27, 1992 4:56 am PST
Subject: paper requests

[Martin Taylor 920425 00.05]

Rick Marken e-mailed me to ask for the paper Bill mentioned. Unfortunately I cannot respond to such requests. I forgot to include our secretary's e-mail address when I left her surface mail address. You should be able to reach her with a request to send the paper at

Madeleine_Plourde@gatormail.dciem.dnd.ca

Martin

Date: Mon Apr 27, 1992 5:19 am PST
Subject: Statistics

[from Gary Cziko 920424.2300]

Bill Powers (920424.1600):

Thanks for helping to restore my faith in the general uselessness of inferential group-based statistics.

>The statistical approach benefits those who deal with many people and are
>not penalized for failures, but are only rewarded for long-term success.
>Success is judged from interactions not with one person, but with large
>populations -- the same sizes, roughly, used to determine the
>statistically-best procedures. Insurance companies, market researchers,
>road and traffic planners, psychological testing companies, and
>professional educators benefit from applying statistical methods to
>populations because their successes are measured in terms of statistical
>effects on populations.

But I fear that the case for inferential statistics is even WORSE than you make out in here, at least with respect to educational research.

And that is because we seldom (if ever) work with random samples from some defined population. So we don't even have a way of knowing if our statistically significant effect can be generalized beyond the sample included in our study. And statistical significance is generally meaningless anyway. (If we get it, it alone doesn't mean that the effect is large enough to be of any practical significance. If don't get it, it just means that our samples were not large enough.)

And even if we DID have a random sample of some population of interest, how would we know (without a generative model of some sort) what in the treatment really made the difference? Would it work with a different instructor in a different pool with older students, etc.? As Greg Williams put it so well a while back, all we would know is that the treatment should be more effective than the control ALL OTHER THINGS BEING EQUAL, but we don't know what these ALL OTHER THINGS are without a model of what's going on.

Trouble is, I don't have a good idea about how to hypothesize and test a working model of reorganization in educational settings. And I can't use inferential statistics anymore. What is a PCT-oriented educator to do?--Gary

Date: Mon Apr 27, 1992 5:23 am PST
Subject: Teaching swimming

[From Rick Marken (920425 08:30)]

Bill Powers -- The "teaching swiming" post goes into the display case with the "behaviorism" and "modeling" posts. I wish I could get some of the "do-gooders" out here to understand it -- even though it is crystal clear.

Joel Judd -- re: Jesus and control theory.
I admit that I do find the myths of the "New" testement (you have a newer one, right) more pleasing than many of those of the old (though it's hard to beat ecclesiates, song of solomon and the psalms). And jesus said some nice things that seem compatible with human nature as PCT views it (the old testament is a bit s-r oriented, no?). But I think that there is one little problem with the attitude in these myths; the authors seem to believe that there is one "right" set of what PCT would call higher level reference signals and one right set of settings for these signals for everyone -- the "god" set. I don't think that PCT knows enough yet to say that this is unquestionably false -- but the current state of the model suggests that this is HIGHLY unlikely -- even for intrinsic variables. A fundemental tenet of the HPCT model is that references for lower level systems must be adjustable in order to produce perceptions that satisfy higher level references (goals). Even the references at the top of the hierarchy are subject to change through reorganization (and I think there is some evidence that changes in system level references do change "involuntarily"). This fits in with Bill's discussion of the problem with statistics. The bible contains a recommended "treatment" for people; it says how all people should behave. Statistically, societies with people who try to behave according to these recommendations (as best as anyone can make out what the hell they are) do better (on the average) than societies that adopt other recommendations (maybe). But these recommendations for behavior are obviously not best for each individual -- and they are often ignored by individuals (usually with no bad social consequences, by the way).

Jesus seems like a well intentioned fellow (or god) but I'm afraid he didn't understand human nature (or statistics).

Just had to get that out of my system before we go to UseNet (if we do).

Best regards (really for sure) Rick

Date: Mon Apr 27, 1992 5:37 am PST
Subject: Usenet Vote Results

[from Gary Cziko 9204]

Here are the results of our vote on whether we should try a trial two-link of CSGnet to Usenet: 19 yes, 10 no.

Since I have posted this proposal to the relevant Usenet group and elicited no opposition, I will proceed with establishing the link.

The link will be invisible to those now on CSGnet, except for those using the local Urbana group INFO.CSG to access CSGnet. These people should prepare to delete this group and add BIT.LISTSERV.CSG-L when I announce that the link has been established.--Gary

Date: Mon Apr 27, 1992 5:37 am PST
Subject: Re: Misc subjects

> Alan Scrivner(920422) --
>
> >Recall that a fuzzy set is an ordered pair consisting of an element and
> >the value of its membership function. Also, sup min{...} is the fuzzy
> >metric ...
>
> Golly, how could that have slipped my mind? Alan, you've said just enough
> to arouse a lot of interest. How about doing a little work on making that
> footpath run both ways (I second Greg Williams' motion)? Is there any way
> the import of the mathematics can be explained to the likes of me?

Bill: I can answer questions about things Fuzzy, but I missed the original point, Greg's or otherwise. What is it you want to know?

O--
>
| Cliff Joslyn, Cybernetician at Large, 327 Spring St #2 Portland ME 04102 USA
| Systems Science, SUNY Binghamton NASA Goddard Space Flight Center
| cjoslyn@bingvaxu.cc.binghamton.edu joslyn@kong.gsfc.nasa.gov
V All the world is biscuit shaped. . .

Date: Mon Apr 27, 1992 6:10 am PST
Subject: (uuuh) religion

From Greg Williams (920425)

>Joel Judd

>... put in a plug for a careful reading of at least New Testament
>christianity. In spite of several hundred years of abuse on the part of
>different denominations, take a PCT look at what we have of Christ's
>teachings.

The last time a PCTer (namely Dick Robertson) took a look at one of Christ's teachings (namely the Golden Rule), he came up with an improvement, by making it more explicit:

1. ORIGINAL (well, after umpteen translations, this is what the masses recite): Do unto others as you would have them do unto you.

2. POSSIBLE INTERPRETATION (especially among zealous missionary-types): Foster your own reference signals in others.

3. PCT VERSION: Help others to achieve their own reference signals (as best you can know them), as you would have them help you to achieve your own.

Maybe 3 is what Jesus really meant. It isn't the way some of his "followers" act. Time to put in a plug at our local churches for a careful reading of at least BEHAVIOR: THE CONTROL OF PERCEPTION?

Greg

Date: Mon Apr 27, 1992 6:49 am PST
Subject: Intro and self-description

I, Avery Andrews, must be unique on CSGNet in not thinking there's too much wrong with Chomsky's actual ideas, as far as they actually go (which is less far than people seem to think, which is part of the problem people have with them). I learned about PCT from being Bill Power's nephew, and having him get me to understand how simple control systems work. My university education was along cognitivist lines (generative grammar, cognitive psychology out of Neisser, etc.). My perception of cognitivism is that it was an excellent idea at the time it arose, since, if behavior is the control of perception, it is a good idea to know something about how perception works (recall that PCT cannot yet explain how we can recognize the refrigerator our beer is in, or tell whether its door is open). But ultimately one cannot escape the issue of where cognitive representations get their meaning from, & some sort of control theory story looks to me like the only hope for a sensible answer.

What I'm currently into is trying to clean up grammatical theory. I see Chomskyan linguists as being between two clauses of a research program that tells them:

(a) they should provide mathematically precise descriptions of idealized versions of languages (the linguistic equivalent to being quantitative, in my view)

(b) they should capture the significant generalizations, and produce restrictive hypotheses about the possibilities for the grammars of natural languages

This is in fact extremely hard to do, when one gets seriously stuck into the details of, say, clitic placement in Romance languages, or case-marking in Australian ones, & what typically happens is that people cannot do both at once but pursue one of the goals to the detriment of

the other (which would be okay, except that they often try to downplay
the importance of the goal they're not pursuing, rather than just admitting
that they don't think they can manage to pursue both at once).

So I'm working with some people to try to produce better ideas
about grammatical organization that will make things that look pretty
complicated turn out to be pretty simple (that's the aim, at any rate;
whether we will manage to do anything useful is another matter).
Some of the guiding ideas are that the formalization should be mathematically
clean and simple, amenable to efficient computation (we're assuming that
sentence-processing isn't miraculous), and that grammatical rules
(or whatever facts about mental structure the rules are (presumably
largely mis-) descriptions of) can be applied in grammatical processing
in a mostly order-independent manner (production and comprehension
do call for rather different orders of application, so making order
mostly irrelevant is a good idea if you want one grammar to be involved
in both).

I'd add that I rather doubt that grammar is a `module' in the Fodorian
sense, though I do think there are various internal formats of grammatical
representation. To me, grammatical processing looks very different
from early vision, inasmuch as sentences are immensely ambiguous, so
grammatical processing has to be closely interleaved with other kinds
of processing. So I'd like to see a sort of `open architecture'
grammatical theory, rather than the `generative monolith' that people
know and often don't like very much.

But my real professional interest (what I'm actually reasonably good at)
is getting sharply focussed and formally precise descriptions of wierd
grammatical phenomena.

Avery.Andrews@anu.edu.au
(currently andrews@csli.stanford.edu)

Date: Mon Apr 27, 1992 10:20 am PST
Subject: Flocking Birds, New Testaments, Reorganization Studies

[From Rick Marken (920427 10:00)]

Gary Cziko (920424.2230) says:

>I just finished watching a TV program on PBS called "Inside Information" on
>the brain as an information -processing machine.

I saw some of it too. Found myself talking outloud at points. What a crock.

>showed a computer simulation of a flock of birds rising from the ground,
>joining in flight, and avoiding obstacles.

> can it
>be anything other than control systems in interaction, whether she realizes
>it or not?)

Nope -- you are correct. It is control systems that are being called s-r
systems. This prevents them seeing some of the possibilities -- like

having systems that change the references for other systems.

Gary Cziko (920424.2300) says:

>Trouble is, I don't have a good idea about how to hypothesize and test a
>working model of reorganization in educational settings. And I can't use
>inferential statistics anymore. What is a PCT-oriented educator to
>do?

I suggest checking out the Roberson and Glines study (Perceptual and Motor Skills, 1985, 61, 55-64). That paper describes a great start at studying reorganization -- on a subject by subject basis. I think that this kind of research should be extended and developed in more detail by skillful, intelligent educational psychologists who understand PCT -- and I know of the existence of only ONE of these (yes, you, boychick).

Greg Williams (920425) says:

>3. PCT VERSION: Help others to achieve their own reference signals (as best
>you can know them), as you would have them help you to achieve your own.

It's reassuring to see that great PCT minds think alike. See my post of (920425 08:30).

Regards Rick

Date: Mon Apr 27, 1992 10:31 am PST
Subject: Oops

[From Rick Marken (920427 10:30)]

Oops -- Gary is not the only educational psychologist I know of who understands PCT. I think Joel Judd is another (and with an official certificate to prove it too). Sorry Joel.

And I think Hugh Petrie would have to count as another (my only excuse is that I think of Hugh as a philosopher of education). Anyway, my apologies to all those who consider themselves educational psychologists who understand PCT.

But do the reorganization study anyway, Gary.

Regards Rick

Date: Mon Apr 27, 1992 11:21 am PST
Subject: intro

[From: Bruce Nevin (Mon 920427 12:18:52)]

A bit of my background coming into CSG, then. Probably a bit scattershot but I'm throwing this down on the PC while the server for my workstation is down for a disk check. As I said some time ago to Judd, No one will ever base an ad verecundiam argument on me. So I might as well make a preemptive strike on ad hominem.

I am a newcomer, having got started participating in CGS-L and reading some of the literature only in about April, 1991. My academic training is in linguistics, and much of my involvement with control theory concerns language. I fear that much that is going on in the field is a dreadful, wheel-spinning waste.

I work as a technical writer for BBN Communications, the division of Bolt Beranek & Newman that makes computer networks and internets, such as we are using to exchange email now. BBN invented and built the Internet, which started out as the ARPAnet. I've been here 10 years.

I was born in Palo Alto 01/23/45. I didn't recognize the sequence in that date until I was 12. I take that as emblematic in a way of the strong pattern and direction in my life becoming apparent to me only after the fact. Palo Alto because my dad was stationed in Redwood City during the war. Both my parents are from Martha's Vineyard. We returned there briefly after the war, then he worked as a radio engineer in Massachusetts. When I was in 3rd and 4th grade we moved around a bit, then settled in Florida. The combination of having started school earlier than most, being a yankee in the Dixiest part of Florida, and living on the outskirts of town in trailer parks surely must have fostered the feeling of being an outsider that I recall.

My undergraduate work started at Florida State in Tallahassee in 1962. In the middle of my sophomore year and went to live for two years in Greece, 1964-65, teaching English (without permit) for a living. This was an important formative experience in many ways, and I would recommend something like it to any adventurous 19-year-old.

I had been frustrated with my classes in German. I thought I wanted an immersion experience. I certainly got it. I became reasonably fluent in Greek, and was often told I had the pronunciation of a native speaker. I also learned a lot of German there, and did quite well in my German classes when I returned. I taught myself to read French, and studied Chinese and Arabic a little. I rationalized my expatriation and this delving into languages in terms of a desire to be able to shift my cognitive mooring from one language/culture world to another and back, the better to apprehend what deeper reality might remain constant under the dislocation. Perhaps the inchoate cravings of a naive Whorfian did in fact underly these explorations. I was mightily impressed by a student of linguistics passing through who attained respectable fluency in Greek in a matter of weeks. He described the same experience over the preceding few months with a dozen other languages. He attributed his quickness to his study of Indo-European historical linguistics and his knowledge of the roots of the modern languages, supported by study of ancient Greek, Sanskrit, Latin, etc.

Shortly after my return from Greece, I got into the University of Pennsylvania, first in a summer program to study Sanskrit with Royal Weiler (just arrived from Columbia, with his students following him), then into the linguistics department.

My strongest influence at Penn was Zellig Harris, one of the great figures in the field of linguistics. He founded the first linguistics department in the country, that at Penn. He is an anarchist (as Chomsky also claims to be--that was something that drew him to be Harris's student). He has arranged nicely for the autonomy of his work. I understand that members of his (wealthy) kibbutz turn over all assets and income to the kibbutz. In turn, the kibbutz supports them in all their needs. For example, the kibbutz bought Harris the apartment house at 22 Charles Street that he lives in near Greenwich Village. With this support, he didn't even need the endowed chair at Penn (Benjamin Frankin Professorship) that further insulated him from political and economic pressures.

One cost of his autonomy, or perhaps more accurately but relatedly his lack of concern whether or not he has lots of followers who are persuaded that he is right, is that he has been marginalized in the field by others who are much more strongly concerned with questions of support for their work and that of their students. From the accepted texts and histories, you would suppose Harris had died in the 1950s, after (according to the standard canard) trying to impose the positivist, behaviorist canons against which Chomsky staged his revolution. But Harris is neither behaviorist nor positivist, his philosophical roots lying with Dewey's naturalism. In various places (e.g. the interview with Mehta) Chomsky has said he never really understood what Harris was doing. I believe him.

It was Harris who discovered linguistic transformations and invented transformational grammar in the late 1940s, while Chomsky was his student. I would not go so far as Gerry Fodor, who some years ago at a conference suggested that there was nothing original in any of Chomsky's work, only a repackaging of Harris, but there is some truth in that view. (I was not there, but am told that Chomsky physically battered Fodor from the microphone and angrily denied it all, so it would seem a nerve was struck.) I can attest from my own experience that espousal of Harrisian theory has been, shall we say, politically inexpedient for me.

After finishing my Bachelor's and Master's degrees at Penn, I wanted to write a grammar of a language very unlike English. Fieldwork seemed the long route (and I can now affirm that it is). Dell Hymes pointed me to texts in the Yana language, phonetically transcribed by Edward Sapir in 1905 or so. Sapir (Harris's teacher) was a towering figure in linguistics and anthropology, a scholar of extraordinary gifts over a broad range, with an ear for language that is legendary in the field. Two of his students had developed a Yana dictionary after his death, but the grammatical analysis of the language had not gone beyond the most rudimentary stage at which Sapir had essentially abandoned it many years earlier, for the sake of work on languages whose informants were more numerous and more cooperative. After I had worked up some Yana slip files for perhaps 6 months Hymes passed on to me an invitation to the first conference on Hokan languages, which he could not attend, so in 1970 I presented a paper in La Jolla on my preliminary findings in Yana. Mary Haas (one of the Sapir students I mentioned) invited me to do subsidized fieldwork on a related and neighboring language, and that's how I got started on Achumawi or Pit River. I did no more with Yana, although Sapir's notebooks from work with Ishi ("America's last wild indian"), lost for many years, were turned up in an attic storeroom of the Kroeber Museum while I was in California. A project involving several faculty and graduate students and a computer database are now carrying that work forward.

During the next four years, I gathered several large looseleaf binders of texts and notes and about 30 reels of tape. I helped organize and fund a Community Center for the downriver bands of the tribe. I worked for a while in the public school system, teaching the language K-12 and an Achumawi "literacy" class to adults while concurrently developing the curriculum and materials, an exhausting and frustrating regimen that I don't recommend. Particularly when being paid as half a teacher aide.

In the internecine conflict dividing Generative linguistics in those years, the department at UC Berkeley came increasingly in thrall of the "generative semantics" faction. The cause of this schism lay ultimately in the unquestioned presumption that paraphrase was the criterion for transformation. I knew that this was unworkable--I was familiar with the reasons that Harris had abandoned judgments of paraphrase and had gone first to a distributional criterion in 1957 (preservation of cooccurrance restrictions under transformation) and then to acceptability gradings in 1965 (preservation of acceptability-differentiation under transformation). The response in Generative Semantics to difficulties with the paraphrase criterion was to follow up all the fine distinctions of nuance and emphasis and reconstruct from them more and more complex and abstract underlying sources for sentences. The opposing view of Chomsky's "((Extended) Revised) Standard Theory" at MIT stressed instead various formal properties of rules manipulating abstract phrase structure trees. Both approaches seemed to me obviously fruitless, a judgment in which all parties essentially concurred not so many years later; but the alternatives I proposed at that time were either incomprehensible to faculty and colleagues, or else immediately categorized as relics from the scrap heap of disproven pre-revolutionary theories because of association with Harris's name. I gave up and left at the end of 1974.

In that year I had begun studies that are beyond the pale for many more people than Generative linguists. It is a curious thing how many people have violent allergic reactions to things they label pseudoscience, or mysticism, or the occult, knowing in advance that it would be a waste of time to find out what, say, palmistry is about in its own terms. It would be a shock to some, perhaps, to learn that the majority of the vast collection of Newton's writings is on the subject of alchemy, of which he was a serious student all his working life. Or that he retorted to a questioner regarding astrology: "I, sir, have studied the subject; you have not."

Sarah was working as a palm reader when I met her. One of the striking cases in her files concerns a withdrawn, troubled young man then in his twenties. There are two sets of hand prints. In the first set, his head line is unusually short, and is weak. (The head line is the middle of three major transverse lines in the palm. With the socalled simian line there are only two, the head and heart lines being merged.) In the second print, taken as I recall less than a year later, the head line is unexceptionally long. His personality was also markedly changed for the better. In the interim, unknown to her, he had been diagnosed with a brain tumor and it had been removed surgically. But of course we know this correspondence (and many others) must be accidental. We know this because there can be no causal chain between personal character and pattern in the palm. And we know that because our theories provide for no such causal chain. And there are no studies showing statistically significant correlations of this sort in populations of brain surgery patients. That, because it is obvious there is no point in looking for any such correspondence. Which is why it would be impossible to get support for such research, or to publish its results. The circularity of foreclosure from scientific discipline seemed oddly familiar. By the way, Sarah would love to participate in serious scientific investigation of her work as psychic and channel.

We were married in New York, her parents' home, then moved to Martha's Vineyard, where I helped my dad build a house for his retirement. I worked in various occupations on the Island, such as running an employment agency and a health food store/restaurant. I wrote a book on astrology, then went to work for its publisher as an editor. That took us to Cape Ann (Rockport). From there I got into technical writing.

I had kept my linguistic files, notebooks, and tapes. I applied several times over the years for funding to resume that work, which I had kept bubbling on a back burner. Reviewers for NSF and HEW approved, but the agency committees themselves turned me down each time. "He never finished his PhD, how do we know we'll get anything for our money." I wrote a review of Harris's 1982 _A Grammar of English on Mathematical Principles_ that was published in _Computational Linguistics_ in 1984. In 1986, Harris gave the Bampton Lectures for that year at Columbia, where he had been teaching and working since his retirement. I went to the lectures and talked with Harris and other old Penn faculty, and met some of Harris's current students. I mentioned difficulty getting funding. One thing leading to another, I was invited to resume matriculation at Penn, in absentia. Though it has not been easy, and the politics are certainly messy, I resumed in 1987, completed course work long distance, and am now at the dissertation stage. I had withdrawn (a kind of suspended animation) to do fieldwork in 1970, so it is now 22 years later. Just call me Rip.

The dissertation will be on an aspect of Achumawi phonology. I intend it to fulfill its purpose of demonstrating membership as efficiently as possible. I just don't have the resources to fight over Harris or PCT with the faculty at Penn, which is now perhaps exaggeratedly anti-Harrisian and who of course don't know PCT from PC. I need an entirely different environment from either Penn linguistics or BBN to move ahead aggresively with the modelling of language as the control of perception. So I nibble away, one small insight, one small identification and abandonment of error at a time.

I believe I have a good nose for truth. At least I hope I do, since I have neither patience nor time for what seems intuititively wrong. This limitation to what I am interested in makes me too subjectively selective to qualify as a scholar. (My failure to study statistics beyond the rudiments is an example.) My involvement with outre' topics I think effectively protects me from the folly of being cited as an authority or running for public office. I have had my livelihood threatened (when I first startd at BBN) because of discussing my book on astrology. I sure get wistful about living and working in an academic environment, though.

I expect some interesting changes in the next year or so.

Bruce Nevin
bn@bbn.com

Date: Mon Apr 27, 1992 11:21 am PST
Subject: Re: (uuuh) religion

from chuck tucker 920427.1400

I would appreciate an exact citation (cite translation also) of the place where Jesus was supposed to have said the "Golden Rule". Do not cite me the Great Commandment - I already know the citations for those statements.

Thanks, Chuck

PLEASE SEND PRIVATELY

Date: Mon Apr 27, 1992 12:04 pm PST
Subject: Usenet Link Established

[from Gary Cziko 920427:1348]

I have been informed:

>You got it. The gateway is operational. A control message has been sent
>out instructing the usenet sites to create the newsgroup. Once it is
>acted upon at any particular site, the group will be available at that
>site.
>
>Let me know if you have any problems or questions.
>
>Jim McIntosh (jim@american.edu)
>The American University
>Washington DC 20016-8019 USA

Some people currently subscribed to CSGnet via listserv may find it more convenient access the group via Usenet group bit.listserv.csg-l. If you want to give this a try, I recommend that you do not UNSUBSCRIBE or SIGNOFF from the listserv but rather just set your mail option to OFF. You can do this by sending the following command to LISTSERV@VMD.CSO.UIUC.EDU (LISTSERV@UIUCVMD.Bitnet):

set csg-l nomail

This will keep you on the list but mail will not be forwarded. If you don't like the Usenet access, you can then get CSGnet posts sent directly to your mailbox again by sending the following message to the LISTSERV:

set csg-l mail

This way there is no need to resubscribe.

In fact, I would prefer that all current CSGnet subscribers who switch to Usenet access keep their name subscribed and just switch to nomail status.

This way, we will know that you are still listening.--Gary

P.S. UIUC users who accessed CSGnet via the local group info.csg will now have to switch to bit.listserv.csg-l.

P.P.S. Some people experience problems sending commands to LISTSERV. The major problem is caused by changing return addresses. LISTSERV gets your address from your return address. So if you signon from one machine and try to change your status from another, you will certainly run into problems since the two return addresses will not match.

Gary A. Cziko

Date: Mon Apr 27, 1992 1:11 pm PST
Subject: Statistical inference and education

[From Bill Powers (920427.1100)]

Gary Cziko (920427) --

>Thanks for helping to restore my faith in the general uselessness of
>inferential group-based statistics.... But I fear that the case for
>inferential statistics is even WORSE than you make out in here, at >least
with respect to educational research.
...
>Trouble is, I don't have a good idea about how to hypothesize and test >a
working model of reorganization in educational settings. And I can't >use
inferential statistics anymore. What is a PCT-oriented educator to >do?

If the hypotheses being tested were model-based, you could still use inferential statistics to test the model. In fact there is a model: y = ax + b, but that's the wrong model (because generally the wrong y and the wrong x are guessed to be related). A control model, as Rick Marken noted several posts ago, can be tested statistically. If you have found a true controlled variable, you will get very high correlations and small scatter in the predictions.

A prediction of our model regarding reorganization is that organization will change at a higher rate when there are failures of control, the error persisting for some minimum time and being above some minimum amount (both to be determined empirically, both thresholds possibly being zero).

You can tell whether people are trying to control by the amount of effort they put out for a given error (it's up to you to devise ways of testing that amount). The ratio of effort to error is the apparent error sensitivity of the system: if it measures high, the person is trying to correct the error and the inner error signal is large. If it measures low, there may be several reasons.

The error you measure, of course, is the external error -- the difference between what the person is supposed to accomplish and what is in fact accomplished. A small action in response to a large external error could result from a person's actions being insensitive to inner error signals, implying either that the person doesn't care much about the inner error signal or that the person doesn't know how to correct the error. It could also result from a more sensitive reaction to an inner error signal, but a faulty comparison process (or difference in definition of the controlled variable) so that what looks like a large objective error to the observer is experienced internally as only a small error.

If the inner error signal is small, the person's control will probably not improve very fast if at all, because reorganization will not start. So by learning how to assess the actual internal error in cases of poor control, you can make predictions as to whether reorganization will occur at a reasonable rate -- i.e., whether learning will happen. This procedure will probably make intuitive sense to teachers, who recognize the difference between levels of "motivation to learn" but have no theory for what that motivation is or what it depends on.

Learning to assess the actual inner error must involve getting to know the student and discussing the student's perceptions and goals relative to the accomplishment one is trying to teach. If there is a low reference level for accomplishing the goal, failure will not create a large inner error signal, and the focus must shift to higher levels of goals. Of course this requires dealing with each student as an individual rather than looking for automatic procedures that can be applied to entire classes without the need for getting involved personally.

I think it helps for people to be taught about reorganization. Once they know what it feels like and what happens as a result (better control) they don't try so hard to avoid getting into states of inner error. They develop some confidence that reorganization will work.

Don't give up on statistics. If you have a good theory of human nature to test, you will get high correlations that don't depend critically on sampling methods, so the problems you mention won't come up. You just have to remember to test the theory against EACH INDIVIDUAL'S behavior and THEN pool the results, rather than, for instance, comparing average level of observed effort against average error in accomplishment. You want to average the ratios of effort to error, not find the ratio of average effort to average error. Of course if you're applying the theory on an individual-by-individual basis, you would pool the results only as a matter of general interest; actual procedures will be varied according to each individual's predicament.

I think that in the long run this approach will result in faster learning even though the teacher must go more slowly in order to concentrate on each individual in turn.

Best, Bill P.

Date: Mon Apr 27, 1992 1:47 pm PST
Subject: Usenet Link Established

[From Rick Marken (920427 13:00)]

>Gary Cziko (920427:1348) says:

>I have been informed:

>>You got it. The gateway is operational.

I guess this means that when we post to the list now it also
goes out to the net. So as a first message to the world:

This group engages in discussions of topics relevant to the control
system model of purposive behavior developed by William T. Powers.
The basic reference on this topic is

Powers, W. T. (1973) Behavior: The control of perception. Chicago: Aldine

This book is available in most libraries.

Other good background information can be found in:

Living control systems:Selected papers of William T. Powers (1989)

2) Living control systems II: Selected papers of William T. Powers (1992)

and

3) Introduction to modern psychology:The control-theory view, Edited by
Richard J. Robertson and William T. Powers (1990)

All published by CSG Book Publishing

If they are not in your library, they can be ordered from

CSG Book Publishing, 460 Black Lick Rd., Gravel Switch, KY 40328 U.S.A.

Regards Rick

Date: Mon Apr 27, 1992 8:47 pm PST
Subject: various comments

[From J Francisco Arocha, 920426; 1810]

I said:

>By perusing several journals I sense that cognitive
science, >has been gaining acceptance among psychologists.

Rick Marken (920423 13:30) replied:

>I'd say it has been the dominant perspective in scientific
psychology >(in the US, at least) since the late 1960's.

I agree, but I was referring to the cognitive science
perspective (to differentiate it from cognitive psychology)
in terms of the building of very general, abstract models,
some of which have factual referents that are NOT specified
and that are supposed to be equally applied to man and
machine. Cognitive scientists frequently talk about
"mechanisms" that are not, strictly speaking, mechanisms,

because they are abstractions, not actual, real processes. Mechanisms produce effects and the only way to produce effects is through material means. This is related to a recent post (by Bruce Nevin, I think) that mentioned a researcher at xerox labs (I think) who developed a computer program (a PhD dissertation) that could produce coherent text without using a grammar (an abstraction). She concluded that grammar rules are in the "heads" of linguists but not in the speakers'. I would agree because grammars are descriptions of regularities in language, not "mechanisms" that produce language.

About "instantiations"

>I think this is the case -- but I'm not quite sure what you mean. >Not being much of a philosopher myself, I think I am eminently >qualified to try to give an answer.

I meant it in the same sense of Plato in the allegory of the cavern: that the material world is an instantiation of a world of ideas. Although it may appear obvious that it isn't to any person living in the 20th century, philosophers and some cognitive scientists have found the way to say the same thing in more "modern" terms, but still expressing the same idea, for instance, in terms of the distinction between software and hardware. As long as is kept in mind that this is a metaphor, it is OK; but some philosophers/AIers take this very seriously. For example, in a recent article in Psychological Science two physiological psychologists argue that psychology is the study of "pure function". They want to discover the "abstract principles of thought". The problem with this approach is that it rests on a semantic confusion between "principles" understood as postulates of a theory, and therefore abstract, with "principles" understood as real processes that govern a natural phenomenon. It is important to separate reality from our models about it.

and

>PCT assumes >that this process is "instantiated" in the nervous system as excitatory >and inhibitory connections between neurons carrying signals in the >form of "neural currents" (spikes/sec).

It could just be a matter of terminology, but I would say "carried out" instead of "instantiated". The reason being that instantiation is a conceptual operation that the theorist carries out, not the nervous system (unless it is the nervous system of the theorist). The only operations the nervous system carries out are electrochemical not conceptual (of course, conceptual operations are the result of electrochemical activities of the nervous system). So, I think that PCT is NOT about instantiated ideas at least in the sense that I intended.

Martin Taylor 920423 17:15

>I think cognitive >psychologY is neutral on the matter. As I understand it, it is about >how we form perceptions of more or less complex abstractions. As such, >it is about the perceptual functions in the control hierarchy as much as >it is about the resulting actions. I think that once you get to the category >level, you have to start worrying about the kinds of things cognitive >psychologists worry about.

Does that mean that at higher levels the differences between PCT and conventional cognitive psychology are not important? What would differentiate it from a PCT approach?

>And I do think PCT is about the instantiation of abstractions. That's what >the neural current that is the perceptual signal is. It represents the >abstraction controlled by that ECS.

The neural current (?) represents the abstraction controlled by THAT ECS? What does the THAT refers to? Could you explain?

Bill Powers (920424.0900)

>I look forward to reading >your thoughts on just where and how the Skinnerian movement went off the >tracks. As Greg Williams notes, Skinner did get as far as doing PC(T)- >compatible experiments, giving the animals control over their own inputs >(reinforcers). But there was obviously some deep-seated belief system that >kept him from seeing autonomy in the behaving system, a belief system that >I think is common to most of the life sciences. What do you think on that >subject?

I agree with your assesment concerning the blindness of Skinner (and his followers) to the autonomy of living beings. I attributed that blindness to his view of science, which was much influenced by his operationism and his alleged atheoretical position. If you believe that science concerns only observables you will never be able to construct deep theories, because this would involve developing (non-observable) theoretical constructs.

Gary Cziko 920424.1500

>Could you give some key Bunge references that you think are most >important?

Bunge is a remarkably prolific author and has written several hundred books and articles. However, a synthesis of his views are in his Treaty on Basic Philosophy published between 1974 and 1988. This work is divided into five parts: SEMANTICS, published in two volumes (Sense and reference and Interpretation and truth); ONTOLOGY, also two volumes: The furniture of the world and A world of systems; EPISTEMOLOGY AND METHODOLOGY (Four volumes: Exploring the world, Understanding the world, Epistemology of Formal Sciences and of Physics, and Epistemology of Biology, Psychology, the Social Sciences and Technology); The final part is one volume: ETHICS. All books are published by D. Reidel of Dordrecht, Holland and cost about $80 each. I think that when reading the Treatise it is important to read the Semantics and the Ontology first, because many of the ideas developed in these books are used in the rest of the books. Bunge's main work concerning specific disciplines has been in the philosophy of physics (he is a theoretical physicist, specialist in quantum physics). He is considered as a "synthesizer" because of his Treatise. As far as I know he is one of the few contemporary philosophers who has written a philosophical system. Bunge has also written a book on the philosophy of psychology and another on the philosophy of linguistics. He has also written a book on the mind-body problem. I don't have the references for these now, but I could post them later.

J Francisco AROCHA

Date: Mon Apr 27, 1992 9:50 pm PST
Subject: uuuh... more religion

Chuck asks about the Biblical source of the Golden Rule. No Bible handy, but our 1961 World Book says (under "Golden Rule," of course) see Matthew 7:12 (the Sermon on the Mount) and also Luke 6:31. The quote given is as follows: "Therefore all things whatsoever ye would that men should do to you, do ye even so to them."

Hope this helps, Greg Williams

Date: Tue Apr 28, 1992 12:03 am PST
Subject: RE: more on clinical example

This is a reply to the recent clinical example of "the man on the baysitter" that David Goldstein posted.

According to the social history on this couple, it seems that the wife has externally expressed control. She seems to attempt to control her husbands experiences as his parents may have. He reports that sex was good with her initially. Could it be that she tries to control, amoung many other variables in this relationship, the ratio of sex the couple has? Then she also tries to control their experiences in social situations with things like embarressment. Then like in his childhood, he passive-aggressively gains control.

In this scenerio, the husband found an experience which he could control because
the babysitter offered it. And his wife could not control this expereience.
His need for control or perceived control was in priority to any other value
perception on the night of the movie. So our immediate gratifier jumped
(a little pun) at the chance to control for the experience of ecstacy
with the babysitter, and his wife could not control this experience.

Our adulter felt great relief in his moment of control. For he had been
deprived
Our adulter felt great relief in his moment of control. For he had been
deprived for so long. But after reorganization, our adulter relized he was an
adulter
because his controlled experience now clashed with the perception of not
cheating on his wife because of personal or religious beliefs (ten commandments)
Also, this guy controls for other maladaptive experiences like staying up late
playing with his computer when he needs sleep. Apparently we all do a little
of this on the net. Well ultimately this guy is experiencing less control
than desired in the marriage and is compensating in maladaptive ways. Starting
with his systems concept and its priority makeup may be a bood place to begin
with him. Then of course the principle and program levels may need a lot of
fine tuning. In regards to the marriage counselling, it seems that she is
goingto need to recognize his needs to control certain expereinces and she is
going
to have to learn to give up some control to do that. And just think what she
might do in resistance that disturbance.

Well, at any rate, this is a good clinical example and my interpretation may
be dead wrong. But it was fun to entertain.

Best regards,
Clifford Gann - a meager graduate student at SFASU, Nacogdoches, TX
but a thesis student of Tom Bourbon!

Date: Tue Apr 28, 1992 9:32 am PST
From: Dag Forssell / MCI ID: 474-2580

TO: csg (Ems)
EMS: INTERNET / MCI ID: 376-5414
MBX: CSG-L@VMD.CSO.UIUC.EDU
Subject: Starter Document
Message-Id: 42920428173224/0004742580NA4EM

[From Dag Forssell (920428)]

Here is one possible outline of the periodic message to be posted as an
introduction to CSGnet. Garys editing goes.

Subject: Intro (to CSGnet) for Usenet

This introduction to CSGnet (Control Systems Group net) provides
information about:

WHY YOU MIGHT READ THE CSGnet

If you are curious about things that are new and exciting...
If you are dissatisfied with the explanations (or the lack thereof) in many of the "soft" life sciences and would like a more rigorous approach that has more power of explanation...
If you insist on thinking things through for yourself rather than accept what the establishment feeds you....

OUR SUBJECT MATTER: THE CONTROL PARADIGM

Human control is the primary subject of CSGnet, but all forms of control are game. Here is a brief introduction by the primary creator and promoter of the control paradigm, William T. Powers:

There have been two paradigms in the behavioral sciences since the 1600's. One was the idea that events impinging on organisms make them behave as they do. The other, which was developed in the 1930's, is PERCEPTUAL CONTROL THEORY (PCT).

Perceptual Control Theory explains how organisms control what happens to them. This means all organisms from the amoeba to humankind.

It explains why one organism can't control another without physical violence.

It explains why people deprived of any major part of their ability to control soon become dysfunctional, lose interest in life, pine away and die.

It explains what a goal is, how goals relate to action, how action affects perceptions and how perceptions define the reality in which we live and move and have our being.

Perceptual Control Theory is the first scientific theory that can handle all these phenomena within a single, TESTABLE concept of how living systems work.

William T. Powers, November 3, 1991

THE PURPOSE OF CSGnet:

CSGnet provides a forum for development of PCT in considerable detail, applications and testing of PCT and the dissemination of PCT to any and all who have a sincere interest in how organisms work.

CSGnet MEMBERS:

Many interests and backgrounds are represented here. Psychology, Sociology, Linguistics, Artificial Intelligence, Robotics, Social Work, Social Control, Modeling and Testing. All are represented and discussed. A challenging quality of participants on this net is that most are prepared to question and re-consider what they think they know, even if it requires that a LOT of previous learning be rejected.

THE EVOLUTION OF THE CONTROL PARADIGM

The PCT paradigm originates in 1927, when Harold Black invented the negative feedback amplifier, which is a control device. This invention led to the development of purposeful machines. Purposeful machines have built-in intent to achieve consistent ends by variable means under changing conditions. Examples are the heating system in your home, which keeps the indoor temperature constant despite the changing seasons and opening doors and the cruise control in your car, which keeps the speed constant despite changing road conditions.

The first use of this concept to better understand people was suggested in 1957 in a paper entitled "A General Feedback Theory of Human Behavior" by McFarland, Powers and Clark. In 1973 William T. (Bill) Powers published a seminal book called "Behavior: the Control of Perception," which still is the major reference for PCT. See literature below.

This book spells out a complete model of how the human brain and nervous system works like a living perceptual control system. Our brain can be viewed as a system that controls its own perceptions. This view suggests explanations for many previously mysterious aspects of how people interact with their world.

Since 1973 an acceptance of Perceptual Control Theory has begun to emerge among a few psychologists, scientists and other interested people. The result is that an association has been formed (the Control System Group), several books published, this net set up for communication and that a dozen professors are teaching PCT in American universities today.

HOW TO DOWNLOAD SPECIFIC INFORMATION

A number of introductory documents are available for you on the (Bill Silverts) listserver. To request one, ... Listserver address; get <document name>

HOW TO ASK EFFECTIVE QUESTIONS

Since PCT puts much conventional, well established wisdom on its head, it is very helpful to begin by demonstrating the phenomenon of control to yourself and studying a few references. It is helpful to study systems and control in general in addition to the texts that focus on PCT. As you catch on to what this is about, read this net and follow a thread that interests you for a month or more.

When you ask a question, please consider that in order to give you a good answer, a respondent will need to put your question in context.

Therefore, please introduce yourself with a statement of your professional interests and background. It will be helpful if you spell out what parts of the demonstrations, introductory papers and references you have taken the time to digest and what you learned.

People on this net are in various stages of learning and understanding PCT. When you get a reply to your post, please consider that the respondent who found your question of interest and invested time in a reply, may benefit from knowing how you perceived the answer. Did it answer your question? Was it clear? Were you able to understand it?

DEMONSTRATING THE PHENOMENON OF CONTROL

The phenomenon of control is largely unrecognized in science today. It is not well understood in important aspects even by many control engineers. Yet the phenomenon of control, when it is recognized and understood, provides a powerful enhancement to scientific perspectives.

It is essential to recognize this phenomenon before ANY of the discourse on CSGnet will make any sense.

Please download the introductory demonstration <Demo> (?kb).

THE CONTROL SYSTEMS GROUP

Serious enthusiasts of PCT have formed the Control Systems Group. This group meets once a year (1992: July 29-Aug 1) in Durango, Colorado, for informal presentations and exchanges. The group also publishes threads from this net. For membership information, download <CSG>

LITERATURE REFERENCES

For a short overview, download <short list> (?kb).
For a more descriptive list , download <descr list> (?kb)
For an extensive list, download <ext list> (72kb)

INTRODUCTORY ESSAYS AND PAPERS

The following papers are highly recommended:

```
Powers, Bill: Introduction to control theory.  <introct> (??kb)
Powers, Bill: Skinners mistake <skinner> (??kb)
Powers, Bill: Modeling    <     >
P             Manifesto   <     >
P             ....
McClelland, Kent: Perceptual Control and Sociological Theory <st> 80?kb
Marken, Rick: The hierarchical behavior of perception.   <behper>
Cziko, Gary: Purposeful behavior... Educ Research.  <   >  (?kb)
```

End of boilerplate suggestion.

Here is a suggestion for <demo>

The following is adapted from: Runkel, Philip: Casting Nets and Testing Specimens. (See literature) pages 105-107. Phil Runkel got it from Powers: Behavior: The Control of Perception, pages 241-44. For a complete discussion and explanation, see these references.

You can demonstrate the essence of this paradigm to yourself, wherever you are, with the simple prop of two rubber bands joined by a knot. Just get a friend to help you play a game. This game will illustrate all the elements of human control, their interactions and functional relationships. Get two rubber bands three or four inches long. Knot them end to end. You hook a finger into the end of one rubber band and your friend hooks a finger into the other. Tell your friend something like: "You are the experimenter. Move your finger as you like. Watch what I do. When you can explain what is causing me to do what I do, let me know."

When you sit down with your friend, place yourself so that the knot joining the rubber bands lies above some mark you can see but which is unlikely to draw the attention of your friend - a small mark on a table top or paper, a piece of lint on your knee, or the like. As your friend's finger moves, move yours so that the knot remains stationary over the mark.

By agreeing to keep the knot over a target, you have adopted a standard for the position of the knot as your want. When something acts to disturb the position of the knot, you will act to restore the knot to its position over the mark. You will move in any way necessary to do that.

You cannot, of course, keep the knot stationary if your friend moves faster than your natural reaction time can compensate. Some people playing this game seem to want to move abruptly, too fast. If that happens, ask your friend to slow down. The lessons to be learned will be much more obvious to both of you if you are able to keep the knot continuously over the mark. You might say, "Don't move so fast that I can't keep up."

Your friend will soon notice that every motion of her finger is reflected exactly by a motion of yours. When she pulls back, you pull back. When she moves inward, you move inward. When she circles to her left, you circle to your left. You must do that, of course, to keep the knot stationary. Your action illustrates very plainly the phenomenon of control - that we act in opposition to a disturbance to maintain a perception we want.

Notice that you perform many different acts to maintain your perception of the knot remaining over the mark. You move your finger to the left, to the right, forward, backward, diagonally at varying speeds.

Most people, when they announce that they can explain what is causing you to do what you do, will say that you are simply imitating what they do, or mirroring it, or words to that effect. Some will put it more forcefully: that whatever they do, you are acting in opposition to it. Almost all will say or imply that they are the cause of your behavior.

A few people will notice that the knot remains stationary, but most will not. Most will say that your intent is to do something in reaction to them. But then you deny that. Those who do not notice the stationary knot will eventually give up and ask, "All right, what is causing your behavior?" Then you explain that you have merely been keeping the knot over the mark.

No, you tell your friend, your purpose has not been to oppose any intention of hers. Your purpose has not been to frustrate her. If, instead of her finger, a machine had been hooked to the rubber band, you would have moved as you did. Your purpose was to keep the knot motionless over the mark; that's all. You moved to oppose any motion of the knot away from the mark, not to oppose her. Your motivation had nothing to do with what your > friend might have been trying to do; you did not care. You watched only the knot and the mark. Indeed, if you had not been able to see your friend's moves, your action would have been identical.

Reactions of "experimenters" will vary widely. A few will accuse you of being devious and go away grumbling. Most will be surprised, even dumbfounded, to have missed the obvious. A few will find many of their previous ideas so shaken that they will think about it for days or weeks afterward.

Suppose you play this game with 10 of your friends and only one is able to explain that you were only watching the knot over the mark. That means that 9 out of 10 fail to recognize the phenomenon of control when it is right in front of them. They have never been shown what control is or how to recognize it. Without a paradigm of control, they are quite literally blind to a phenomenon that is a vital characteristic of all living organisms.

Here is my <short list> for consideration

Books to provide introduction, applications and perspective on Perceptual Control Theory.

Powers, William T., Behavior: The Control of Perception. Hawthorne, NY: Aldine DeGruyter, 1973, 296 pages. The foundation of PCT! A seminal book.

Robertson, Richard J. and Powers, William T., editors. INTRODUCTION TO MODERN PSYCHOLOGY; The Control Theory view. Gravel Switch, KY: The Control Systems Group, 1990, 238 pages. Textbook on psychology for universities. Highly recommended.

William T. Powers, LIVING CONTROL SYSTEMS: Selected Papers. Gravel Switch, KY: The Control Systems Group, 1989, 300 pages. A collection of previously published papers.

William T. Powers, LIVING CONTROL SYSTEMS II: Selected Papers. Gravel Switch, KY: The Control Systems Group, 1992, ??? pages. A collection of

previously unpublished papers.

Marken, Richard S., editor. PURPOSEFUL BEHAVIOR: The Control Theory approach. Thousand Oaks, CA: Sage Publications: American Behavioral Scientist, special issue. Vol. 34, Number 1. September/October 1990. 11 articles, 16 contributors, 121 pages. A very readable introduction to a science of purpose and supportive research. Highly recommended.

Runkel, Philip J., CASTING NETS AND TESTING SPECIMENS. New York: Praeger, 1990, 186 pages. Contrasting the proper and improper uses of statistics with modeling for understanding and prediction of people as well as processes. Highly relevant to TQM efforts!

Hershberger, Wayne, editor, VOLITIONAL ACTION, CONATION AND CONTROL. Advances in Psychology 62. NY: North-Holland, 1989. 25 chapters, 33 contributors, 572 pages.

Ford, Edward E., FREEDOM FROM STRESS. Scottsdale AZ: Brandt Publishing, 1989, 184 pages. A highly readable introduction to PCT and a personal problem solving guide. The most accessible text available. Written as a comprehensive counseling story anyone can relate to.

Gibbons, Hugh, THE DEATH OF JEFFREY STAPLETON; Exploring the Way Lawyers Think. Concord NH: Franklin Pierce Law Center, 1990, 197 pages. Textbook for law students which spells out how lawyers think by explaining and using a PCT framework.

McClelland, Kent, PERCEPTUAL CONTROL AND SOCIOLOGICAL THEORY. 1991. This unpublished paper suggests that individual control as a phenomenon is central to understanding sociology. <st>

McPhail, Clark, THE MYTH OF THE MADDING CROWD. Hawthorne, NY: Aldine de Gruyter, 1990, 265 pages. Explains group behavior as a function of purposeful individuals.

Petrie, Hugh G., DILEMMA OF ENQUIRY AND LEARNING. Univ. of Chicago press, 1981. Discusses learning with explicit recognition of PCT insight.

Richardson, George P., FEEDBACK THOUGHT IN SOCIAL SCIENCE AND SYSTEMS THEORY. Univ. of Pennsylvania Press, 1991, 374 pages. A review of systems thinking in history, cybernetics, servo mechanisms and social sciences. Provides a perspective placing PCT in context in relation to other paradigms of human behavior.

Books to provide background on systems thinking, scientific thinking and learning.

Barker, Joel A., FUTURE EDGE. New York: Morrow, 1992, 228 pages. A popular discussion of the role and power of paradigms in our lives. Recommended.

Kuhn, Thomas S., THE STRUCTURE OF SCIENTIFIC REVOLUTIONS. University of Chicago Press, 1970, 210 pages. A landmark book! Introduces the concept of paradigms and explains how the struggle of ideas advances science. Basic reference for Future Edge above.

Magee, Bryan, PHILOSOPHY AND THE REAL WORLD; An introduction to Karl Popper. La Salle, Illinois, (1973) 1985, 120 pages. Popper and Kuhn are both great philosophers of science. Read both and ponder how you have convinced yourself of what you think is true in your world.

Senge, Peter M., THE FIFTH DISCIPLINE; The Art & Practice of the Learning Organization. New York: Doubleday, 1990, 413 pages. An excellent introduction to systems thinking and the pervasive presence of systems influences in our environment. Includes an introduction to the idea and phenomenon of control processes as special cases of systems.

Wurman, Richard S., INFORMATION ANXIETY; What to do when information doesn't tell you what you need to know. New York: Doubleday, 1989, 360 pages. Refreshing insight and presentation of the processes and requirements for comprehending new information.

Ackoff, Russell L., THE ART OF PROBLEM SOLVING, accompanied by Ackoff's Fables. New York: John Wiley, 1978, 208 pages. Dr. Ackoff employs systems thinking in his witty, literate and convincing discussions of real problems faced by real managers around the world.

....

I hope this will prove helpful. Best to all.

Dag Forssell
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Date: Tue Apr 28, 1992 10:11 am PST
Subject: divergence and convergence?

[from Joel Judd]

Jeff Dooley--

You in northern California (I don't think there's a whole lot of Santa Clara Counties)? Where exactly? I'm from San Jose originally. Let me know. [excuse me if I didn't get so excited about Rick or Dag being from CA--they're in the wrong part of the state.]

Rick, Greg, Chuck, physicists, etc,

I have a sort of inquiry. I wasn't going to pursue it for now but I think (hah) I can state it succinctly.

Re: the Bible comments, there is still a feeling that religion tells people what to DO; there is a lot prescriptivism to the Bible and other scripture. People in general, not just PCTers, often have an aversion to being told what to DO, even when it may save a life, or prevent injury, for example. However, I think that there can be some divergence at lower levels, but convergence at higher levels. In religion, this would relate to getting to "Heaven." But let's use a more mundane example. I'm getting a degree in

Education. So is the guy down the hall. But his five years have been spent learning about and practicing counseling psychology, while mine have been spent studying neuropsychology and teaching English as a Second Language. Yet we are both getting Education degrees. We both had to enroll, pass a prelim and final, submit a dissertation, pay the fees, etc. Yet noone would say that we DID the same things. There are requirements that EVERYONE must fulfill, yet much leeway in how they are fulfilled.

Now returning to religion, I read once a comparison made by a church leader between the seemingly "rigid" requirements of religion (Christian, in this case) and natural phenomena. People balk at the idea of "requirements" to get to a higher place (or perhaps they balk at the idea that a *man* purports to know what these are--that's another problem). Anyway, he said that we shouldn't be surprised that a God would place requirements on us, as we can see limits placed on things all around us. For example, water boils at 212 F (assuming we're not trying to make cocoa on Everest, of course). Now we can heat the water anyway we want, as long as the water's temperature reaches 212F. Dancing around the pot and chanting doesn't get it.

My basic question is-- and this sounds familiar--why not high level convergence, and low level divergence (ignoring for the moment the "who's gonna decide which high level values" problem)?

Date: Tue Apr 28, 1992 10:38 am PST
Subject: Society

[From Hank Folson (920428)]

Rick Marken (920424 08:30) said:

>Excellent post, Hank. Thank you, Rick.

>More than ever in my lifetime it seems that the world is bound
>and determined to solve it's problems by controlling people.

>problems come from the fact that we have let people get out of
>control. So the proposed solutions are more laws, more police,
>more jails, more regulations, more death penalty, stricter
>moral codes -- control, control, control.

You are referring to politicians spending beyond resources, legislative systems imposing coercive laws, judges setting inappropriate sentences, people selling destructive drugs, people using destructive drugs, citizens not voting, etc.

In PCT terms, these are not problems, these are behaviors. Why are you, a control theorist, addressing only behaviors? What might the controlled variables be that cause these behaviors that concern you (and me)?

>If people don't want to understand it then, fine, we are no worse off than before.

I have seen clever writers present a series of quotes describing the sad state of our society today, and then identify the quotes as from ancient China, ancient Greece, England in 1400, the USA in 1900 and so on. The implication being that history and societies repeat themselves. Until Bill Powers came along with PCT, this was understandable. Now that we understand PCT, we can see that societies were bound to misdiagnose their troubles and take inappropriate corrective actions because they would not be isolating the controlled variable. If we perform The Test on each area of concern in our society, will we not produce better solutions than if we concentrate on behaviors?

>But I think that the potential benefits of understanding PCT outweigh the potential unpleasantness associated with trying to teach it.

Ditto

Hank Folson, Henry James Bicycles, Inc.
704 Elvira Avenue, Redondo Beach, CA 90277
310-540-1552 (Day & Evening) MCI MAIL: 509-6370 Internet: 5096370@MCIMAIL.COM

Date: Tue Apr 28, 1992 11:10 am PST
Subject: Misc stuff

[From Rick Marken (920428 09:00)]

Using UseNet will take some getting used to (reorganization) so this is just a start at getting used to it. I see that posting to CSG-L or UseNet sends articles to both locations. I wonder if a post from a non-CSG-L subscriber to UseNet also goes to the listserver? If so, maybe I'll just get CSG-L mail through the listserver (which is easier for me to deal with) and unsubscribe to the UseNet group. What's the story on this Gary?

Bruce Nevin -- Thanks for posting your very interesting bio.

I hear that Penelope Sibun is on CSG-L. She developed the text generation system that produces grammatical text without using grammatical rules. Perhaps she would be willing to discuss her approach on the net. It seems quite interesting.

I do have a language question for the language freaks out there. I like the idea (though I don't thoroughly understand it) that language is structured to some extent by the structure of our perceptual experience. And I like the idea that the grammatical rules of language are perceptions of the linguist, not necessarily of the speaker. But it is also a fact that people DO try to control their perception of some linguistic rules -- even when "violation" of these rules creates no semantic problms. For example, my wife and daughter are very keen on the proper use of "I" and "me" -- as in "Jim and I went to the store". I would be corrected if I said "Jim and me went to the store". I frequently make this kind of error (I don't perceive the error usually -- because I'm not controlling for that particular rule) -- but it creates an error for the ladies in the house -- and I get politely rebuked. I have my own little grammatical fetishes too. I tend to correct people who use "less" and "fewer" incorrectly (not "less beads", "fewer beads"). So it seems that some aspects of grammar seem to be perceived and controlled by people sometimes. Perhaps this aspect of language is controlled all the time -- people just have slightly different references (at the relationship and program levels) for what aspects of "grammar" are controlled.

What say ye, linguists?

Regards Rick

Date: Tue Apr 28, 1992 12:41 pm PST
Subject: Usenet to Listserv

[from Gary Cziko 920428.1215]

Rick Marken (920428 09:00) asks:

>I wonder if a
>post from a non-CSG-L subscriber to UseNet also goes to the
>listserver?

Yes, it should. In fact, the way things are set up right now (wide open mode), anyone can also post to CSGnet via the listserver.

>If so, maybe I'll just get CSG-L mail through the
>listserver (which is easier for me to deal with) and unsubscribe
>to the UseNet group. What's the story on this Gary?

This is not consistent with your previous question. I think you reversed listserver and UseNet here.

I presume you are considering unsubscribing to the listserv CSG-L list and accessing via UseNet. I recommend staying on the listserv list (so we don't forget who you are), but setting your option to nomail. To do this, send the following command to LISTSERV@VMD.CSO.UIUC.EDU:

set csg-l no mail

--Gary

Date: Tue Apr 28, 1992 12:54 pm PST
Subject: Re: Society

(Henry James Bicycles Inc) writes:

>Rick Marken (920424 08:30) said:

>>More than ever in my lifetime it seems that the world is bound
>>and determined to solve it's problems by controlling people.

>>problems come from the fact that we have let people get out of
>>control. So the proposed solutions are more laws, more police,
>>more jails, more regulations, more death penalty, stricter
>>moral codes -- control, control, control.
>

>You are referring to politicians spending beyond resources, legislative
>systems imposing coercive laws, judges setting inappropriate sentences,
people
>selling destructive drugs, people using destructive drugs, citizens not
>voting, etc.

Not really. I only see "coercive laws" as an example from your list of
what I was talking about. In fact, selling and using "destructive"
drugs is one of the things that would most emphatically NOT be on my
list. I was referring to "proposed solutions" to social problems that
use coercion in the service of getting other people to start or stop
producing some particular behavioral result.

>In PCT terms, these are not problems, these are behaviors. Why are you, a
>control theorist, addressing only behaviors? What might the controlled
>variables be that cause these behaviors that concern you (and me)?

I'm not sure what you mean here. I am quite aware of the fact that people
are controlling perceptual variables; I am also aware that observers
see the means and ends of this process as "behavior". What concerns me
is not "the behaviors" -- the symptoms of control. What concerns me is
the efforts well intentioned people to control their own perceptions of
the behavior of others through the use of coercion. I am worried
about the people who want to restrict people's access, for example, to
drugs and/or throw them in jail because they don't like to see the behavior
called "taking drugs".(I suppose I have to note that I, personally, DON'T
like or take drugs -- other than the odd beer --and I don't particularly
like to see others taking them. But then, I don't like 98% of what I see
on TV or hear on the radio either.)

> Now that we understand PCT, we can
>see that societies were bound to misdiagnose their troubles and take
>inappropriate corrective actions because they would not be isolating the
>controlled variable. If we perform The Test on each area of concern in our
>society, will we not produce better solutions than if we concentrate on
>behaviors?

Again, I'm not sure what you mean. If you mean to use the test on an
individual
basis to figure out what people are controlling and then, to the extent
that they are unsuccessful, helping them control it, YES, it will make
things better. However, if you mean that by using the test we can control
social problems more effectively, then NO.

What I was trying to say in my previous post is that there is no way
to MAKE society better -- that is, to control it -- other than by
recognizing that society is made out of individual control systems that work
best (and, I believe, work together best--this is my guess) when they
are ALL able to control what they need to control; that is, when the
set of 250,000,000 simultaneous equations (for the US) can be solved
for all the unknowns (each equation's controlled variables) simultaneously.
The PCT orientation is to help people control -- and not judge whether or
not you think it is something they should control (of course, you can't
help making that judgement if their efforts to control interfere with
your efforts to control; when that happens you are probably running into
the degrees of freedom problem -- not enough resources available to allow

everyone to control. The PCT solution to the degrees of freedom problem is not very original -- population stabilization and non-piggy resource usage).

Best regards Rick

Date: Tue Apr 28, 1992 1:31 pm PST
Subject: conflicting grammatical norms

[From Avery Andrews (920428 10:49)]

(Rick Marken (920428 09:00)

I don't have strong beliefs about what's going on when people criticize each other's grammar, but in the cases that Rick describes it looks like a contest between two different norms that are in a sense equally valid. Much of the time, people assimilate to different norms with out noticing it: when I went to Australia I started writing `analyze' instead of `analyse' without even noticing it, but then there are these `shibboleths' such as the ones that Rick describes that seem to be recurrent bones of contention.

One possibility is this: one aspect of language is that it marks one's social identity (sort of like an IFF system), and some of these norms probably get involved with particular systems of attitudes that people want to uphold (it is a standard observation that women are keener on adhering to the norms of prescriptive grammar, supposedly due to a greater interest in appearing to be `respectable'). There is a great deal written on these topics in sociolinguistics (Bill Labov is probably biggest name), which it would probably be worth thinking about from a PCT point of view. `Sociolinguistics', by Peter Trudgill is a nice introduction to this field.

And, the nominative case-marking rule that Rick is following is perfectly sensible (the actual subject is nominative, but parts of the subject are not), it's just different from the ones that the female members of his family prefer.

Avery.Andrews@anu.edu.au
(currently andrews@csli.anu.edu)

Date: Tue Apr 28, 1992 3:14 pm PST
From: marken
EMS: INTERNET / MCI ID: 376-5414
MBX: marken@aero.org

TO: * Dag Forssell / MCI ID: 474-2580
Subject: Starter Document

Hi Dag

This is direct to you.

I think the prototype starter document looks great. Thanks for doing all that work.

I hope all is well with you guys.

I was relatively disappointed about not getting the very small promotion to section manager -- but I'm OK with it now, mainly because the guy who got it is a nice fellow and because everyone I talk to expresses their shock that it was not me. I actually think it was a good decision -- given my level of committment to this job (I'd LOVE to get back to teaching or anything having to do with what I really love to do -- PCT of course).

I have seen annette b in the hall occasionally but she is always deeply enmeshed in conversation so I have not been able to talk to her. When I first talked to her (about you) I suggested that I would be happy to participate in the company's TQM program if she wanted -- and she expressed great excitment about that prospect. Apparently, once she found out that I really believed in the importance of personal control over one's experience she lost her enthusiasm. Actually, I don't know what is up with her; I'll keep trying to find out what she thinks TQM is about.

I'm pretty sure Linda will be coming along to Durango this time. I hope Christine is coming along too. Linda would really like to see her there.

Best regards to you both

Rick

Date: Tue Apr 28, 1992 9:42 pm PST
Subject: types of rules

> I hear that Penelope Sibun is on CSG-L. She developed the text generation system that produces grammatical text without using grammatical rules. Perhaps she would be willing to discuss her approach on the net. It seems quite interesting.

well, i'm here, but i'm still trying to figure out what y'all's interests are, and trying to read everything i've gotten so far (i'm already on several high-volume lists!).

but i can offer a couple remarks on your question.

> perceptual experience. And I like the idea that the grammatical rules of language are perceptions of the linguist, not necessarily of the speaker. But it is also a fact that people DO try to control their perception of some linguistic rules

i think the best way to look at this is: just as we might distinguish grammar rules used descriptively by linguists from whatever ``rules'' might be in speakers' heads, we can distinguish both of these from rules that we learned in school. linguists' rules are supposed to be descriptive, that is, they're supposed to describe some natural language. they address the question of ``what do people say?'' (in reality many linguists' rules address the question of ``what sounds ok to me?'' chomsky and others have argued that these questions are equivalent.) the stuff you learned in school (or wherever) is just that; it's often referred to as prescriptive grammar. it doesn't necessarily have anything to do with reality (how people speak) or intelligibility or sense. for example, i can usually inflect ``who'', and actually say things like, ``whom did you see in here?'' this is ``correct,'' but it generally confuses and annoys people.

that many of us go around ``correcting'' how people speak, based on something we were taught, is not particularly revelatory of how language works; it's a social practice, much like correcting people on which fork to pick up or how to hold their wine glass, which practices shed little light on how people use such tools to ingest food and drink.

--penni

Date: Tue Apr 28, 1992 10:59 pm PST
Subject: Re: Convergence/Divergence

From Greg Williams (920428)

>Joel Judd (920428)

>However, I think that there can be some divergence at lower levels, but
>convergence at higher levels. In religion, this would relate to getting to
>"Heaven."

But, speaking purely empirically rather than normatively, it appears that there are wide divergences in notions of "Heavens" and about whether getting to one of them is desirable.

>My basic question is-- and this sounds familiar--why not high level
>convergence, and low level divergence (ignoring for the moment the "who's
>gonna decide which high level values" problem)?

Last year I began an extensive review of the literature on first-person reports about "ecstatic" or "mystical" experiences, both sacred and secular, Western and Eastern, because I was interested in Bill's Method of Levels as a psychological explanation of "the primordial experience" of unity with the universe. I've not gotten completely through the accounts I've been able to locate, but I've read enough to conclude (tentatively) that there is indeed a "convergence" in the expressions of essentially all mystics who have reported on their experiences in fair detail. The convergence has to do with the loss of desiring (in accord with what you might expect at the top of a PCT hierarchy, where there is no higher-level reference signal to be satisfied).

But there seems to be a split in the way the Western and Eastern mystics talk about the no-desire state. The Westerners tend to use theological language and talk about "their" desires being replaced by "God's" (or, in some sense, "the whole universe's") desire, while the Easterners tend to just say that "their" desires simply went away. If there is convergence at the highest level, the way the experience of that state informs the rest of one's life seems to be highly dependent on the non-mystical aspects of one's religion. In other words, in this case, low-level divergence possibly OBSCURES high-level convergence. I can even imagine sects persecuting each other's members for reporting the "same" phenomena in heretical terms!

Greg

Date: Wed Apr 29, 1992 11:46 am PST
Subject: types of rules, heaven

[From Rick Marken (920429 08:30)]

penni sibun (920428) :

Hi penni. Welcome to CSG-L. You say:.

>well, i'm here, but i'm still trying to figure out what y'all's
>interests are

Everything that has to do with living control systems.

>that many of us go around ``correcting'' how people speak, based on
>something we were taught, is not particularly revelatory of how
>language works; it's a social practice, much like correcting people on
>which fork to pick up or how to hold their wine glass, which practices
>shed little light on how people use such tools to ingest food and
>drink.

I agree that it doesn't say much about the "correctee"; my point was that it says something about the "corrector" (in both the language case and the social practice case). In the language case it suggests that the corrector is perceiving something about the correctee's speech, comparing it to a reference specification for what that perception should be and acting (by saying something) to try to get the perception closer to that reference. In other words, the "corrector" is trying to control his or her perception of some aspect of the language of someone else. Of course, this is not very effective because there is no causal link between the actions of the "corrector" and the language behavior of the "correctee". However, the fact that the "corrector" tries to control a perception of the language of others suggests that he or she could control this aspect of his or her own language.

Note that I have been ambiguous about what it is about the language of the "correctee" that the "corrector" is correcting. This is because, from a PCT point of view, the only thing that the "corrector" could be correcting is his or her own PERCEPTION of some aspect of what we are calling "language". One goal of research in PCT would be to figure out the best way to characterize what it is that the "corrector" is correcting -- ie. what perception is being controlled. This is called the test for the controlled variable. It works like this - 1) hypothesize what it is that is being controlled; this hypothesis should be quite precise. I'm not a linguist so I can't come up with good examples. But let's say you guess that the controlled variable is a rule like "make it past tense by adding ed". 2) test this hypothesis by producing language that follows and does not follow the hypothesized rule 3) watch to see whether the "corrector" (now the subject) corrects only those examples that violate the rule and makes no response to those examples that follow the rule; if you can predict the subject's response to EVERY language example, then you probably have guessed the controlled variable. If there is even 1 deviation from prediction then 4) revise the hypothesis about the controlled variable (taking into account the subject's previous corrections) and go to (2).

My point is that "correcting" can be very informative about what perceptual variables INDIVIDUAL people are trying to control. Indeed, "correcting" is very strong evidence that people are controlling (rather than just reacting). "Correcting" implies that something about the current situation (as experience by the corrector) is WRONG. This implies that the corrector him or herself has some idea about what is RIGHT. This is the essense of control: knowing the way you want things to be (what is RIGHT) and doing what you can to get these things (perceptions) to be that way.

Note also that I don't mean to imply that people who control for the perception of certain language rules necessarily generate all their utterances to be consistent with these rules. To the extent that any person controls rules at all, this is probably just one aspect of the phenomenon of language that can be or is controlled.

Greg Williams (920428) on convergence/divergence:

>Last year I began an extensive review of the literature on first-person
>reports about "ecstatic" or "mystical" experiences, both sacred and secular,
>Western and Eastern, because I was interested in Bill's Method of Levels as a
>psychological explanation of "the primordial experience" of unity with the
>universe.

>there is indeed
>"convergence" in the expressions of essentially all mystics who have reported
>on their experiences in fair detail. The convergence has to do with the loss
>of desiring (in accord with what you might expect at the top of a PCT
>hierarchy, where there is no higher-level reference signal to be satisfied).

> In other
>words, in this case, low-level divergence possibly OBSCURES high-level
>convergence. I can even imagine sects persecuting each other's members for
>reporting the "same" phenomena in heretical terms!

This was great Greg. Is there any chance that you could discuss your findings at the meeting this Summer? I want to know more.

Best regards Rick

Date: Wed Apr 29, 1992 3:21 pm PST
Subject: Re: Misc stuff

Rick's questions about what aspects of grammar are controlled for intrigues
me too. I think that control theory would say that we learn about socially
acceptable ways of speaking, and that we have some goal of speaking in
socially acceptable ways, to match or feed into a higher goal of being
socially
accepted in general. This would account for behavior like correcting
our spelling or our own grammar. As for correcting others', unless it's
part of our jobs in grading papers, I guess we are trying to control
other people. But this might be motivated my an urge to reaffirm our
own sense of what is socially acceptable...or wvwn justy what _works_
to communicate. What interests me is that we also use conventional
signals like phrases, clauses, sentences and even types of paragraphs
(e.g., abstracts, problem statements) partly because we are told to by
our editors, teachers, etc., but partly because we seem to choose from
a set of acceptable signals those which we feel can get across our meaning.

How would a control theorist state this?

Cindy Cochran
Dept. of English UIllinois/Carnegie Mellon

Date: Wed Apr 29, 1992 3:35 pm PST
From: g cziko
EMS: INTERNET / MCI ID: 376-5414
MBX: g-cziko@uiuc.edu

TO: * Dag Forssell / MCI ID: 474-2580
Subject: Re: Starter Document

Dag (direct):

Thanks so much for the starter document initiative. I will try to fill in
the blanks soon and try to get this revised as necessary and available to
new netters.--GAry

Date: Wed Apr 29, 1992 3:54 pm PST
Subject: miscellanea

[From: Bruce Nevin (Wed 920429 14:22:10)]

>From: Avery Andrews <andrews@CSLI.STANFORD.EDU>
>Date: Sun, 26 Apr 1992 11:53:43 PDT
>
>I, Avery Andrews, must be unique on CSGNet in not thinking there's
>too much wrong with Chomsky's actual ideas, as far as they actually
>go (which is less far than people seem to think, which is part of the
>problem people have with them).

Chomsky is a master of creative ambiguity, who invites sweeping
interpretation while retaining always the ability to claim he only
intended a very narrow, technical interpretation--a claim that he
invokes only when necessary, otherwise allowing the sweeping
generalizations and encomiums to stand. Caution is in order.

> sentences are immensely ambiguous, so

>grammatical processing has to be closely interleaved with other kinds
>of processing. So I'd like to see a sort of `open architecture'
>grammatical theory, rather than the `generative monolith' that people
>know and often don't like very much.

Sounds good to me. From the beginnings we have seen IBM marketing here--you have to buy the whole package, nothing unbundled.

(Joel Judd Tue, 28 Apr 1992 10:36:00) --

>Re: the Bible comments, there is still a feeling that religion tells people
>what to DO; there is a lot prescriptivism to the Bible and other scripture.

My understanding is that e.g. the 10 commandments are better understood as descriptions of how you will be when you have matured a bit.
One can take descriptive attributes of any esteemed person as a source of reference perceptions, but there are problems getting underneath the personal particulars to useful principles. The religious precepts are attempts to do that for the student. The finger-shaking severity of puritans of every age and clime mostly represents denial and projection in a climate of blame that is usually passed on as a family heirloom.
Just clouds the issues.

(Rick Marken (920428 09:00)) --
(Penni Sibun Tue, 28 Apr 1992 18:05:47 PDT) --
(Avery Andrews (920428 10:49)) --

Bill Labov describes the disparity between one's model of correct language and one's actual performance, measured by what he terms the "index of linguistic insecurity." This index (disparity) is greatest for members of social classes that are upwardly mobile and downwardly vulnerable. Simplifying: upper-class people speak with an upper-class dialect, lower-class people speak with a lower-class dialect, middle-class people speak with more upper-class features in careful speech (formal occasions, during interview, public address, etc.) and with more lower-class features otherwise (interview interrupted by telephone conversation, etc.). The index shows up in the difference between their self-assessment of their performance and their actual performance. People genuinely believe they are doing as they say they do rather than as they actually do.

> linguists' rules are supposed to be
>descriptive, that is, they're supposed to describe some natural
>language. they address the question of ``what do people say?'' (in
>reality many linguists' rules address the question of ``what sounds ok
>to me?'' chomsky and others have argued that these questions are
>equivalent.)

A problematic position to take. They really have left themselves no choice, however, since they rejected the analysis of records of what people utter as being too "data-bound" (Chomsky's term).

PCT shows how we might chart a middle course. What we are after are the reference perceptions that people aim at in their use of language. This approximates the Generativist notion of linguistic intuitions (with the caveat that a lot of their "intuitions of rules" etc. turns out to be properties of hierarchical perceptual control). Deviation and variability reflects in large measure differences in gain at different points in the hierarchy--roughly, how much attention you are paying to matching a particular reference perception, how important it is to you to reduce that mismatch/error to zero. This covers the sort of variability that David Stampe talks about as lenitions and fortitions in phonology, for example. There is other variability in the case of a high "index of linguistic insecurity" reflecting the existence of two partly intersecting systems of reference perceptions, one associated with a self image as a member of one group, one with another. Can't explicate that without accounting for the fabrication and maintenance of personalities in/by the control hierarchy.

In this middle course, records of what people actually produce (behavioral outputs) are useful as indicators of resistance to disturbance. That is how we find out what the reference perceptions are. Introspection has its limits. We deceive ourselves too easily, we lose our bearings too easily. Generations of linguists are now former native speakers of English, having got used to all sorts of peculiar utterances predicted by abstract theories. Some extensions beyond fully acceptable usage are necessary, if only to account for language change and variation. Only the Test for Controlled Perceptions can show which are real and which are bogus artifacts of mathematical formalism (such as iterated center embedding).

(Greg Williams (920428)) --

Yes, at the top of the control hierarchy there is no one home. This does not prove that there is no God or One or Great Spirit or whatever. It is only a matter of the limits of the perceptual control hierarchy. I believe that the higher levels of the control hierarchy are there because we make them up and teach them to our children, and that has been going on for a very long time. (Animals teach "cultural" things to their offspring--sorry, I had a citation of research on that but not handy now.) But at the top there is no control. There is only the capacity for agreement, etc.

Gotta run. I'm late for the train.

Bruce
bn@bbn.com

PS--Welcome aboard Penni! I got your paper and dissertation but haven't had time to look at them yet. I'll have questions about those domain structures and how they are distinguished from language structures.

Date: Wed Apr 29, 1992 6:36 pm PST
Subject: Re: types of rules

[From Penni] Rick Marken (920429 08:30)

thanks. your discussion has shed some light on where pct is coming from.

> I agree that it doesn't say much about the "correctee"; my point was that
> it says something about the "corrector" (in both the language case and

the social practice case). In the language case it suggests that the corrector is perceiving something about the correctee's speech, comparing it to a reference specification for what that perception should be and acting (by saying something) to try to get the perception closer to that reference.

well, we can think of some exceptions, like someone explicitly trying to teach a dialect to someone else, but i think in general an act of ``correction'' is not particularly motivated by a consideration of the language issues that may be involved. as a couple other people have pointed out, the kinds of things people correct may not match well with how they themselves speak, and may have a lot more to do with conveying status and affect and the like. there are also some things that get ``corrected'' and some that don't, and the distinction is mediated by all sorts of things. for instance, i would find it hard to imagine someone would point out to someone else that the subject of their clause didn't agree in number with the verb, even though that happens and one sometimes even notices it. but jumping on people for getting their case markings on pronouns ``wrong'' is socially sanctioned, as well as the phenomenon being a lot easier to point out. as another example, ``they'' as a singular pronoun has a venerable history, but it was decided in the 19th century to teach people that this usage is ``wrong,'' and we've all been taught this. however, this inculcation often doesn't stick very well; furthermore, there are plenty of purely social reasons for using ``they'' is a way that's supposed to be ``wrong.''

that follows and does not follow the hypothesized rule 3) watch to see whether the "corrector" (now the subject) corrects only those examples that violate the rule and makes no response to those examples that follow the rule; if you can predict the subject's response to EVERY language example, then you probably have guessed the controlled variable. If there is even 1 deviation from prediction then 4) revise the hypothesis about the controlled variable (taking into account the subject's previous corrections)
and go to (2).

is that really how one tests hypotheses in pct? it would seem that the hypothesis would have to be enormously long and complex to cover every contingency; conversely, it seems that you cannot transfer your predictions out of the test setting, since you will have tailored them so much to it.

--penni

Date: Wed Apr 29, 1992 7:40 pm PST
Subject: Belief systems

[From Bill Powers (920429.0900)]

The religious thing seems to be coming up again, with the usual sniping between the True Believers and the Unbelievers. It's obvious that the Unbelievers are not suddenly going to be converted to Control Theory for Christ, and that the True Believers are not going to switch from BEING believers to STUDYING believers. I don't think that railing against a belief is going to advance PCT much, nor is blindly defending any particular belief going to win the day. Perhaps what we might more profitably do is examine belief as a phenomenon.

Belief is a phenomenon worth studying, quite aside from what is believed. What is most interesting is not just a single belief -- there will be a sunrise tomorrow -- but a SYSTEM of belief. A single belief is usually defended for rather simple reasons: it's hard to find an alternative. But a system of beliefs is an elaborate thing that has the power to take over the mind and shape every aspect of experience to fit it -- perceptions, goals, and actions.

In Perceptual Control Theory (PCT), and even more in Hierarchical Perceptual Control Theory (HPCT), we attempt to build up a concept of how individual human systems work. In trying to learn and improve this theoretical system, we have all come up against our own beliefs; those who have spent years in conventional disciplines have often found their private confrontations of the new with the old unsettling, painful, and even costly.

It seems that simply growing up in a normal educational system, devoting oneself to study, learning what others have found, and meeting the demands of one's mentors is enough to allow systems of belief -- or of unbelief -- to get a grip that is hard to loosen. Consider the biologist's resistance to the concept of inner purpose. When children who are to become biologists do things on purpose, they take their own intentions, hopes, wishes, and goals for granted: the main problem is how to satisfy them. But put them through the series of educational courses that produces professional biologists, and they come out of it knowing in their hearts that organisms are just biochemical mechanisms with no purposes at all but survival to the age of reproduction. And not only do they "know" this, they BELIEVE it. To say they believe it means that they now consider their beliefs to be self-evident aspects of the world -- not beliefs, but facts. They consider it their duty to inform the world of this truth, to reinterpret the descriptions offered by the misinformed so they properly acknowledge the purposelessness of life, and to deal with other people and more particularly animals as if they had no inner goals of their own. And of course they conscientiously interpret their own experiences so they fit the belief that purpose is an outmoded illusion -- in their speech, as least, if not in their actions.

This phenomenon of belief isn't confined to biology. People arrive at firmly fixed belief systems about electron flow, quarks, continental drift, natural selection, grammar, etiquette, construction practices, and proper forms of music, art, poetry, and dancing. If you challenge their beliefs they will defend them. In most cases having to do with less material beliefs, the ultimate defense is "I was raised to think that ...". and of course that is true, although it doesn't make the belief true.

Repudiating or even examining beliefs or unbeliefs is as much a social as a personal problem. To examine a belief or unbelief closely is already to devalue it slightly. To doubt it is to doubt all the circumstances that led one to adopt it in the first place. It is to question people whom one has admired, respected, submitted to, and loved. In effect, it is to see the truthtellers of one's formative years as liars, although of course they were telling what they believed to be the truth.

To question beliefs or unbeliefs is also to question the reasons for which one adopted, or once and for all rejected, a belief. A belief in the ability of one person to control another is not just an article of faith adopted because of love for the teacher, or rejected because the teacher was unpleasant. Believing in the ability to control others suggests all kinds of interesting possibilities if one sees the chance of becoming one of the controllers, and all kind of horrifying possibilities if it looks as though one will be among the controlled. Beliefs are adopted or denied in part because of what they imply about one's ability to achieve other goals. They are, or at least certain details of them are, expedient in furthering one's own interests.

And finally (although not exhaustively), belief systems are intertwined with one's self-esteem. A scientist who believes in science above all doesn't hold this as an abstract belief. Along with it goes the consciousness that I AM A SCIENTIST. Science is the best of all possible approaches to life, and being a scientist is the best of all possible ways to be. And of course those who reject science and choose some other belief system feel that they are among those living some other best-of-all-possible lives, while scientists are either neutral or the worst of all possible people.

The most serious conflicts that take place between people, and the most difficult to resolve, are those that originate at the highest levels of organization. It is not systematic belief per se, nor systematic unbelief, that produces the conflict, but the inability to step back and re-examine a belief when it is confronted by a contrary one. If the Jews and the Moslems come into conflict over their divine destinies, the productive thing for the Jew to do would be to say "Wait a minute -- my beliefs say that this land is historically mine, and you seem to believe it isn't, or that it's yours just as much as mine. How strange -- these beliefs can't both be true. What's going on here?"

Of course that isn't what happens, because to most people a fundamental system of belief is to be defended, not examined. The defense, however, guarantees conflict to the limits of brutality.

At the level of systematic belief, both principles and reasoning become subservient to preservation of the belief system. When you look at the arguments against purposiveness in behavior that were advanced -- and thought rather clever -- in the early parts of this century, you find elementary logical errors and straw-man arguments that wouldn't convince a schoolchild if the subject were something else. You find abandonment of principles of scientific detachment and objective argument in favor of emotional attacks and innuendo. The belief system justifies these alternative uses of principle and reason, because above all, the belief has to remain true. When you are defending something that is above logic and principle, logic and principle must be bent to the higher purpose.

I count belief and unbelief together as system concepts. There is nothing inherently wrong with either -- if there were, we wouldn't have evolved the capacity to form beliefs or unbeliefs. What goes wrong at this level of organization is loss of the ability to alter the organization of one's belief systems to achieve harmony among all the different belief systems necessary to a complete life -- different belief systems inside oneself, and different belief systems among different people. I have not identified yet a higher level of organization than system concepts, but this may be entirely due to the fact that the currently-highest level of consciousness is never itself an object of awareness; one must occupy a higher viewpoint to see that level as a level, an object of awareness and a subject for potential modification. Even to speak of belief systems as belief systems rather than as truths implies, intellectually, that one is looking from a higher-level viewpoint. But there reason speaks; if there is no still higher level to which one can retreat, as there evidently isn't for me, the viewpoint can only be experienced as a ghostly sense of something just outside the range of peripheral vision that eludes the attempt to see it directly.

As I believe on all the evidence that I am not unique, I can only recommend that others who want to see belief systems as objects of study try to see them that way, thus occupying if not being able to describe this viewpoint from which one sees belief systems without identifying with them. To see them this way is not to accept or reject them, or to make them seem less than what they are. It is only to see them FOR what they are.

Best to all   Bill P.

Date: Wed Apr 29, 1992 9:00 pm PST
Subject: Controlling Perception, The Test

[From Rick Marken (920429 20:00)]

Well, I hope someone can explain the brilliant legal reasoning that led to the shift of venue of the King trial to a place where the jury was bound to be all white. My guess -- riots by 21:00.

But first --

Penni Sibun (920429):

Welcome to the dialog. I think there is a LOT of background material you might want to go over to understand the phenomenon and the model that we are dealing with (PCT stands for perceptual control theory, by the way, but you seem to know that).

You say:

>well, we can think of some exceptions, like someone explicitly trying
>to teach a dialect to someone else, but i think in general an act of
>``correction'' is not particularly motivated by a consideration of the
>language issues that may be involved.

People control LOTS of variables. This is why it might be nice to look up some of the background on PCT. Controlled variables are ALL the perceptual experiences that people control; Powers categorizes these in terms of eleven (or so) possible classes of variable - intensities, sensations, transitions, configurations, events, relationships, sequences, programs, categories, principles, and system concepts. People control all these types of variables all the time (grammar is just a subset of the program variables that people could control). So language is part of the control model (because people do talk -- boy, do they) but not a big part. Even a talkie person like myself spends most of the time controlling lots of stuff besides sequences of words.

>the kinds of things people correct may not match well
>with how they themselves speak, and may have a lot more to do with
>conveying status and affect and the like.

Yes, "status" is a word that stands for a relationship perception that can be (and seems to be) controlled by many people, indeed. But remember, "status" is just a word -- PCT wants to know the perception that is actually being controlled. We PCTers try to get past the words to what the words represent -- perceptual experience. It seems that you are doing something like this in your model of text generation -- but letting the database (the words describing the house, for example) stand in for perception. That might be a good way to go in many types of modeling.

>is that really how one tests hypotheses in pct?

Pretty close -- there are earlier posts to CSG-L that give a far more rigorous description of the method. I could repost one of my favorites (from Bill Powers) if you are interested.

>it would seem that
>the hypothesis would have to be enormously long and complex to cover
>every contingency;

Not necessarily, though certain verbal descriptions of it might seem so. According to PCT, what is controlled is perceptions -- experiences; what you see when you open your eyes. Some of these experiences take a lot of words to describe -- like the perceived love that exists between a man and a womao+cn. Poets have worked on this for years but even little kids know what perception(s) they are controlling for. It's just tough for an observer to know what these perceptions are -- so we have to use this test. But usually, the perceptions controlled can be described quite easily -- often with a simple equation (for lower level perceptions). r3{}3eE

The "coin game" is a good way to learn PCT methodology. Put 5 coins in front of the subject who is asked to "arrange the coins in any pattern". Your job (as experimenter) is to guess the pattern that the subject has in mind. You do this by moving coins -- when the pattern is disturbed the subject should say "no" indicating that you cannot make that move and preserve the pattern. If the pattern is not disturbed the subject should say nothing. You will find that, indeed, it is not easy to determine what pattern the subject wants to perceive. But when you do discover it, you may find that your final hypothesis (which might be "nickle over one penny and to the left of the other") could be expressed more easily as "right triangle".

>conversely, it seems that you cannot transfer your
>predictions out of the test setting, since you will have tailored them
>so much to it.

All PCT wants to explain is what perceptual variable a person is controlling

"right now". I know that people are always changing the variables they control. All that the PCT method shows is that the person CAN control a particular variable -- because they were controlling it. PCTers think of the control of perception as something people are ALWAYS doing, whether they are in a lab of in a downtown bar.
Regards

Rick

Date: Thu Apr 30, 1992 7:35 am PST
Subject: control and influence

[From: Bruce Nevin (Thu 920430 09:44:14)]

(Rick Marken (920424 08:30)) --

>Hank Folson (920423) says:

>PCT is a hard sell.
. . .
>There is no escaping the fact that when the big guy created
>life he placed us squarely in the middle of a frustrating
>paradox -- we live by controlling but we cannot control
>what is living. As you said, because we are control systems,
>we cannot be controlled; and because we are control systems
>we cannot help trying to control.

This is related to Job's paradox. As paraphrased in Archibald MacLeish's verse play JB:

> If God is God, he is not good;
> If God is good, he is not God.

Any superior intelligence, be it God or visitors in UFOs, cannot control humans and humanity, they can only influence. Bang! Right away there goes occasion for fear of God or of any truly superior intelligence.

The principle method of influence is by suggestion. An important form of suggestion, whether explicitly in hypnosis or otherwise, is by nonverbal example. As Albert Schweizer said:

> There are three ways to teach a child.
> The first one is by example.
> The second is by example.
> And the third is by example.

A possible PCT paraphrase of a famous prayer (attributed to John Goldmouth [Chrisostomos] in the Book of Common Prayer, to some other saint by the 12-step groups that have taken it up):

> Let me have the reference perceptions for controlling what I can control, for not trying to control what I can at most influence, and for discerning the difference.

Another very important form of influence is by presuppositions riding stowaway on agreements reached by more overt means, such as use of language. Sales techniques depend on this. So does socially institutionalized prejudice. So do most social conventions; only a small, visible minority of social conventions are normally available for conscious attention, those shibboleths that are overtly enforced, like the ones Rick and Penni have been discussing.

Important among these conventions are those out of which we weave the fabric of personality and self-image. I described some time back research done in which a few practiced speakers recorded the same text repeatedly, varying parameters of delivery and voice quality such as nasality, pitch variation, speed, orotundity, etc. Subjects evaluated these on graded scales for polar adjectives like fat/thin, honest/dishonest, intelligent/unintelligent. All subjects perceived them as different people, and there was near unanimity in the judgements of the personality attributes of those imagined people.

It is my belief that humans unconsciously control for such variables in constructing a self image for presentation to others. Certainly this must be so for choice of linguistic dialect; it appears to be so for a very great deal more. We unconsciously control our behavioral outputs in ways that are consistently interpreted by others. Some of this is social convention (how much of the upper teeth and even the upper gum is exposed in a smile in parts of the midwest as opposed to elsewhere--Birdwhistell); some of it is probably biologically innate (smiling when pleased, as a family of gestures encompassing a range of such details). The forced "toothpaste" smile of a model in some ads reads false and may register pain and anger. The Madison Avenue appeal may then actually be to mysogyny, coercion of women--whether the ad people know it or not.

David described a case involving a man in conflict about adultery with the babysitter. The occasion of pleasure was apparently revenge for being embarrassed by his wife. There was some suggestion that he may have experienced more control with the babysitter. But he had to be seduced by the babysitter, as he was seduced by his wife. My question: how did they know he needed to be seduced? I propose that we unconsciously advertise such things. We drop bait in the water, and keep a watchful (but not consciously acknowledged) eye out for nibbles. We do this by deliberate ambiguity. There is a socially sanctioned interpretation of the interaction that is admitted to awareness. The other levels of meaning are available for awareness, but we choose to ignore them.

I propose that this is the real function of patterns such as those Eric Berne and his students describe (games people play, games alcoholics play, scripts people live, etc.). They're not just to reduce anxiety by structuring time, as Berne suggests. They're auditions, means of trying one another out for roles in unresolved psychodramas.

They're also opportunities for influence, because they are marvellously suited for re-framing at various levels. I suspect that any competent and experienced therapist does just this at least sometimes.

When I was at Penn in the '60s, I heard a story about someone coming across the Walt Whitman bridge and paying for the car behind. Maybe the car following him got sidetracked to a different lane and the driver paying extra didn't notice, or maybe something more was intended. The toll taker may have started it out of pocket on a whim. In any case, the next driver who came up holding out his fare, on being told "they guy in front paid for you," said "what the heck, I'll pay for the guy behind me." This reportedly went on for several hours before someone put his money back in his pocket. This may be an urban legend (though I have not heard it since).

So here's something about how influence can happen. Now if you were an ESPish sort of God or angel or UFO rider and wanted to help humans shape up, maybe you would look for someone easy to influence. Not someone so suggestible as to have come to grief and wreckage in life, it has to be someone in a position to influence others. For example, here's someone who spends time constructively daydreaming, looking for ideas. A writer. Writes and edits from time to time for Whole Earth Review.

OK. Here's the story about Anne Herbert's idea. I transcribed it from a much-photocopied single sheet (small type) that my wife handed to me, asking me to make copies for her students.

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Practice Random Kindness and Senseless Acts of Beauty

It's a crisp winter day in San Francisco. A woman in a red Honda, Christmas presents piled in the back, drives up to the Bay Bridge tollbooth. "I'm paying for myself, and for the six cars behind me," she says with a smile, handing over seven commuter tickets.

One after another, the next six drivers arrive at the tollbooth, dollars in hand, only to be told, "some lady up ahead already paid your fare. Have a nice day."

The woman in the Honda, it turned out, had read something on an index card taped to a friend's refrigerator: "Practice random kindness and senseless acts of beauty." The phrase seemed to leap out at her, and she copied it down.

Judy Foreman spotted the same phrase spray-painted on a warehouse wall a hundred miles from her home. When it stayed on her mind for days, she gave up and drove all the way back to copy it down. "I thought it was incredibly beautiful," she said explaining why she's taken to writing it at the bottom of all her letters, "like a message from above."

Her husband, Frank, liked the phrase so much that he put it up on the wall for his seventh graders, one of whom was the daughter of a local columnist. The columnist put it in the paper, admitting that though she liked it, she didn't know where it came from or what it really meant.

Two days later, she heard from Anne Herbert. Tall, blonde, and forty, Herbert lives in Marin, one of the country's ten richest counties, where she house-sits, takes odd jobs, and gets by. It was in a Sausalito restaurant that Herbert jotted the phrase down on a paper place mat, after turning it around in her mind for days.

"That's wonderful!" a man sitting nearby said, and copied it down carefully on his own placemat.

"Here's the idea," Herbert says. "Anything you think there should be more of, do it randomly."

Her own fantasies include: (1) breaking into depressing-looking schools to paint the classrooms, (2) leaving hot meals on kitchen tables in the poor parts of town, (3) slipping money into a proud old woman's purse. Says Herbert, "kindness can build on itself as much as violence can." Now the phrase is spreading, on bumper stickers, on walls, at the bottom of letters and business cards. And as it spreads, so does a vision of guerrilla goodness.

In Portland, Oregon, a man might plunk a coin into a stranger's meter just in time. In Patterson, New Jersey, a dozen people with pails and mops and tulip bulbs might descend on a run-down house and clean it from top to bottom while the frail elderly owners look on, dazed and smiling. In Chicago, a teenage boy may be shoveling off the driveway when the impulse strikes. What the hell, nobody's looking, he thinks, and shovels the neighbor's driveway, too.

It's positive anarchy, disorder, a sweet disturbance. A woman in Boston writes "Merry Christmas!" to the tellers on the back of her checks. A man in St. Louis, whose car has just been rear-ended by a young woman, waves her away, saying, "It's a scratch. Don't worry."

Senseless acts of beauty spread: A man plants daffodils along the roadway, his shirt billowing in the breeze from passing cars. In Seattle, a man appoints himself a one man vigilante sanitation service and roams the concrete hills collecting litter in a supermarket cart. In Atlanta, a man scrubs graffiti from a green park bench.

They say you can't smile without cheering yourself up a little--likewise, you can't commit a random act of kindness without feeling as if your own troubles have been lightened if only because the world has become a slightly better place.

And you can't be a recipient without feeling a shock, a pleasant jolt. If you were one of those rush-hour drivers who found your bridge fare paid, who knows what you might have been inspired to do for someone else later? Wave someone on in the intersection? Smile at a tired clerk? Or something larger, greater? Like all revolutions, guerrilla goodness begins slowly, with a single act. Let it be yours.

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Bruce
bn@bbn.com

Date: Thu Apr 30, 1992 9:17 am PST
Subject: belief under a microscope

[from Joel Judd]

re: Bill (920429)

>It's obvious that the unbelievers are not suddenly going to be converted to
Control Theory for >Christ, and that the True Believers are not going to switch
from BEING believers to STUDYING >believers.

In my case, I agree with the first clause but not the second. I am interested in BELIEVING, whic is why I tried to formulate the convergence/divergence question ABOUT belief, and framed it in terms of my own experience. The go-around last spring made it clear why someone is not going to suddenly switch belief systems, and that's fine. But that polemic ended with the call to be more "scientific" and find ways of understanding CONCEPTS. Regardless of the belief, there should be characteristics of control of CONCEPTS which can be examined just as control of other perceptions. I think Greg's post shows one way to start.

>...a system of beliefs is an elaborate thing that has the power to take over
the mind and shape every >aspect of experience to fit it...In trying to learn
and improve...we have all come up against our own >beliefs...

All the more reason to understand them. Would it be fair to say that even such objective topics as PCT are understood according to these belief systems? Obviously, I've been trying to fit it into mine, and judging from the comments, so have/are others.

I think what I would find useful is the development of an efficient way to get a handle on one's beliefs and their influence on one's actions--a sort of placement test, if you will. The method of levels has been discussed previously as a way of getting at higher level goals, or at least as far as one can recognize and verbalize them. What about going the "other way"? Supposing that one's belief about the nature and purpose of language is X, Y and Z, how do I begin to be aware of how that system influences linguistic principles and the syntactic quirks I control for and so on in a way that can be useful, both as a potential teacher or learner? Ed Ford has explained several times how he uses a proceedure to help people become aware of what they're controlling for and how this helps empower them to improve important relationships and resolve conflict. I am thinking that learning some things requires even more detail in terms of the perceptual hierarchy--another language, or adult literacy--there's a lot involved in making such changes in one's life. Obviously, such changes can be made. But how might we go about explaining such change in more detail?

Rick (920429),

Congratulations! Does City Hall need a PCT analyst? You were a little bit off, though. CNN was showing burning buildings and rioters by about 1900 hours, I think.

Date: Thu Apr 30, 1992 11:10 am PST
Subject: linguistic rules, correction

[Avery Andrews (920430.1053)]

One possibility about linguistic corrections is that most of the time the correctors aren't interested in actually controlling how other people talk, but in getting themselves perceived as superior in some respect (e.g., the fact that I use fewer/less properly and you don't shows that I'm more numerate than you are, or something like that).

What's suspicious about correction is that for the most part language seems to be learned without it (though some child language researchers I've talked to suspect that correction of children by slightly older ones, delivered of course in the most soul-destroying manner attainable, may be more significant than orthodoxy would have it). So as far as linguistics is concerned, it's a pretty marginal phenomenon.

What I'd currently conjecture to be at the bottom of language-learning is a control system that fiddles with the grammar so as to satisfy the goal:

  people are saying what I might say (under the circumstances)

So if you expect somebody to say X, but they actually say Y, there is a bit of evidence that X is not the right thing to say under the circumstances, and the grammar is modified so as to inhbit X and facilate Y (sort of like back-propagationk, but in a much more structured environement).

A system working along these lines would not necessarily pay any attention at all to what a person was told they ought to say--it might not even be able to process that kind of information at all.

Date: Thu Apr 30, 1992 11:12 am PST
Subject: correction to linguistic rules, correction

[Avery Andrews (920430.1055)]
Correction to my formulation of the language-learning goal:

  people are saying what I would say to mean what they mean

Date: Thu Apr 30, 1992 11:17 am PST
Subject: incommunicable convergence

(Greg Williams (920428)) --

I'm interested in what you come up with in your research.

>The Westerners tend to use theological language and
>talk about "their" desires being replaced by "God's" (or, in some sense, "the
>whole universe's") desire, while the Easterners tend to just say that "their"
>desires simply went away.

If you look again, I think you will find that in the east they talk about a cessation of *attachment* to results coming out as desired. There may be some problems with translations with Judaeo-Christian conceptions about desire. Consider how the theosophists got themselves all confused about the idea that they should "kill out desire" and "subjugate the desire nature." If all desire ceased, life would cease. Put it this way: Having no desire is having no reference perceptions.

(Maybe having zero gain in all control loops amounts to the same thing?)

Having no _attachment_to_results_ means that after experiencing an error you proceed with the trial and error process of living without getting upset about the error. It's the emotional reactiveness that goes away. As Ken Keyes puts it, in his very successful Humanistic Psychology repackaging of basic Buddhism, instead of the current particular addictive demand drowning out all alternatives, life becomes "a parade of preferences." It's true, some specific desires (reference perceptions) might go away, generally do. The meditation process does seem to dissolve "karmic" knots in the psyche, traces of experience in memory that appear to me to be something like a self-sustaining loop compounded of memory and emotional reactiveness to the memory. But you do go on living, you do go on chopping wood and hauling water, as the Zen story puts it.

The absence of self is logically equivalent to there being but one self, of which each person is a center of expression. The absence of will is logically equivalent to there being but one Will, of which each person is a center of expression. The former is the characteristically Eastern way of putting the matter, the latter the Western way. In both cases, what is lost is the illusion of separateness of the ego, a very fundamental nexus of beliefs and disbeliefs indeed, and one that we work very hard to sustain. Why go to all that trouble to grow an ego? When you're working on a project, you concentrate on it, that is, you exclude what is irrelevant for purposes at hand. You take a point of view, a perspective. By excluding and ignoring extranea, you make of yourself a center of expression for a particular reference perception. That is how a process of creation works.

The Qabalistic view is that all such creative processes follow the general outline laid out (metaphorically) in Genesis. Or in symbolic representations like the Tree of Life (Otz Chayim) and teachings about them. But that's just one traditional lineage of instruction among many.

>If there is convergence at the highest level, the
>way the experience of that state informs the rest of one's life seems to be
>highly dependent on the non-mystical aspects of one's religion.

Well, it's a truism now that "they" say "those who know don't speak, and those who speak don't know." That's not quite all. It's more like, those who know aren't telling you what they know even when they speak as plainly and carefully as they can, with as good as possible a will to communicate to you, unless you also have had at least some measure of the kinds of experience of which they speak. The alchemists said repeatedly that they laid their secrets out in plain view. (They also said that their secrets were recondite and most cleverly hidden, which is the same thing. By far the cleverest and most recondite place to hide a thing is in plain view.) Jesus said, let those hear who have ears to hear, and cautioned about pearls before swine. The Buddha was quite clear about how to handle different degrees of readiness. And so on.

All language is metaphorical. We just don't notice the metaphors most of the time. Metaphor is in the essential nature of control systems correlating words with experiences. It is in the essential nature of control systems correlating their experiences with one another by means of words. The uniformitarian hypothesis (or presumption!) is a *necessary* license for metaphor. You simply cannot proceed without it. When someone uses language to describe experiences beyond the knowledge of their audience, the audience notices the metaphors and complains about them. Of course that's why religious literature abounds with metaphors, parables, and teaching stories.

So the preferred method of teaching, East and West, seems to be to indicate directions to look, suggest how to go about looking, try to set up experiential conditions and if necessary trick the student into falling out of preconceptions into something new and unexpected. And never to teach about something in the learning situation, verbally, that the student can work out for herself. Because then the codification in words, with its metaphorical bridges to established belief-disbelief systems, gets in the way of the experience.

(Sound familiar, Bill?)

Bruce Nevin
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Date: Thu Apr 30, 1992 11:38 am PST
Subject: Pilots vs. Engineers

[from Gary Cziko 920430.1155]

In my research for the book I am writing, I have been reading:

Vincenti, Walter G. (1990). _What engineers know and how they know it: Analytical studies from aeronautical history_. Baltimore: The Johns Hopkins University Press.

For my present purposes, I find very useful Vincenti's adaptation of Donald Campbell's notion of "blind variation and selective retention" to explain technological progress. But I thought that CSGnetters would appreciate the following quote taken from page 76:

"A pilot and aircraft, taken together, form a single dynamic system, with feedback loops to the pilot via the feel of the cockpit controls plus cues from instruments and from vehicle orientation and acceleration. The pilot is a dynamic part of this _closed-loop system_ (to use modern control terminology) and senses him- or herself as such. The engineer, on the other hand, views the system from outside and tends to focus on the airplane, the part of that can be designed. Engineers of the 1930s, as a result, tended to see the airplane as an _open-loop system_--though they didn't use the term--with the pilot as an external agent who supplied whatever more or less quasi-static actions were required. (The preoccupation with moment-curve slope for specification of longitudinal stability refleced this view.) The difference was one of viewpoint rather than pilot-aircraft reality; it gave (and still gives) rise to subtle and troublesome differences between pilots and engineers, not only in how problems are defined and solutions attempted, but in psychology and language as well." (p. 76).

As CSGnetters know, the trouble is not limited to differences between pilots and engineers.--Gary

Date: Thu Apr 30, 1992 11:53 am PST
Subject: systems concepts and stuff

from Ed Ford (920430.11:44)

To all linguists:

My father was a Phi Beta Kappa(sp), first in his class at Yale University, majored in Latin and English, ended up with a law degree from Harvard, and was, for the last 20 years of his life, a judge. He had a great sense of humor and kept a running tab on the worst language used in court, both by a lawyer and witness. For the witness it was "Me and Eddie, we was rose down to the ridge together." For the lawyer it was "Them bottles of wine, they was drunk between who?"

from Powers (920429)

>...I can only recommend that others who want to see belief systems as
>objects of study try to see them that way......To see them this way is
>not to accept or reject them, or to make them seem less than what they
>are. It is only to see them FOR what they are.

The problem for me is that to be properly studied, understood, and fully tested, a belief system has to be checked out through experience. As a Roman Catholic, I have found great internal satisfaction over the years from the standards I've set and decisions I've made which have flowed from my religious beliefs. I know others who have left my church and established other beliefs. Some have found satisfaction in their lives, some have not. I think the standards I've set based on my systems concept, the choices I've have made which reflected those standards, and, most important of all, the satisfaction that comes from achieving the various things for which I have controlled are the real test of a systems of belief. It is pretty hard to see this system as an object of belief if, in order to validate it, you have to live it to test it. I think a valid test of any systems concept is this: Does it respect the rights and beliefs of other living control systems? Is it enough to judge a system of beliefs just by how others live it or by what it claims.

Perceptual Control Theory is a good example. Much of the understanding I have of PCT comes from my application of it within my own life, through my dealings with others, and through the success others have made of their lives through their understanding and application of PCT. It has given others a whole new way of looking at their fellow human beings and of respecting the worlds they know little about. I had to immerse myself in the concept and actively live it to really understand it.

Finally, we all have a belief system. It would be hard for my own view or systems of beliefs not to get in the way of those systems I'm trying to study. To me, the real test is when it is given a try, when the rubber hits the road. I guess it's the same as when many of you create a model of what you are thinking. A model for me is when people with whom I work attempt to find satisfaction by using a particular systems concept AND, through using this system, they are able to deal successfully with conflict. When people are functioning effectively, then what ever they're using to drive their system should be given respect.

To All CSGneters:

For those who want back issues of Closed Loop, they must be purchased. The first four issues cost $3 per issue. The last two issues cost $5 per issue. Membership in the Control Systems Group is $40 per year. Send all requests for back issues and membership fee to Mary Powers, 73 Ridge Road, CR 510, Durango, CO 81301. Make checks payable to: Control Systems Group.

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Date: Thu Apr 30, 1992 1:59 pm PST
Subject: correcting

[From Rick Marken (920430 13:20)]

Avery Andrews (920430.1053) says:

>What's suspicious about correction is that for the most part language
>seems to be learned without it

> So as far as
>linguistics is concerned, it's a pretty marginal phenomenon.

I agree that correcting is rare. I don't claim that it is an important linguistic phenomenon. But the fact that it happens AT ALL is evidence that people do (or, at least, can) control "linguistic" type variables. People must be controlling many variables at the same time when they speak (or type). It is difficult to study these variables by introducing disturbances to the language generated by a speaker. But we can study some of the variables that people might control as they speak by asking whether utterances generated by others (such as the experimenter) are ok. I think linguists actually do this kind of research, right? They give an "informant" a sample of language and ask if the sample is ok in some sense. This is an informal version of "the test"; usually the hypothesized controlled variable is a syntactic structure -- a rule. But there are other variables that could be controlled aspects of the sample -- the pronunciation (sound configuration), intonation (sound event), semantics (relationship of sound events to other perceptions), appropriateness (whether usage violates a principle -- like using the familair "you" to address a stranger), and such.

So I don't think "correcting" is an important aspect of language or social interaction. But the fact that it happens suggests that people have references for the way they want particular perceptions to be -- and if they themselves can influence that aspect of their own perceptions, then they might be able to control that perception. ("Correcting" does not mean that we CAN control something; I think the rioting going on in my city should not be happening -- my perception of the situation differs considerably from my reference for it -- but I cannot do anything to bring the situation closer to that reference level -- ie. I can't control it).

Regards Rick

Date: Thu Apr 30, 1992 2:54 pm PST
Subject: correcting

[From Avery Andrews (920439.1358)]

Rick Marken (920439 13:20)

> But the fact that it [correction] happens AT ALL
>is evidence that people do (or, at least, can) control "linguistic"
>type variables. People must be controlling many variables at the
>same time when they speak (or type).

But it doesn't follow that the processes whereby this kind of conscious `corrective-control' is achieved have anything much to do with normal language-use. On the basis of other's and my own experience of the relationship between interactive exposure to a language and progress in acquisition, I'd tend to conjectyure that they have almost nothing to do with each other.

Where grammaticality judgements come from is an interesting question. I suspect that they are a side effect of the operation of machinery whose purpose is to reduce the range of available meanings of an utterance, so that it will be easier to figure out which of the remaining possibilities is the intended one. The grammar for example tells us to group a demonstrative with the following rather than the preceeding nominal material, so we know what `that' is modifying in:

John gave the student that article

but it follows from this principle that

John gave the student article that

gets no meaning (as long as the constraints of the grammar are obeyed; we can then get a possible interpretation by relaxing them, but this won't work very well for complicated sentences, as people who try to read instruction manuals written in Japan sometimes get to observe).

So the `ungrammaticality' that is the major focus of work in generative grammar would be a side-effect, albeit a very instructive one. The more central idea is the range of things an utterance can `mean-in-accordance-with-the-grammar'. So in speech production you would be controlling for `saying something that means what you want to be believed to be meaning' (this rather circuitous formulation has been engineered to deal with lying, though I'm not sure if it adequately covers storytelling & artistic fiction).

An important contribution that PCT might be able to make here is to provide a rationale for the widespread but not very well-supported assumption that the same grammar is involved in production and comprehension, the rationale being that the grammar defines the reference`-level' (I think `reference situation-type' would be a better term, since it's not real clear how to represent these things as neural currents) that production is organized to match.

Avery.Andrews@anu.edu.au
(currently andrews@csli.stanford.edu)

Date: Thu Apr 30, 1992 4:58 pm PST
Subject: Re: Mystics' reports

From Greg Williams (920430)

>Bruce Nevin on mystical metaphors

>If you look again, I think you will find that in the east they talk
>about a cessation of *attachment* to results coming out as desired.

That comes AFTER (or BETWEEN) the ecstatic experiences. IN the ecstasy itself (as reported) is "no-desire." Again, this is to be expected if the attention (whatever that might be) is focused "above" the PCT hierarchy, where there is no reference level set by any higher level. The non-attachment, or as I have learned to say after observing some Zen practitioners first-hand, "noncompulsive nitpicking" (striving to succeed, but not being frustrated by failure), certainly IS found in the NON-ecstatic parts of the lives of many mystics.

>Put it this way: Having no desire is having no reference perceptions.

How about this way: DURING ecstasy, one is AWARE OF no reference signals, whether they are met or not -- perhaps because awareness is focused above all reference signals. Certainly, homeostasis goes on, even during ecstasy! (Whoops, that's my prejudice -- some mystics speak of God's kiss of (temporary) death, and the world being destroyed, and so forth...)

>those who know aren't telling you what they know even when they speak as
>plainly and carefully as they can, with as good as possible a will to
>communicate to you, unless you also have had at least some measure of
>the kinds of experience of which they speak.

Of course, verbal descriptions of feelings of ANY sorts are limited in obvious ways which are partly remedied by the listener's experience of (presumably) similar feelings. One way to partly get around this problem is to relate the feelings to OTHER feelings which the listener has had. So the data-taker asks, e.g., did it feel like having no desire? like pain (of what sort)? like making love? like being humbled? And then the data-taker proceeds to flesh out the analogy: "no desire" like when you're satisfied? like when you're asleep? like when you've given up? Etc. And the analogies -- the metaphors -- begin to inform -- suggest! -- to some degree.

Greg

Date: Thu Apr 30, 1992 8:44 pm PST
Subject: a new subscriber's self-introduction

Hello! friends:

(This letter may be "buggy" & vague.Be patient to read it,please)

My name is Yi P. Huang,a new subscriber coming from Taiwan in Asia. Before joinning this group,I thought it as a completely academic discussion.Now I find "CSGnet" is what I really wish.

I major in control engineering at Chiao-Tung university,but spend a lot of time studying social science and literature(John Steinbeck is one of my favorite writer).

I have ever lead a "stundent community-servise" team to help poor kids & been an encounter-group leader for children,and now the chief editor of graduate reminiscence in my school.

I am to graduate from college this year & attend a military service shortly.After two years' compulsory army life,I intend a ME & Ph.D. program in America.

I decide to keep on studying control theory & utilize it to other disciplines.Either way to go:

1) To continue E.E. studying & try to join control theory with other realms in my research work.
2) To study other subject,such as socialogy,psychology,history,etc., & use control theory to handle.

I hope someone can give me advices about what's my better choice or provide me some information about programs & institutes suitable for applying.

Thanks for your reading.

Yi P. Huang
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