CSG_9012

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Date: Sat, 1 Dec 90 07:24:37 CST
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: Bill Powers <FREE0536@UIUCVMD.BITNET>
Subject: Correction + quarks

Cliff: Correction --
I meant to say " I don't see how society can even know the state of my thoughts, much less CONTROL them." Of course society can affect my thoughts but: society proposes, the individual disposes. I'm perfectly free to shout "fire!" is a crowded theater -- if the consequences of doing so are acceptable to me. The external world, including society, determines how my actions will alter my perceptions (as well as introducing independent perturbations). But the state in which I want those perceptions to be is up to me and my own human nature.

Anything else? Oh, yes, quarks. While minds can explain quarks, I doubt that quarks can explain minds. I don't think that quarks, if they exist, have sufficient organization to construct or even contain an "explanation." All they can do is interact and combine. Anyway, quarks ARE an explanation created by minds. I think you're leaving out the human agent here. Quarks are a human invention intended to explain certain expensive kinds of observations. They will exist (in imagination) only until someone comes up with a better explanation.

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Date: Sun, 2 Dec 90 00:15:48 EST
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: BARKANA@DUPR.OCS.DREXEL.EDU
Subject: Some thoughts on control of cars,temperatures, and thought.

I am very busy these days (the CDC -Conference on Decision and Control- the main annual event in engineering control happens to be next week, Dec 5-7 in Honolulu, and I organize a workshop on "Simple Adaptive Control") and I am still noy easy intruding this discussion, but Bill Powers's appeal to better times when we will not fight each other about whose model is good, combined with Rick Markens's new comments on the "wrong idea that the control systems control the output" need a comment. If this is a family with specific definitions and problems, that I do not belong to, I would rather keep quiet or say "excuse me, it was nice meeting you." But if it is a control theory group, and when the thermostat is an illustration, then some things deserve, at least, clarification. I am not even sure what you mean by "sophisticated" control people. Are they mathematical psychologist, or engineering control theorists like Petar Kokotovic and the like (the group that I belong to, at large)?
The model that you all seem to use POSITION = POSITION + K*ERROR is of course, perfectly correct. However, here I am confused about what you call output, controlled object and control objective.
No matter how good or how bad is the function of the organism, or of any control system, the only thing that it can affect is the output. Even if one closes his eyes, or if one is drunk, he still controls the output, which is the name of the controlled variable in my dictionary. A closed-loop system, properly designed or properly organized, will try, in general, to minimize the signal representing the error between the measured reference signal and the measured output (controlled variable). But this is not the only way to do the job. A fresh driver, on a new route, with a new car, will perform very much of what was said above. However, a better controller uses all possible prior knowledge to get a "better" quality of control. In our case, the effect

of learning, (and here we very much try to learn from organisms) seems to become cardinal. After a while, the brain has a sufficiently good modeling of both the route and f. e., the car, and the control is much of an open loop. Based on some details of the route, the brain predicts both the changes and the rate-of-change of the route, and also the response of the car to various inputs, and pass to the car a very complex signal, mainly open-loop control. No, I do not try to advertise open-loop control! There is still much uncertainty in my model of the route and in my model of the behaviour of the car, therefore I keep my eyes open and monitor the error between the desired trajectory and the actual trajectory, but now the gain of this error can be much lower, and my mainly open-loop control is now smoother and "better" than the closed-loop only. In both cases we control the position of the car. The input is the controlling, not the controlled signal, even though in closed-loop it may be hard to tell.

Now, about the external reference. Indeed, no route can tell me where I want to go, but is a decision. Once I decided where I want to go (and, hopefully, based on the knowledge or valid "representation" of what is going on out, there) I must follow the route that exists somewhere, whether I follow it or not. This is the reference input that is measured and transmitted, after "cleaning" (filtering) it of noise and processing it so that fits the needs of the control system (the transfer function with its leads and lags, and lead-lags and nonlinearities, etc.). In the same time, The "result" (as was called in Bill Powers answer to me, why not output?) of the control, the position of the car on the route, that I call output, is measured and compared with the reference. The difference is then the controlling or one of the controlling signals that now affect the output. The objective of the control can be to cancel this error, or to minimize without requiring the use of all my vital reserves, or to minimize some combination of the error and other variables. What is wrong here? I don't use this model because it is Wiener's model, or anyone else's model, nor because it fits some complex mathematical formulas, but because that is how I understand the control systems.

If a thermostat is designed to maintain a constant temperature, than it is a regulator, and the referenced temperature can be considered as being internal, hopefully corresponding to some desired but real temperature. Good or bad, the system can only control the output, the heat in the room, either it measures it correctly or not.

Sometimes I seem to get here old names for old things, only shifted. For example, Bill Powers, what does it mean "position is represented as integral of motion?" Position is the integral of velocity. I am not surprised that in the brain, motion is hierarchically higher than position, because motion is obtained by changing positions.

Finally, the control of thought by society. Of course, it is a form of control, more appropriate to open-loop control in a free society, and to closed-loop control in a coercive society, both in general. For a long time, slavery was maintained in open-loop, because the models that the government had built about both the owners and the slaves fitted pretty much the real thing. (Sorry, I mean during the antiquity). The society (like any controller) cannot control directly the internal (state) variables, only the output -behavior and expressions- can be monitored and used for adjusting the next law, penalty or other "control signal." In a stalinist society, the loop was prety closed and almost any word or expression monitored and controlled, up to the point that almost no private thought was existent, at least apparently. You may have an independent mind in the begining, and may even hate me, the leader. But after I send your son to kill his neighbor, you are going to defend my regim, or even to love me, because your son may be punished or killed if my regim is changed.

Like any open-loop, slavery cannot observe the drift that slowly but steadily changes the output. Like any tight closed-loop based on very high gain, the stalinist society leads to self-destruction. I am not an expert in sociology, and is the first time when I try to see them as control model, and I always hope that the man is not only a machine and that man's thought will always remain free. But living in a free society, that actually does not try hard to control private thought, is no reason to

deny the existence of control system models for control of thought. Finally, even a bad control system is a control system.

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Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: Bill Powers <FREE0536@UIUCVMD.BITNET>
Subject: Outline of Model Program

For those who want to try some simple modeling (the kind we do in the CSG, that is), here's an outline. This is really just an outline of the general procedures -- Several of us can help with programs if you need them, or you can find a computer guru in any local high school.

1. Define a variable that is to be controlled. The easiest kind to present on a computer screen is a dot or a short line that is positioned using a single number. The number is the x- or y-coordinate. You have to erase the old dot (or line) by writing it with foreground color 0, the change the coordinate, then write the new dot (or line) with a visible foreground color like WHITE. You can, of course, define much more complex variables, as long as they can be varied in a single dimension. I tried one once that was a chronological list of all the U. S. Presidents, presented one name at a time. Difficult.

If appropriate, put stationary marks on the screen to define the midrange position.

2. Provide a means of control that converts a limb movement into a smoothly-varying number. A game joystick works, but you'll want to lengthen the control handle. A mouse gives better control. A crank on a potentiometer feeding a 12-bit Analogue-to-Digital converter works best because of the high resolution. I scale the handle position number so it ranges from -2048 to 2047 for all devices.

3. Generate a table of random disturbances to use during experiments. It's good to record this disturbance in an array and then play it back instead of generating it in real time, so you can compare subjects using the same disturbance, and so you can repeat the same disturbance used with a live subject when you run a model.

I use this Pascal algorithm to make smoothed disturbances (adapt for your computer language. All variables are integers):

```
d1 = 0; d2 = 0; d3 = 0; d4 := 0;

for i := 0 to ndata - 1 do
begin
 d1 := d1 + (random(10,000) - 5000 - d1) div slowfact;
 d2 := d2 + (d1 - d2) div slowfact;
 d3 := d3 + (d2 - d3) div slowfact;
 d4 := d4 + (d3 - d4) div slowfact;
 disturbance[i] := d4;
end;
```

With slowfact set to about 15 you get a reasonable amount of smoothing. By varying slowfact you can create disturbances of differing difficulty. After the table is generated, find the maximum absolute value "max", then scale the table by multiplying each entry by 1024/max. This means that the maximum disturbance corresponds to half the range of the handle position number. The multiple-stage smoothing makes the high-frequency cutoff sharper than a single stage would do.

4. To do a run, add the handle position number to the i-th disturbance table entry and use the sum to position the mark or shape on the screen.

The index i advances from 0 to ndata - 1 during the run. You will have to scale the display coordinate so that a range of 1024 corresponds to half the dimension of the screen, in dots. Use integer arithmetic -- this doesn't have to be exact.

I recommend sampling data at least 30 times per second. A run of 1 minute is sufficient for modeling, so this means ndata = 1800 when dt = 1/30 sec. The sampling rate should be a multiple of the frame rate of the display -- it does no good to sample faster than new points can be displayed. If you do this in BASIC, compile it: interpreted BASIC won't be fast enough.

You can synchronize samples to the frame rate on a PC-compatible system by using the following routine (this seems to work right, but I can't test every possibility):

```
procedure retrace;
begin
 case graphdriver of
  8,9,4: begin          {ATT, VGA/EGA, EGA64 }
           while (port[$3da] and 8) = 8 do ;
           while (port[$3da] and 8) = 0 do ;
         end;
  7,5: begin            { Hercules mono, EGA mono }
           while (port[$3ba] and $80) = 0 do ;
           while (port[$3ba] and $80) = $80 do ;
           end;
  ELSE begin            {Everything else }
         while (port[$3da] and 8) = 0 do ;
         while (port[$3da] and 8) = 8 do ;
       end;
 end;
end;
```

"Retrace" waits for start of a vertical retrace period and then returns. You can pick out just the case appropriate for your display. If you pick the wrong one the program will run too fast or else freeze (waiting for a bit to change that doesn't change).

To do a run it is nice to have a run-in period of a few seconds so you don't record the startup transient. Here is an outline of the run program:

```
cursor := 0; { provide known cursor position to erase }
ClearViewPort;  { clear screen; be sure you're in graphics mode }
DisplayReferenceMarks;  { if any }

for j := -200 to ndata - 1 do
begin
 i := abs(j);
 Handle := readhandle;  { read in the scaled handle position }
 Cursor := Handle + Disturbance[i];
 Retrace;               { wait for start of vertical retrace period }
 Retrace;               { do again if half the frame rate desired }
 DisplayCursor;         { erase the old cursor, draw the new one scaled to}
```

```
                        { fit screen}
 SubjHandle[i] := Handle;  { save just the handle position }

end;
```

The run-in period data is overwritten when j is >= 0, so you don't waste storage space.

5. The pure integral model (the simplest one):

Running the model goes exactly the same way, including the run-in period. Of course you don't have to display the cursor or wait for retraces.

```
Handle := 0;  { Initialize handle position }

for j := -200 to ndata - 1 do
begin
 i := abs(j);
 Cursor := Handle + Disturbance[i];
 Handle := Handle - round(Cursor * intfactor);
 ModHandle[i] := Handle;  { save just the handle position }
end;
```

The integration factor "intfact" is a real variable, to remove a few of the rounding errors. You run this model repeatedly, computing the RMS difference between "subjhandle" and "modhandle" each time, and adjust "intfact" until you get a minimum.

If you want the model to have a non-zero reference level "ref," the handle computation becomes

```
  handle := handle + round((ref - cursor) * intfact); { Note signs}
```

You'll get smaller RMS differences if you set ref to the average of the handle-position data for the subject. You'll HAVE to do this if the subject is maintaining the cursor at some position other than zero. Or you can vary "ref" to find the minimum RMS error.

For moderately difficult disturbances, this model's handle behavior should correlate with the subject's handle behavior better than 0.98, for a practiced subject. The RMS error should be around 5 per cent of the peak-to-peak handle excursion.

You can get a somewhat better fit by adding a "leak" to the integration:

```
 handle := handle + round((ref - cursor) * intfact - leak * handle);
```

The best "leak" will be something like 0.005. The correlation won't change much, but the RMS error will be less. This would be more meaningful if you scaled the handle and disturbance to around 10,000 max.

6. To predict behavior, generate a new disturbance pattern and run the model using the integration and leak factor found in a previous run for the same subject. Then do the live run.

AS NOTED, this is a very simple model. It isn't even nonlinear. But it predicts the behavior so well that only a fussbudget like me would bother trying to get the RMS error even smaller. The right transport lag will do

that.

Most CSGers started with this simple experiment and model. For experimental psychologists used to the kind of data you get in "normal" behavioral experiments, the experience was a revelation. It's worth the trouble to set

up this experiment and try it, even if you have to get the help of a computer nerd. Nothing could show better what we're doing.

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Date: Sun, 2 Dec 90 17:07:56 EDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: Cliff Joslyn <cjoslyn@BINGVAXU.CC.BINGHAMTON.EDU>
Subject: Re: Comments on comments
In-Reply-To: Message from "Bill Powers" of Nov 30, 90 at 11:05 pm

Bill:

> The prediction-corroboration loop is a control system.

Sounds good. I'd also call it a "cybernetic system", although I suspect that not all cybernetic systems are control systems.

> "Superficial" knowledge is knowledge gained by observing apparent causal or
> coincident relationships without any generative model of underlying
> processes. Statistical studies yield superficial knowledge. I presume
> you're not much interested in statistical studies of the effect of A on B.
> So I presume that what you're interested in isn't superficial.

I agree with the distinction between *models* which merely *describe* correlations vs. *theories* which further *explain*. A model can be a mere simulation of a phenomena, like a curve-fitting, but the criteria for explanationa are much stricter. This is the fundamental problem that e.g. so-called Artificial Life is missing. But I *am* interested in statistical correlations, since they can have a *lot* to do with the underlying theory. A good model can serve as *evidence* that a theory from which it is produced is correct. But, as you say, the goal is the theory, not the statistical description.

So if I understand your position correctly (from your earlier posting), you're saying that in order to construct an explanatory theory, and not just a statistical model, we must have a further theory about how brains work. Again, this doesn't seem compelling, since we have a whole lot of excellent theories about all kinds of things (albeit mostly physical), but precious little about the brain.

> A person who thinks that organisms are stimulus-response machines will look
> for ways in which "reactivity" or "irritability" might originate. A person
> who thinks they are control systems will look for ways in which closed-loop
> control relationships might originate. And so on. A theory of origins is
> highly dependent on what you think got originated. What model of organisms
> do you use as That Which Needs Explanation by a theory of origins?

I wish my theory was better developed, but it is not. I have found your general perspective very attractive. It is highly coincident with the body of cybernetic theory I subscribe to. As my colleagues and I develop our ideas further, I hope we will attempt to incorporate your views, and you may wish to participate with us (more on this posted to this list shortly).

Another observation which leads me to agree with your statement is derived from classical systems theory. All systems descriptions can be based on the concepts of input, output, and state. Input maps to stimulus, and output to response. In simple systems, state is null or small relative to I/O, and adequate theories can be constructed on strictly I/O grounds. But in complex systems (e.g. organisms), state

grows in significance so that simple I/O models fail, and either multi-level I/O models are necessary, or state models. In very complex systems (e.g. mammals), system state is vastly preponderant over I/O, and this includes *mental state*. The whole class of imaginations and volitional behavior arises from system state, not from I/O (this resonates with the "spite" argument against determinism: give me a prediction about my next action, and I'll do otherwise out of spite).

> In the latest post I've seen (30 Nov), it seems to me that you're using the
> term control a bit loosely.

I suspect you're right, thanks for the clarification. The schema I posted was a first approximation.

> So motion control seems to be hierarchically above
> position control. That is, to maintain a specific movement it is necessary
> to alter position continuously, but position can be maintained without
> entailing any controlled motion. The reverse is not true: in order to
> control motion, it is necessary to alter positions.

I agree, that is what my schema stated.

> If you were to read my stuff, we might save some time in finding common
> ground.

Absolutely. I regret my dissertation (on an almost totally different subject) is my first priority now. In my philosophical work, we've been focusing on technical issues for a while, but will (hopefully!) shortly begin this kind of reading which is so essential.

I must say, listening in to this list promises to continue to be helpful in that regard. Although I'm not a "true" CSG'er, I'll pitch in my $.02 if appropriate.

> A
> high reference level for a perception defines that perception as good; a
> low or zero reference level defines it as bad.

What about a neutral perception, then?

O-->
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V All the world is biscuit shaped. . .

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Date: Sun, 2 Dec 90 17:21:45 EDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: Cliff Joslyn <cjoslyn@BINGVAXU.CC.BINGHAMTON.EDU>
Subject: Re: Correction + quarks
In-Reply-To: Message from "Bill Powers" of Dec 1, 90 at 7:24 am

Hmm, I'm afraid that I may not have explained myself well.

> While minds can explain quarks, I doubt
> that quarks can explain minds.

What I mean by "explain" is that adequate theories can be constructed for one in terms of the other (note: this is not "reduction"). Obviously, in current science neither minds nor quarks can be used to explain the other. What I meant was that the classical (modern) bottom-up research program of explaining mind through physics can (and should, and is) complemented by the kind of research program it seems to me that you are pursuing.

> I don't think that quarks, if they exist,
> have sufficient organization to construct or even contain an
"explanation."

I did not mean to imply that the quarks themselves would do the explaining. It is *people* that explain things (not even minds, and certainly not physical objects like quarks or chairs). I meant that quarks would be a component of a theory which explains minds, and/or vice versa.

> Anyway, quarks ARE an explanation
> created by minds. I think you're leaving out the human agent here. Quarks
> are a human invention intended to explain certain expensive kinds of
> observations. They will exist (in imagination) only until someone comes up
> with a better explanation.

This is where I'm afraid that an anti-realist fallacy is being introduced. I do not deny that quarks are used to explain certain observations, that they are theoretical entities, and that the *theory of quarks* is a human invention. But the quarks themselves are not created by people anymore than positing a "theoretical entity" which causes plague somehow creates plague bacteria. Although quarks can be used *to explain*, whatever they are, they are not *themselves* an explanation.

The reasons we believe in the existence of quarks are not different in principle from the reasons we believe in the existence of anything. Although some entities are more theoretical than others, *all* "entities" are "theoretical" in that our belief in them is based on inference from observation. On your view, if I understand it correctly, *all* physical objects are created by minds, which leads to sollipsism.

O--->
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V All the world is biscuit shaped. . .

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Date: Sun, 2 Dec 90 20:31:41 EST
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: BARKANA@DUPR.OCS.DREXEL.EDU
Subject: Response to Bill Powers: We will close this loop!

Bill Powers -
Sorry, I was preparing my trip and I did not expect an answer today. Thank you for the compliment, but you know and I know that one must learn a lot to discover that no one is expert. In fact, I try to understand your discussion, as control theorists in psychology, because it may be very relevant to my questions in artificial learning.
I am not sure I really understand the difference between my model and yours, and may be that what you call "input" I call measured output. By the way, the temperature sensor is supposed to be in distant rooms, not in the neighborhood of the furnace. Now, if the window is open near the sensor, the temperature in this room is not affected by the thermostat system. In this case, the system receives some value of a low temperature, sends out lots of heat, the only thing it can directly control, and has no effect on what it is supposed to affect, the desired output, the temperature in the room, or on its measurement, the feedback input to the system. It does not change the fact that the input signal is used to affect the output or different stages of outputs, some of them measured if needed, to guarantee satisfactory behavior of the system.
By the way, the basic design would take into account the basic thermal

properties of the room, and nominal ambient external and internal temperatures,
and the rate of fuel burned and heat supplied would have to maintain this nominal condition "almost" with no other regulation. So, the input is the desired temperature, and the output is the actual temperature. Because I know that uncertainties are always present, I monitor the actual "controlled output"
and use the difference between the desired output (by the way, I also call it the reference input, so may be I just live in another kind of dichotomy) and the real output, and use it (after filtering the measurement noise) to generate
supplementary heat that, hopefully, will compensate not only for uncertainties,
but also for changes in the ambient temperature, and so on. I may even decide that the closed loop is enough, especially if the desired temperature is fixed.
The error between the desired and the measured output is used as input to the controller, amplified and processed, and then sent to fix or change the rate of fuel, the rate of heat, and change the temperature in the room. If the loop is well designed, it finally brings the room temperature (as it measures it) to or close to, the desired temperature. I might repeat myself, but now, suppose that the sensor is broken and frozen at a fix, low temperature. Then the control system gets some constant input and sends waves of heat, changing, I call it controlling, this output, whether this is the desired output or not.
Anyway, I think I start understanding you, and I am only afraid that may be difficult having a dialog with the control community at large, if the claim "the control system controls its input" is not understood. In spite of this argument, I think that things are not as distant as they might seem. Some more eavesdropping and more reading from my side, will straighten things even more. I am not used with such a high level of patient discussion over different opinions and I am honored to participate in it, even if I may introduce noise.
I would like to bring the discussion to the hierarchical organizations. My colleagues seem to take for granted the model that you you seem to dislike: the higher the level in the hierarchy, the lower the level of knowledge, or may be of detailed knowledge, and this is the model offered for automatic manufacturing. I am also reluctant to accept this model, although it may represent some companies, organizations, or universities, especially the large ones. But this is exactly what I detest: also I, or you, may be very important control units in this organization, we are all the time alienated to find out that the most important decisions may be taken by people that may stay somewhere up there in the hierarchy, but in fact have no knowledge or understanding about what really goes on. I do not know much about the brain, but I think it contains a lot of knowledge and modeling about what is and what should be going at the lower levels.
But now, I must go back to prepare my trip. Thank you very much for this interesting discussion, and ALOHA.

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Date: Mon, 3 Dec 90 13:29:04 +0800
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: mark@CS.UWA.OZ.AU
Subject: Re: Brooks vs Powers hierarchies

Chung-Chih Chen:

I replied to you the day after I received your article. But now I don't recall getting an ACK from the list server and I can't find a copy of my reply in my csg-l mail directory. So, here I go again...

> >From what Bill said, Bill's and Brooks' are very different.
> For example, brooks' levels are defined according to the difficulty of
> behaviors. But Bill's levels are defined according to the difficulty of
> the control functions.

It is true that the Brooks hierarchy is defined in terms of navigational competence and that the Powers hierarchy is defined in terms of data abstraction. When you say "difficulty of the control function" do you mean the control function controls a variable which is abstracted from the real world. Is an example of a `simple' control function one that controls a muscle while a `difficult' control function is one that controls an aspect of personality.

> ... In fact, I can imagine that each Brooks' level
> corresponds to the whole hierarchy of Bill. Of course, Brooks don't
> implement each of his levels using Bill's hierarchy.

I agree that each Brooks level can be implemented by a Powers hierarchy. Each of Brooks level of competence is "horizontally" decomposed into the following traditional functionality:

sensing -> modelling -> planning -> task execution -> motor control

In a Powers hierarchy "sensing" and "motor control" belong in the lower levels while "planning" should be implemented in much higher levels. Each of these fuinctions would most likely be implemented by several levels of control systems.

> Bill Powers talked about the difference between his control hierarchy
> and others. He said:
> ...
> >One last difference from other hierarchical models (like Brooks'). I've
> >tried to use neurological information as much as possible, and to define
> >
> >levels that seem possible to find in ordinary human experience. Many other
> >hierarchical approaches are more like ad-hoc inventions, organizations put
> >together to achieve some immediate purpose without the constraint of
> >achieving it the way a living system does. I'm not basically interested in
> >robotics, although it can be fun. I'm interested in how human beings and
> >other organisms work. For me, the constraint is always to figure out how
> >the real system achieves a given behavior, not just to find ANY way of
> >achieving it.

There is a fundamental difference between the approaches of Brooks and Powers. In the mid to late 80's Brooks approach was typical of what has been described as "AI simulation" or "Investigative Simulation". The intension of this type of research is to imitate the overt behaviour of biological systems by simulating broad cognitive processes such as distributed processing and memory without attempting to reproduce the actual internal structures. However, recently Brooks has headed more in the direction of "AI modeling" with his work on reflexive control architectures for insect walking control.
AI modelling is concerned more with reproducing the actual control structures which are believed to be existent in biological systems. I see Powers approach
as being a good example of this type of approach.

> I am very interested in your work. I mentioned the subsumption

> architecture of R. Brooks in CSG-L before. In fact, It's one of the research
> areas in my lab. I have thought about how to integrate Powers' and
> Brooks' works together. That's why I want to know more details.

I believe that integrating the two approaches is a very interesting idea. The approach would be to use the Brooks hierarchy of navigational competence to define the evolutionary path of the control system. That is, the behavioral development of the system can be performed by implementing the lowest level of behaviour and then incrementally building each higher level in succession. The implementation of each level would be in terms of a Powers hierarchy. This approach combines a model of how biological processes perform control (i.e. the Powers hierarchy of control systems) together with a model of the behavioural evolution of biological systems (i.e. the Brooks hierarchy of competence).

> How you integrate them together?

My own work started with the Brooks behavioral paradigm. In the last 12 months since I encountered the Powers philosophy I have been able to describe my control architecture in terms of control theory. That is, I have a distributed network of control processes that each have a reference and perceptual input, an error output, etc. I have not explicitly applied the Powers hierarchy though one of the dimension of my control architecture is a hierarchy of data abstraction that seems to parallel the Powers hierarchy. My hierarchy is defined according to the data being modeled within the system. I have found that all the control processes and data models (stored in Long Term Memory) form disjoint groups where all the processes that share models do not access data from models in other groups. The models in each group seem to contain data that is of a similar level of abstraction and hence the hierarchy. My hierarchy is as follows:

Level	Model	Description	Example
4	Precepts	Rules of behaviour	If lost then find a landmark
3	Maps	Relationships between objects	Spatial map of landmarks
2	Objects	A structure of percepts describing an object in the environment	A perception of a landmark such as a hill top or a fork in a stream
1	Percepts	A sensory pattern used to match and interpret a sensory experience	A prototype for the visual perception of a hill top
0	Reality	The real world	The physical robot (sensors, motors, etc) and the static and dynamic environment

After I submit my thesis early next year I intend to observe the Powers approach more closely. I am currently reading up on control theory so that I better understand concepts such as lag, loop gains, stability, adaptation (my main interest) and their inter-relationships.

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o Mark Nelson
< - PhD Student
/ > Computer Science Department
' ~ University of Western Australia

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Date: Mon, 3 Dec 90 09:37:30 -0600
Reply-To: "Control Systems Group List (CSG-L)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group List (CSG-L)" <CSG-L@UIUCVMD>
From: g-cziko@UIUC.EDU
Subject: Re: Education and Models

Hugh Petrie:

Thank goodness we now have someone else interested in education joining the conversation.

>THE DILEMMA OF ENQUIRY AND LEARNING, (Chicago, University of Chicago,
>1981. This is probably my most extended treatment of issues in
>education and control theory and it adds evolution as well for those
>who have that bent.

I read this a while back (before I learned about control theory) and don't remember any explicit reference to control theory. I'll have to look again. Perhaps you can tell me where to search (I use the first one or two chapters of this book for my course "Knowledge Processes"; does the title sound familiar?).

>the only way to proceed is to try to
>introduce disturbances as a teacher which, given your hypothesis
>about what the student is controlling for, may not be able to be
>counteracted by the student's control system. If not, there may be
>some reorganization and the teacher is there to suggest more adequate
>reference signals which the student may use to reduce overall error.

This is basically how I've seen education now that I have some understanding of control theory. Educators must provide disturbances which will cause students to reorganize. The problem is, there is no way to be sure what type of disturbance will work, since reorganization is blind in the same way that genetic mutation and recombination are in biological evolution. However, it seems a good bet that if the error is too large or requires to radical a reorganization, it is unlikely that this will be achieved. It is more likely that too large a disturbance (e.g., requiring 3rd graders to solve calculus problems) will simply cause a reorganization which sees math as impossible and meaningless (it seems that many children in or inner cities come up with this perception of education as a whole). However, it seems from your statement that you see reorganization as adjusting only reference levels while I believe it is far more complex than that.

I must also add that I would love to how you manage your work as a dean of a college of education when from a control theory perspective most (maybe all) of the research done by your colleagues makes no sense at all (trying to find what inputs (stimulus conditions) result in what outputs (educational outcomes). Runkel's new book makes this point very nicely and I would make it required reading for my faculty if I were dean. Have you had any luck in getting your colleagues to look at things from the control theory perspective?

>I will be criticizing the New York plan next week in testimony to the
>Board of Regents, relying on control theory, but, of course, not being
>able to mention it directly.

If we have a name for this theory that we cannot use in public, we ought to think about coming up with another name that we *can* use. Any ideas?

Something with "purpose" or "intention" in it might be good. What is intriguing about the name "control theory" is that the theory turns out to be in many ways the opposite of what one initially thinks when hearing the term. But to those with less inquisitiveness, it may be a real turn-off to wanting to learn more about the theory.--Gary

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Date: Mon, 3 Dec 90 11:39:01 EDT
Reply-To: "Control Systems Group List (CSG-L)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group List (CSG-L)" <CSG-L@UIUCVMD>
From: Cliff Joslyn <cjoslyn@BINGVAXU.CC.BINGHAMTON.EDU>
Subject: Principia Cybernetica CFP

As those who've seen my recent postings here know, the following actually is relevant to CSG-L.

O--->
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| Systems Science, SUNY Binghamton, Box 1070, Binghamton NY 13901, USA
V All the world is biscuit shaped. . .

Preliminary Announcement and Call for Papers

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* *
* 1st WORKSHOP OF THE PRINCIPIA CYBERNETICA PROJECT *
* *
* computer-supported cooperative development *
* of an evolutionary-systemic philosophy *
****

Free University of Brussels, Belgium
July 2-5, 1991

Organized by: the Principia Cybernetica Editorial Board
the Transdisciplinary Research Group

Theme

Principia Cybernetica is an attempt by a group of researchers to collaboratively build a system of cybernetic philosophy, moving towards a transdisciplinary unification of the domain of Systems Theory and Cybernetics. This philosophical system will be developed as a network, consisting of nodes or concepts, linked by specific types of semantic relations. The network will be implemented in a hybrid computer-based environment involving hypermedia, electronic mail, and electronic publishing, thus providing readers and authors with flexible access to every part of the system.

Development of this system is seen as a long-term project involving many participants, communicating and conversing through electronic media, and supervised by an Editorial Board. While traditional

publication of parts of the network will be made periodically, the project is seen as necessarily open-ended and developing, a process of discourse among a community of researchers.

The philosophy to be developed is systemic and evolutionary, emphasizing the spontaneous emergence of higher levels of organization or control through variation and natural selection. It includes: 1) a metaphysics, based on processes as ontological primitives, 2) an epistemology, which understands knowledge as constructed by the subject, but undergoing selection by the environment; 3) an ethics, with the continuance of the process of evolution as supreme value.

Workshop Topics

* Supporting Technology: electronic mail and publishing, computer-supported cooperative working, groupware, Standard Generalized Markup Languages, hypertext markup languages, hypermedia, object-oriented environments, ...

* Semantic and Conversational Systems: knowledge representation schemes for philosophical systems and arguments, different semantic categories and relations, knowledge structuring and integration, dealing with disagreements and contradictions, ...

* Constructive Epistemology: model-building, selection criteria for models, evolutionary epistemology, metacognitive reasoning, levels of cognition: associative, rational, metarational, ...

* Evolutionary Ethics: survival and immortality as fundamental values, freedom and/or integration, self-actualization, individual-society-ecosystem relations, the next metasystem transition: super-being versus metabeing ...

* Process Metaphysics: modelling of emergence and metasystem transitions, actions as ontological primitives, the process of evolution as generator of complexity, levels of organization ...

We would particularly appreciate contributions that cut across these different tracks: for example, emergence mechanisms applicable to evolution and to computer-supported knowledge structuring, or hyperspace as technology and as substrate for "cyberbeings". The emphasis is on contributions that integrate or synthesize multiple domains or issues. They are not limited to these separate topics.

Organization of the Workshop

After the organization of a symposium on "Cybernetics and Human Values" at the 8th World Congress of Systems and Cybernetics (New York, June 1990), the next official activity of the Principia Cybernetica project will be a Workshop at the Free University of Brussels (VUB) in July 1991. The official language is English.

The informal Workshop will allow all researchers interested in collaborating in the Project to meet and to discuss the main problems. It will be introduced by a more formal Symposium where the main issues in developing a systemic philosophy will be expounded, as a basis on which to continue working. The Symposium will take one day, the Workshop will take two or three days, depending on the number of contributions. The attendance to the Workshop will be limited to some 35 participants in order to intensify the interactions; the attendance to the Symposium is unlimited.

The event will be organized in the tradition of a pleasant, informal setting and warm social contacts initiated by the conference on "Self-Steering and Cognition in Complex Systems: toward a new cybernetics" (proceedings edited by Heylighen et al., Gordon and Breach, New York, 1990), which was held at the same place in 1987. There will be a possibility for inexpensive accomodation in university rooms. In addition to that Brussels offers plenty of hotels of all standards. Interested people may combine participation in the workshop with participation in the congress of the Int. Fuzzy Systems Assoc., which is held at the same location, July 7-12.

Brussels, the capital of Europe, is very easy to reach by a variety of means of transportation. As the second international city in the world (measured by the number of headquarters of international organizations), and with its 1000 years of history, it offers many interesting sights to visit. It boasts the most beautiful historic market place and the highest concentration of restaurants in the world.

Submission of Papers

Abstracts of minimum one, maximum two pages (about 300-600 words) should be submitted to one of the addresses below, as soon as possible but not later than March 15, 1991. If possible, abstracts should be submitted in printed and in electronic form: email or 3.5 inch floppy disk (720 Kb MS-DOS or 800Kb Mac) with ASCII text. Authors should give their full address, phone number, and electronic mail address where possible. Abstracts will be evaluated by the scientific committee. Authors will be notified about acceptance before April 15, 1991. It is possible to send in abstracts late (after March 15), but they will be considered depending upon whether there are still places available at the Workshop.

Accepted abstracts will be published in an abstract book available at the conference. The best contributions will be considered for elaboration into full papers, to be published in a book devoted to the Principia Cybernetica project and edited by its present editorial board. An international scientific publisher is being sollicited.

Conference Chairman:

Francis Heylighen (Free University of Brussels)

Scientific Committee:

Francis Heylighen (Free University of Brussels)
Cliff Joslyn (State University of New York at Binghamton)
Valentin Turchin (City University of New York)
Jean Paul Van Bendegem (Free University of Brussels)
Gordon Pask (London)
Gerard de Zeeuw (University of Amsterdam)
Jean Ramaekers (Int. Assoc. for Cybernetics, Namur)

Local Organizing Committee:

An Vranckx, Eric Van Engeland, Alex Housen

For submissions of abstracts, or further information, contact:

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Date: Mon, 3 Dec 90 11:40:06 EDT
Reply-To: "Control Systems Group List (CSG-L)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group List (CSG-L)" <CSG-L@UIUCVMD>
From: Cliff Joslyn <cjoslyn@BINGVAXU.CC.BINGHAMTON.EDU>
Subject: Principia Cybernetica newsletter

The following may also be of interest.

O-->
| Cliff Joslyn, Cybernetician at Large, cjoslyn@bingvaxu.cc.binghamton.edu
| Systems Science, SUNY Binghamton, Box 1070, Binghamton NY 13901, USA
V All the world is biscuit shaped. . .

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Newsletter of the
PRINCIPIA CYBERNETICA PROJECT
No. 0, November 1990
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This Newsletter is going out by both electronic and postal mail to a variety of people who have either expressed an interest, or with whom we hope to develop an interest, in the Principia Cybernetica Project for the development of a consensually based system of cybernetic philosophy. Please spread this information to other people who might be interested, or to other newsletters. The authors of this introductory issue are the members of the Principia Cybernetica editorial board, listed below. Direct all inquiries to them.

WHAT PRINCIPIA CYBERNETICA IS

Principia Cybernetica is an attempt by a group of researchers to collaboratively build a system of cybernetic philosophy, addressing epistemology, metaphysics, and ethics, or the supreme human values. We intend to move towards conceptual unification of the relatively fragmented fields of Systems and Cybernetics through consensually-based philosophical development. Such a unification is proposed in the spirit of the original plans of the founders of the General Systems Theory. It is also in the spirit of Whitehead and Russell's Principia Mathematica. Where they reflexively applied mathematical principles to the development of the foundations of mathematics, in the Principia Cybernetica the objective is to reflexively apply cybernetic methods to the development of the foundations of cybernetics itself.

This philosophical system will not be developed as a traditional document, but rather as a conceptual network. A unit, or node, in the network can be a book, a chapter, a paragraph, a definition, an essay,

a picture, a reference, etc. By linking nodes together, using several types of semantic relations, multiple hierarchical orderings of the network will be maintained, giving both readers and authors flexible access to every part of the system. The network will be implemented in a hybrid computer-based environment involving hypermedia, electronic mail, and electronic publishing [2,3]. Such a system is intended to allow the dynamic development of a multidimensional system fully reflecting and incorporating the semantic relations inherent among the terms being explicated, and to unify and synthesize notations and terminology used in different traditions and disciplines.

Each node will typically represent a separate concept, and contain at least a definition and an exposition. The definition defines the concept in terms of other concepts, by providing specific semantic links to those concepts. Primitive terms are undefined, or implicitly defined by their links with other primitive concepts. The exposition gives additional information about the concept (e.g. bibliographical or historical references, analogies, examples, applications), and may also contain different types of (referential) links. A node may contain text, formulas, drawings, ..., and even programs that perform specific operations (e.g. creating an instance of that concept, with specific attributes, or sending a "message" to another node).

The meaning of a node is partially formal, determined by the network of semantic relations to which it belongs; and partially informal, determined by the personal interpretation of the user who reads the exposition, and tries to understand the concept by associating it with the context. Such a format allows the adequate representation of precise, mathematical concepts, of vague, ambiguous, "literary" ideas, and of the whole continuum in between.

Development of this philosophy is seen as a long-term project involving many participants supervised by an Editorial Board (below). Nodes in the network will be classified according to their status as consensus statements, individual contributions, or general discussion. While traditional publication of parts or the whole of the network by individual authors or small groups will be made periodically, the project is seen as necessarily open-ended and developing, essentially a process of discourse among a community of researchers.

AN EVOLUTIONARY PHILOSOPHY

The organizers have in mind not only a process of development of a cybernetic philosophy, but have also established a strong basis for the content of such a philosophy [4,5,6,8,9]. A philosophical system is seen as a clearly thought out and well-formulated, global "world view" (Weltanschauung), integrating the different domains of knowledge and experience. It should provide an answer to the basic questions: "Who am I? Where do I come from? Where am I going to?". The proposed philosophy is based on the process of evolution which generates subsequent levels of complexity out of more simple components, through the trial-and-error mechanism of variation and selective retention. It includes:

A Process Metaphysics
Our ontology starts from elementary actions or processes, rather than from static objects or particles. Relatively stable "systems" are constructed by such processes through the mechanism of variation and selection. This leads to the spontaneous emergence of more complex organizations during evolution: from space-time and elementary particles, to atoms, molecules, crystals, DNA, cells, plants, animals, humans, and human society and culture. Events of emergence are the "quanta" of evolution. They lead to the creation of new systems with

new identities, obeying different laws and possessing different properties. In such systems, the behaviour of the whole depends on the behaviour of the parts (a "reductionistic" view), but the behaviour of the parts is at the same time constrained or directed by the behaviour of the whole (a "holistic" view).

A fundamental type of emergence is the "meta-system transition" [5,6], which results in a higher level of control while increasing the overall freedom (variety) and adaptivity of the system. Examples of metasystem transitions are the emergence of multicellular organisms, the emergence of the capacity of organisms to learn, and the emergence of human intelligence.

A Constructivist, "Metacognitive" Epistemology

Knowledge is understood as consisting of models that allow the adaptation of a cybernetic system to its environment by anticipation of possible perturbations. Models function as recursive generators of predictions about the world and the self. Models are not static reflections of the environment, but dynamic constructions achieved through trial-and-error by the individual, the species and/or the society. They are both subjective, in the sense of being constructed by the subject for its own purposes, and objective, in the sense of being naturally selected by the environment: models which do not generate adequate predictions are likely to be later eliminated. There is no "absolutely true" model of reality: there are many different models, any of which may be adequate for solving particular problems, but no model is capable to solve all problems. The most efficient way to choose or to construct a model which is adequate for the given problem is by reasoning on a metacognitive level, where a class of possible models can be analysed and compared. This requires a metasystem transition with respect to the variety of individual models.

An Evolutionary Ethics

The fundamental "good" is the continuation of the process of evolution, avoiding evolutionary "dead ends" and general extinction. Natural selection entails survival as the essential value. Although the death of organisms is necessary for biological evolution, it is no longer necessary for cultural evolution. Hence the maximisation of survival leads to the striving toward immortality. It also leads to the desire of actualizing the human potential, to maximally develop the knowledge, intelligence and wisdom which may help us to secure survival for all future contingencies. We contend that humanity is in the process of a new metasystem transition, leading to a yet higher level of evolution: the human "superbeing" or "metabeing". Such (a) being(s) may become "cybernetically immortal": what would survive is not so much the biological material of their bodies, but their cybernetic organization, which may be embodied in organic tissues, electronic networks, or other media. The main problem of an evolutionary ethics is to reconcile the goals of survival on the different levels: the level of the individual (personal freedom), the society (integration of individuals), and the planet (survival of the world ecology as a whole) [7, 9].

Form and Content

The proposed philosophy, constituting the content of the project, and the conceived hypermedia/email implementation, constituting the form of the project, are in fact closely connected. Both are constructive, in the sense that they start from "primitive" systems from a variety of origins (nodes containing expositions written by diverse participants), which are brought into contact (email conversations with a central place where messages are kept in store), connected (semantic links), and selectively stabilized, so as to retain those combinations which define a new, more integrated system. In this way

new systems, models or concepts are continuously constructed, and each newly emerged system can again be recursively used as a building block for constructing further systems. When a system that was constructed earlier does not longer seem to fit the criteria, it can be again "deconstructed", so that its components become available for alternative constructions. In this way the overall system maintains a maximum of flexibility and openness, while continuously proposing possibilities to organize the data in a more efficient way. One way to implement this kind of structuring and restructuring of concepts in a hypermedia system is based on the concept of "relational closure" of a network of concepts [14].

WHAT WE HAVE BEEN DOING

Origin of the Project

The Principia Cybernetica project was conceived by Valentin Turchin, a physicist, computer scientist, and cybernetician, whose political activity and antitotalitarian views led to his forced emigration from the Soviet Union to the United States in 1977. He had developed a cybernetic philosophy based on the concept of the "metasystem transition" with implications for human evolution [5], political systems [7], and the foundations of mathematics [15]. He had a further desire to develop an integrated philosophical system with a hierarchical organization, and involving multiple authors.

In 1987, Turchin came into contact with Cliff Joslyn, a systems theorist [18], software engineer, and proponent of Turchin's philosophy [17]. After discussing Turchin's ideas for a collaboratively developed philosophical system, Joslyn suggested a semantic network structure using hypertext, electronic mail, and electronic publishing technologies as a viable strategy for implementation, maintenance, and production of such an ambitious project. Together they founded the Principia Cybernetica project and formed its first Editorial Board. They wrote a first general proposal (later subsumed in [1]), and a document they called "The Cybernetic Manifesto" [4] in which the fundamental philosophical positions were outlined. Joslyn began publicizing Principia Cybernetica by posting the relevant documents on the CYBSYS-L electronic mailing list.*

This generated a fair amount of response, including that of Francis Heylighen, a physicist, cognitive scientist, and systems theorist [19, 20, 21]. Heylighen had been developing a very similar philosophy to Turchin's [9] and had been thinking along the same lines of creating a network of people interested in this domain who would communicate with the help of various electronic media [12]. He started a very active correspondence with Turchin and Joslyn, and finally joined them as the third member of the editorial board in spring 1990. Heylighen has strong interest in the connection between the content of Principia Cybernetica (the evolutionary philosophy), and its form (the hypermedia organization of knowledge).

Criticisms

Other reactions to Principia Cybernetica were more contentious. The strong tone of the "Manifesto", which was intended to provoke reaction, engendered a sometimes heated debate on the CYBSYS-L list. The most outspoken critic of Principia Cybernetica was Joseph Goguen, who interpreted the use of concepts like "control", "hierarchy" and "integration" as signs of a dangerous, totalitarian ideology. Joslyn and Turchin reacted by stressing the essential role human freedom plays in the philosophy, and by remarking that terms like control and hierarchy should be understood primarily in their abstract, technical sense. In fact, the metasystem transition, where a new control level emerges, should be seen as an increase, rather than a decrease, of the

freedom of the system [8,9]. This criticism led to a deeper understanding of the necessity for careful articulation of the ideas behind Principia Cybernetica, in the hope of avoiding misinterpretation.

Goguen also opposed the striving towards consensus, which is a fundamental goal of the Principia Cybernetica, on the grounds that all opinions are valuable, and that no one viewpoint should be privileged. This criticism is more difficult to answer in a few words.** It was repeated in different forms by different people, mostly those with a "post-modernist" or "social constructivist" philosophy (see e.g. [10]). These critics stress the relativity of knowledge, and the creativity which arises from a variety of different opinions.

But we hold that this creativity can only appear through a confrontation and conversation between the different opinions, and that is just what Principia Cybernetica proposes. Without at least an attempt to reach consensus, people will stick to their own opinions, and no novelty is created. But it is not our intention to impose a consensus, and we start from the principle that Principia Cybernetica must be open-ended: every new idea or opinion can be incorporated somewhere along the way, even if only as a "discussion node". We do not expect to reach a complete consensus in any foreseeable future. Yet we do hold that there is a deep unity in the ideas characterizing Systems Theory and Cybernetics. In our experience, those with a background in Cybernetics or Systems share these fundamental concepts and values, although they may express them with different words. Further, we hold that a fundamental, broad consensus at the conceptual level is necessary for the advancement of a discipline, or a society [16].

Recent Activities

Other criticisms (albeit in a generally sympathetic spirit) about the philosophy behind Principia Cybernetica, and its practical realizability, were made by Gerard de Zeeuw and Rod Swenson to Heylighen when he presented the ideas of Principia Cybernetica to a number of people at the European Meeting on Cybernetics and Systems in Vienna (April 1990). Swenson mentioned in particular the difficulty of maintaining copyright in a network which is authored collectively by many different people. On the other hand, Principia Cybernetica was enthusiastically welcomed by Gordon Pask, who is one of the main theorists in the "social constructivist" paradigm, and the creator of conversation theory.

Much of the efforts of the past year have been devoted to the holding of a session on "Cybernetics and Human Values", chaired by Valentin Turchin, at the 8th World Congress of Systems and Cybernetics at Hunter College in New York in June, 1990. There we presented our ideas [2,8,9], and were very pleased to be joined by BenYamin Lichtenstein (Praxima Institute, Boston), and David White (Philosophy, St. John Fisher College). BenYamin examined the possible connections and contradictions between our proposed cybernetic epistemology and post-modern philosophies [10]. White discussed the concept of consensus in general and proposed some principles for structuring argumentation [11]. We found this an excellent opportunity for the group of us to get together and work on the details of Principia Cybernetica at both the conceptual and technical levels. This has led to the publication of [6] and [3], and to greater communication about Principia Cybernetica with the Systems and Cybernetics communities, from which we got many, mostly sympathetic, reactions.

We were especially pleased by the reaction of Donald T. Campbell, who is well-known for his contributions to experimental and quasi-experimental psychological methodologies and for the creation of the

field of "evolutionary epistemology" [13]. Campbell quickly saw the parallels between his philosophy and Turchin's, and came to join Principia Cybernetica. He sees his contribution to Principia Cybernetica in the first place in a cybernetic analysis of the problem created by the human predicament of participating in a social system while also participating in an individual (and familial) system, a problem which is at the basis of a cybernetic ethics.

WHAT WE ARE GOING TO DO

Workshop

We are organizing a workshop on the Principia project at the Free University of Brussels in July, 1991. The informal workshop would allow those interested in collaborating in the Project to meet and to discuss the main problems. It would be introduced by a more formal symposium (open to non-Principia contributors) where the main consensual ideas behind the Principia would be expounded, as a basis on which to continue working. The symposium would take one day or afternoon, the workshop would take two or three days, and would be organized in the same tradition of a pleasant, informal setting and warm social contacts initiated by the conference on "Self-Steering and Cognition in Complex Systems", which was organized at the same place in 1987 [22].

There would be possibility for cheap but comfortable accommodation in university rooms. In addition to that Brussels, the capital of Europe, offers plenty of hotels of all standards. Brussels is very easy to reach by a variety of means of transportation. As the second international city in the world (measured by the number of head-quarters of international organizations), and with its 1000 years of history, it offers many interesting sights to visit. It boasts the most beautiful historic market place and the highest concentration of restaurants in the world.

All people who have reacted to this newsletter or to one of the previous notices about the Principia Project will receive a call for papers and a first announcement of the workshop. Further inquiries may be directed to Heylighen.

Book

We are further planning to edit a book in which the full development of the Principia Cybernetica concept will be elaborated. The provisional title is: The Principia Cybernetica Project: Computer-aided Collaborative Development of a Philosophical System. Chapters will include: the architecture of the knowledge structures used to implement Principia Cybernetica; the full extent of nodes and semantic structures developed to date; an essay on "cybernetic foundationalism" (our epistemic view of how to build consensus without risking the errors of traditional foundationalism); the properties of the meta-system transition; and the general principles of evolutionary philosophy. This book will probably be published in a series on Systems Science or Cybernetics (e.g. from Pergamon Press, or Gordon and Breach Science Publishers). People who feel that their own research would fit in nicely with this framework can propose possible contributions to the editors.

Introductory Paper

We are writing a paper [23] describing Principia Cybernetica, for submission to a Systems Science journal. This paper should complement the already existing texts which are either too sketchy [1, 4], or which discuss only one aspect of the project: the content [8, 9], or the form [2, 3].

Node Development
We are experimenting with the development of Principia nodes through interactive email conversations among the editors. A first list of nodes, with a definition of the most important ones has already been made during several days of work by the editors, but the definitions need to be further elaborated and complemented by more informal expositions.

Software Architecture
We are researching object-oriented software environments for possible implementation of Principia-related subsystems. The idea is to start with an environment which is as flexible as possible, so that later changes in the organization of the network can be done without having to reprogram the whole system. The advantage of object-oriented techniques is that they allow a great modularity, so that it is easy to add, change, or delete parts of the system without having to change the whole organization. Moreover an object-oriented architecture fits well with the basic idea of nodes (corresponding to objects) arranged in a semantic network, which inherit properties from higher-order nodes, and which communicate with each other via hypertext links (corresponding to messages). An object can contain not only data, but also procedures, so that the semantic network might in some future stage be elaborated into an active support system that would help the user in building models by using cybernetic concepts.

Hypertext Markup Language
We are researching hypertext environments and hypertext markup languages compatible with the Standard Generalized Markup Language (SGML) [24]. Our main requirement is a system that provides a rich and flexible representation of nonlinear textual semantic networks in different formats (text, formulas, drawings) which can be portably communicated across hardware and software environments.

WHAT YOU CAN DO

We are making efforts to develop and maintain contacts with researchers in the Systems Science and Cybernetics communities. We are maintaining mailing lists for both electronic and postal mail, and are engaged in ongoing collaboration with a number of people.

In this first stage, the communication among the different people interested in the Principia Cybernetica will be through this newsletter. If you have not been in contact with us before and are interested in continuing to receive the newsletter, please let us know directly. If you do not respond, your name will be removed from the mailing list. We would appreciate that in your note you would give a short overview of your current interests and how they relate to Principia Cybernetica. More specific proposals about how you might contribute would be helpful.

Interesting ideas or proposals will be published in one of the future issues of the newsletter, and we are soliciting other contributions. Submissions must be in English, and authors can retain copyright if they so indicate. The preferred form for submission is an ASCII computer file (possibly in \TeX\ or \LaTeX\ format), sent by electronic mail or diskette to Joslyn (Mac or DOS). A WordPerfect or Word disk or file will also be accepted, and should be sent to Heylighen (for Mac) or Joslyn (for DOS). For the (we hope very few) people who do not use computers at all, we will try to do the effort to retype their texts on the computer ourselves, but please keep them as short as possibleQnot more than 2 pages for a newsletter contribution, please!

The newsletter will be published in the first place electronically, on a mailing list maintained at the Binghamton computer center. There will also be a printed version which, however, will be much slower in distribution to the interested people. Moreover, if there is a lot of response, then the paper version will contain only a selection or summary of the most important contributions received on the electronic list. Hence, we would urge those interested in collaborating to use electronic mail as much as possible, since this makes the management much easier.

Those people who do not use email as yet are encouraged to inquire about existing facilities at their university or research center. Although email does require a (short) learning period, its use is in general quite simple once somebody has explained the conventions to you (these can be different for different computers and networks). It does not require any knowledge of computer science or programming.

We have (or will have) the need for people to work with us in a variety of capacities, including contributors, reviewers, readers, and general source-people. We would also appreciate help with the administration: sending out mail, editing and printing newsletters and documents, connecting different communication channels (e.g. translating printed or faxed text to electronic texts). If you would dispose of secretarial or technical facilities, or have the time to help, please contact us. We are in particular looking for people with experience in hypermedia and computer-supported collaborative work environments, who might help us in choosing or developing the right tools. If you feel a strong resonance with Principia Cybernetica and the views we are expressing, we would also be very interested in talking about involvement at deeper levels. Please contact one of the editors below.

Editorial Board
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Footnotes:

* This is a mailing list for Systems Science and Cybernetics maintained by Joslyn on the SUNY-Binghamton computer system. For more information, contact Joslyn or the list moderator at the electronic mail address:
cybsys@bingvaxu.cc.binghamton.edu.

** A more fundamental critique of the anti-foundationalism characterizing the post-modern position and our concept of "cybernetic foundationalism" is forthcoming (see below)

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Date: Mon, 3 Dec 90 12:38:39 GMT
Reply-To: "Control Systems Group List (CSG-L)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group List (CSG-L)" <CSG-L@UIUCVMD>
From: mar@CS.ABER.AC.UK
Subject: Re: Marken effect

Rick Marken:

Thanks for your mail. As for the Marken effect, I'm just curious about it at the moment. I would love to have a pre-publication of your report on it when it is available.

To others:

If you have saved some early discussions on the Marken effect, would you please forward them to me? I'm new in this group, so I've missed it. Thanks in advance,

Marcos.

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Date: Mon, 3 Dec 90 10:22:44 -0800
Reply-To: "Control Systems Group List (CSG-L)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group List (CSG-L)" <CSG-L@UIUCVMD>
From: marken@AEROSPACE.AERO.ORG
Subject: Cause/Control

Izack (and everyone else, of course):

Although Izack is probably on the beach at Maui right now (lucky fellow) I would like to make a couple of comments relevant to the discussion about control of input vs output. I will try to add some stuff to Bill's very

cogent points.

First, I think we have a confusion of terms -- from what I read, I think Izack means the same thing by "control" as we mean by "cause". We (perceptual control theorists -- how's that for a name?) see control and cause as two very different phenomena. Cause is involved in control, but it is not the same.
The thermostat does cause the furnace to go on and off and the furnace causes the room temperature to change and the room temperature causes the bimetallic strip to expand, etc. These are causal relationships -- where the value of one variable depends on the value of the other. A variable is controlled when its value is made to be a a particular values that is specified by the value of another variable -- our reference value.
What "makes" control happen is variation in the causes of the value of the controlled variable. Temperature is caused by the control system -- true. But temp is not controlled. The variable that "causes" the temperature does not make that temperature be a particular value.

Look at it this way. Say that a the control system causes temperature according
to the following rule -- ALL ELSE BEING EQUAL.
Temp = 1000*signal where signal is the control systems electrical signal that actuates the furnace. So .07 signal units causes 70 temp units; .06 signal units cause 60 temp units, etc. But .07 signal units does not control the temperature -- making it be 70 units. If we turn on a heater in the room then .07 signal units will combine with the heater output to cause 80 temp units. The control system still has a causal influence on the temp but it doesn't control it -- in our sense of the word control.

If the signal is a reference signal in a control loop and Temp is the perceptual signal corresponding to temp in the control loop, then the signal does control the Temp variable. If signal is set at 30, then the temp variable (which is in the same units as the signal) will be very close to 30 (arbitrarily close, depending on the loop gain). Varying the signal will "cause" concommitant variation in the temp (input) variable. But the input variable is controlled, not caused, because it is kept matching the reference signal by variations that counter the tendency of other variables to move the input from the reference.

Perhaps Bill can make this distinction more coherently -- but I think it is a distinction that is extremely important. Once I realized that there was such a distinction it gave a whole new meaning (or meaninglessness) to statements like "the environment controls behavior" or "genes control behavior". Think about it -- if the environment controls behavior then it must take steps to prevent behavioral variables from drifting from their reference values. Talk about animism!

One other point -- perceptual control theorists see things differnetly than engineering control theorists because we don't have to build the control systems that we are trying to understand. Engineers know what variables they want to control -- there are the "outputs" that they want controlled. Their problem is to design the machine so that it actually keeps the "outputs" matching the references (which also come in from outside the machine because the engineer wants to be able to change the reference "input" to produce a new stabilized output). The thermostat has reference inputs accesible to the user. The user adjusts the references to produce the desired "output". Nobody cares that, from the point of view of the thermostat, what is being done is control of a perceptual input relative to the "user selected" reference.

The situation is quite different for the perceptual control theorist. With living organisms we don't even know what variables are being controlled. Moreover, the reference levels for these variables are set
inside the organsism -- they are not accessible to the psychologist (except by

surgery, maybe). So we need to be very aware of the fact that we are looking at a system that is controlling its own perceptual input relative to internally
specified reference signals. We are in the position of trying to figure out what the themostat is doing (controlling) and why it changes the reference level for the variable it is controlling (if it does). This is very different than the goal of the engineering control theorist.

The perceptual control theorist's analysis of the situation is based on a careful analysis of the relationship between organisms and their environment. It is not based on trying to superimpose an attractive model on some pre-conceived notion of behavior as a response to environmental input. Unfortunately, the latter approach has too often been the one taken by control engineers who try to model human behavior. Again, even this misapplication of control theory can yield interesting information about the dynamics of control in living organisms. But it completely misdirects the inquiry, away from the search for controlled variables, to the analysis of input-output dynamics.

Best Regards

Rick M.

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Date: Mon, 3 Dec 90 13:19:14 -0600
Reply-To: "Control Systems Group List (CSG-L)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group List (CSG-L)" <CSG-L@UIUCVMD>
From: Joel Bennett Judd <jbjg7967@UXA.CSO.UIUC.EDU>
Subject: Thanks & Ethics, cont.

Wayne Herschberger: Thanks for the article, it arrived last week.

Rick Marken and Bill Powers: Appreciate the clarifications/elaborations on previous questions and comments, especially regarding societal aspects of Control Theory. The one overwhelming thought I've had as I've been coming around to a CT point of view is "how powerful this makes a human being!"; that is, one is truly free to act/think as one wishes. We may be persecuted, persuaded, tricked, forced, etc. in order to make us act or think as another would have us, but ultimately, we our responsible for ourselves! My religious beliefs have held this to be the truth for me for quite awhile, but it's refreshing to find a sort of secular basis to hang it on. The insiduousness of S/R really serves to demean the human (or any) organism, doesn't it? Is this (general) idea accurate?

I don't think I was too clear in the last part of my comments. In a nutshell, I was asking if anyone has discussed what I guess might be called the "epistemology of a human control system"? Or, from a CT viewpoint, what does "growing up" mean? Of course, this will bear on the development of language.

Hugh Petrie: Thank you for the ED. references. I will delve into them IMMEDIATELY.

Until later,

Joel Judd

Date: Mon, 3 Dec 90 17:52:09 CST
Reply-To: "Control Systems Group List (CSG-L)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group List (CSG-L)" <CSG-L@UIUCVMD>
From: Bill Powers <FREE0536@UIUCVMD.BITNET>
Subject: Philosophy, Input control, Brooks' model

Cliff J. --

Only a philosophical nitpicker would deny the evidence of a dynamic reality with reliable properties, independent of us. What we can't know is how that reality maps into human perceptual and conceptual space. We're looking at the map, not the territory, and the map is probably not isomorphic to the territory. There's a projection, a transform, of an unknown nature between Reality and Experience. Could it be one-to-one? Sure -- but how can we tell? Everything we know comes in through the same transforms (including the evidence). So there is a direction in the realist/anti-realist dichotomy that goes at right angles to the one-dimensional split. What's that called?

And anyway, what happens to the reality of quarks when someone comes up with a better theory that doesn't use them? The same thing, I'm sure, that happened to phlogiston. Was phlogiston real during the 150 years that scientists believed in it?

Ihzak Bar-Kana:

I know you won't see this for a while, but I presume you'll read your mail when you get back.

I am very pleased that you are so open-minded and willing to work out these problems of communication. I'm interested in the higher-level and social applications of control theory in much the way you are, but I think we ought to get some groundwork laid before going on to such things, so that language and conventions for partitioning control systems don't get in our way. As we go on, I'll try to address selected topics in your communications that might help resolve whatever problems there are. Today's text:

>I am not sure I really understand the difference between my model and
>yours, and may be that what you call "input" I call measured output.

I think you have the key to one of our differences in nomenclature. What I call input is what you term measured output. I call this "output" an input partly because it is the external variable that affects the sensor, an input of the control system, and mostly because it is affected only indirectly by the actual output of the system -- that is, the actuator or effector. The state of the controlled variable is not determined by the system's action alone; control is required because there are unpredictable disturbances that also contribute. If we use the term output for the controlled variable, then we have the odd circumstance in which we can't define the output of the control system itself -- the output depends on independent factors just as much as on the behavior of the effector. I prefer to reserve the term "output" for the effector's action, which is the last thing in the chain of output processes that is completely determined by the control system. Between the effector's output and the controlled variable there are many sources of disturbance -- they are the primary reason that control is needed in the first place. We can agree on the term "controlled variable." But I claim that variable is more closely associated with the sensory input than with the effector output.

>By the way, the basic design would take into account the basic thermal
>properties of the room, and nominal ambient external and internal
>temperatures, and the rate of fuel burned and heat supplied would have
>to maintain this nominal condition "almost" with no other regulation.

I don't think it would be practical to design a home thermostat as a basic open-loop system with feedback added to handle details. The "details" are the whole problem. The steady-state thermal output of the furnace that is needed ranges from zero to the most the furnace can produce, depending on unpredictable heat losses and gains in various seasons and at various times of day and night, varying conditions of cloud cover and wind velocity, and under various conditions of occupancy. A real home thermostat is simply designed by picking a furnace that can keep the room above the maximum desired temperature on the coldest cloudy day at 100 per cent duty cycle, and then letting the feedback do the rest, as it does. The problem a thermostat has to deal with isn't "uncertainty" in the sense of system noise. It's the fact that there are very large and unpredictable disturbances of the controlled variable. When the main causes of variations in the controlled variable are major disturbances rather than noise, there's no way an open-loop branch in the system can accomplish much.

Human systems, I believe, are in the same situation. Most of the output (motor) activites that take place are there to counteract large disturbances of controlled variables. There is very little random variability in the system itself -- ponly a few per cent of the range of action. Living systems got the reputation of being highly variable because the wrong model was applied: psychologists thought that the behaviors were responses to stimuli, whereas they are probably just actions that protect controlled variables from disturbance. The disturbances, of course, were mistaken for stimuli. Because disturbances occur unpredictably, the behavior that counteracts their effects is just as unpredictable. But if you know what variable is being controlled, behavior suddenly looks far more regular: it opposes the effects of disturbances in a highly systematic way.

Suppose you have a motor controlling the angular position of a load through a gear train. The angular position sensor is located at the load and not on the driving shaft, because the shaft can twist and there can be play and runout in the gears. The actual output of the system is not the position of the load, but a torque applied to the armature. If an extraneous force is applied to the load, the torque will immediately rise to counteract it. If the force is too large, the motor will still produce maximum output, but the shaft will not turn: the position no longer is affected by the system's output torque.

If we now remove the position sensor and substitute a tachometer, without changing anything else (except perhaps the stabilization filter and scaling amplifier), the controlled variable will become angular velocity instead of angular position. The output torque, in the steady state, will equal the sum of all frictional and viscous resistance plus any opposing torques, and the angular velocity will match the reference signal. So THE NATURE OF THE SENSOR DETERMINES THE NATURE OF THE CONTROLLED VARIABLE. That's another reason for saying that the controlled variable is an INPUT variable.

The reference signal is certainly one input to the system's comparator. The sensor signal is another. But I like to say "reference signal" because in living control systems, reference signals very often are supplied by higher-level systems, not by sensory inputs. In fact I can't think of any case where a known reference signal comes from the sensory inputs. But we can get into that later. Is my nomenclature beginning to make any more sense to you?

Chih Chen and Mark Nelson:

I concur generally with your comparison of my model with Brooks'. My model is indeed patterned (as nearly as I can) after the organization of "real" (that is, living) control systems. The general rule seems to be that NOTHING important happens open-loop. It's important for a muscle force to be maintained at a specified level, but leverages change and muscles fatigue: therefore there is a control system that senses tension in the tendon and makes sensed muscle tension match a reference signal that enters a spinal motor neuron from higher in the nervous system. The signals actually driving the muscles vary so as to compensate for changes in muscle sensitivity and leverage even when the reference signal is constant. It's the sensed tension that is under control by this "spinal reflex."

The next level up is only a tiny step. The annulospiral sensors embedded in the muscles measure a quantity that is the sum of muscle length and the rate of change of muscle length (for damping). The comparator is mechanical; small muscles in the muscle spindles contract, stretching the length sensors. The error signal provides one reference input to the spinal motoneuron: this is the "stretch reflex."

Up one more level, we have sensors that detect joint angle. In the midbrain and cerebellum, the joint angle signals are compared with reference signals coming from higher levels still. The error signal becomes the reference signal for the stretch-reflex system via the gamma efferents.

This is a simplified picture, of course. Simple's about my limit. There are cross-connections between control systems at the same level, and there are alternate ways of providing reference signals. For example, there is an "alpha" reference signal entering the spinal motoneuron that controls both muscle tone and applied force (in cases where position is fixed by contact with an external object). The sum of the reference signals applied to opposing muscles sets the muscle tone; the difference sets the applied force. The feedback signals associated with each muscle cross over to opposing muscles, where they are connected with the opposite sign. This is true of both the tendon and stretch reflex systems.

Clearly, nature has left nothing to chance and never relies blindly on calibration of outputs. Every possible controlled variable is sensed and there is strong negative feedback involved in every case. The only open-loop system I know of is in the lowest level of oculomotor control systems: there seems to be no kinesthetic feedback of eye position.

This makes evolutionary sense, and the general idea is supportive of Brooks' concept of making each level work to accomplish something significant before another level is added. In my model, however, the lowest levels are much simpler than they are in Brooks' model. His lowest system, for example, avoids obstacles. That actually requires, in the organism, many levels of control (unless we mean just avoiding touches and other contacts, which are indeed handled crudely but effectively at the first level). Note, however, that touch reflexes don't prevent you from touching things. That's a matter of where the reference signal is set by the alpha motoneurone reference signals.

If I were building robots, I would start by putting limb position and applied force under control in every limb that is to be used, and every bendable joint in the body. Then I'd work on creating vector forces in some kind of sensory space composed of a sensory representation of the individual limbs and limb segment forces. Then I'd try to make the thing

hold postures by adding a configuration-control level. Then transitions (movements). Then events (maybe -- I'm beginning to not like that level, or maybe just the term). And then relationships. With the relationship level I could make the thing stand up, maybe walk, and maybe walk so as to avoid obstacles. The vision part, of course, would be tough to accomplish.

Another big difference between my hierarchy and Brooks' is the way the hierarchy is built. His higher-order systems seem to sense the same input variables that lower-order systems sense. I've followed the design principle that nothing should ever be done more than once. If a lower system has already sensed force for its own purposes, then a force signal is available and higher systems can use it as input. The higher system doesn't need its own sensors. It can simply use signals from the sensors that are already involved in lower systems. At the lowest levels there are probably many exceptions to this rule, because there are inputs (like sound and vision) that aren't involved in more than a few simple low-level control systems like the iris reflex. But at the higher levels, I think that essentially all sensory signals are derived from existing signals at lower levels. This makes a nice design because the higher systems whose perceptions are derived from sets of lower level perceptions would naturally control their inputs by varying reference signals entering the same lower systems where the sensory signals originated. If kinesthetic limb position in space is derived from sensory signals representing joint angles (to pick one example), then if those joint angles are individually under control, it makes sense for them to be combined to make a limb- position signal, and for the errors in limb position to be converted into adjustments in the reference signals for the individual joint-control systems. If I were in charge of evolution, that's how I would have done it.

Best -- Bill Powers

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Date: Tue, 4 Dec 90 09:11:00 EST
Reply-To: "Control Systems Group List (CSG-L)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group List (CSG-L)" <CSG-L@UIUCVMD>
Comments: Warning -- RSCS tag indicates an origin of SMTPUSER@UBVMSC
From: Hugh Petrie <PROHUGH@UBVMS.BITNET>
Subject: Re: Education and Models

A couple of comments on education related matters

DILEMMA Gary Cziko. The references to control theory in DILEMMA are in chapter 5 on assimilation. Indeed, on p. 94 I reproduce one of Powers' early schematics of a control system. That chapter also begins to sketch how control systems might be used in education. You ask if learning can be anything more (or less) than reorganization. I think it might be, at least if we allow for the possibility of "fine-tuning". Let me give a couple of examples. As a child is learning to recognize and deal with triangles, the first reference signal acquired may be solely that of an acute triangle. That probably requires reorganization. But what amount of reorganization is necessary to add obtuse triangles to the system? I would say, not much, probably just some refinement of the reference signal and perhaps the perceptual input. On th other hand to learn that electricity can light a bulb without any loss of current, probably requires a reorganization of more massive proportions. If one insists that all learning is reorganization, I wouldn't object too much, since that is actually one of the more persistent criticisms leveled at DILEMMA by the philosopher types. I just had this intutive idea that there is probably a distinction between fine-tuning existing systems and really coming up with significant changes--

conceptual changes as they are called elsewhere. I suspect that they do shade into one another. Of course, the trick for the teacher introducing disturbances is to arrange the environment so that the result of reorganizing along "preferred" lines will quickly reduce the error. This includes both providing ideas about the new reorganized system as well as "reinforcing" correct answers. Of course, reinforcement is just reducing error at another level for the student--at the level of wanting teacher approval and if they don't want that, as many alienated students don't, then the "reinforcement" doesn't work.

As to how I can be a dean when many of my faculty are doing "irrelevant" research, the answer I think is similar to the main ethical lesson of control theory--respect other control systems. An;yway I have a higher order goal of having my school of education well regarded and so I realize that most people don't understand control theory and will react favorably to more traditional research. I also throw in a few disturbances now and again to see if I can get anywhere, and I am seriously considering running an advanced seminar next year for faculty and students to see if some of them can be turned on to control theory.

Joel Judd--Right, control theory is truly liberating and that is also one of the problems with its name. It suggests to most people, the exact opposite.

Cheers, Hugh Petrie

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Date: Tue, 4 Dec 90 09:40:12 -0600
Reply-To: "Control Systems Group List (CSG-L)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group List (CSG-L)" <CSG-L@UIUCVMD>
From: g-cziko@UIUC.EDU
Subject: Eye Control

Bill:

>Clearly, nature has left nothing to chance and never relies blindly on
>calibration of outputs. Every possible controlled variable is sensed and
>there is strong negative feedback involved in every case. The only open-
>loop system I know of is in the lowest level of oculomotor control
>systems: there seems to be no kinesthetic feedback of eye position.

Trying to understand control theory can lead you to do crazy things. Today is no exception. Here I am sitting with my eyes closed and trying to find out if I can control the movement and position of my eyes. It's a very interesting experiment. If I close my eyes and move my *arm* to a certain position, I can sense very clearly where my arm is. With my eyeballs, the feeling is quite different. I close my eyes, look to the left and if feels at first as if my eyeballs are turned in the desired direction, but then after a few seconds I'm not sure if my eyes have wandered from this position. However, I feel as if I can control the *movements* (transitions) of my eyes under closed lids (for example, I feel as if I can repeatedly look from left to right, or up and down), but not maintain a position (configuration). I wonder if this control of movement is due to the feedback provided by the inside of the eyelid concerning where the bulge of my eyeball (cornea) is.

I suppose none of this should be too surprising. When our eyes are opened, the image on the retina gives us all the feedback we need about their position and so propioceptive feedback is unnecessary. Such feedback would only be necessary to control eye movements in the dark or under closed lids, but what good would this control of the eyes be when they are not functional?

Tonight I'll have to try to see what control of my eyes I have when they

are open but in total darkness (if I can find some). If it is found that we cannot control the position of our eyes without visual feedback, wouldn't this be a nice demonstration of the basic tenent of control theory that we cannot control behavior but rather control only the perceptual input? Perhaps Wayne Hershberger and his group can fill us in on this.--Gary

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Date: Tue, 4 Dec 90 08:35:14 -0800
Reply-To: "Control Systems Group List (CSG-L)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group List (CSG-L)" <CSG-L@UIUCVMD>
From: marken@AEROSPACE.AERO.ORG
Subject: Lots of little comments

Joel Judd -- Another relevant reference, on the development of control systems,
is F.X. Plooij (1984) The behavioral development of free living chimpanzee babies and infants. Norwood, NJ: Ablex. Plooij and his wife have a nice summary of their work in the control theory issue of American Behavioral Scientist (October,1990) Sage Publications, Newbury Park, CA.

Hugh Petrie -- great to hear from you. Thanks for taking the time to provide the very interesting posts.

Gary Cziko -- Yes, control theorists do end up doing strange things, don't they?. Once I find some privacy I'm going to try you eye experiment myself.

Bill P. -- The random procedure for search for the best parameters worked poorly -- but it was a pretty dumb algorithm. I finally did it manually. I determined that the sampling rate in my tracking task is 30/sec. I stored 900 samples of my tracking data for analysis. The best fit to my data was a model with gain=.2, leak=.002 and lag = 5 (150 msec). The model was h(t) = h(t) + gain * error - leak * h(t) and the error was (r - x(t-lag)). Does this lag match your result? I think it does but I lost the post where you mention what it was. I know that my gain and leak values are different but I think that's because you had a higher sampling rate an, thus, your lag was on the order of 15 rather than 5 samples.

I am still puzzled about how to determine whether variables are hierarchically related or not. I've ben fiddling around with my area control experiment again. Is area the controlled variable? Is it a higher order variable than the length of the component lines? My subjective experience is that it is harder to control area than the length of a line. Might this be due to hierarchical relations between the variables? Could you suggest some easily variable aspects of a computer display that might by just one or two perceptual levels apart? I'm interested because I think this notion of controlling perceptions of various levels of complexity is one that makes it easier for people to see the generality of perceptual control theory and how control theory is not just "manual control".

I know you are busy but I'd appreciate whatever help you could provide.

Thanks

Rick

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213 336-6214 (day)
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Date: Tue, 4 Dec 90 11:55:53 EST
Reply-To: "Control Systems Group List (CSG-L)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group List (CSG-L)" <CSG-L@UIUCVMD>
From: "CHARLES W. TUCKER" <N050024@UNIVSCVM.BITNET>
Subject: Re: Philosophy, Input control, Brooks' model
In-Reply-To: Message of Mon, 3 Dec 90 17:52:09 CST from <FREE0536@UIUCVMD>

I PROPOSE THAT A DOCUMENT BE CONSTRUCTED THAT INCLUDES ALL OF THE CHOICE STATEMENTS WHICH EXPLAIN THE POSITION OF CSG REGARDING PHILOSOPY, INPUT CONTROL AND THE MOST ESSENTIAL MATTERS. THIS COULD BE PUT ON A LIST THAT MEMBE
RS CAN ACCESS; NEW MEMBER CAN HAVE THIS AS AN INTRODUCTORY STATEMENT AND CAN INQUIRY FROM THAT BASIS. I KNOW IT MAY SOUND COLD BUT I THINK THAT IT WOULD BE MUCH MORE EFFICIENT THAT REPEATING THE SAME MATERIAL OVER AND OVER ESPECIALL
TO THE CHOIR.

THE ISSUE THAT SEEMS TO BRING ABOUT THE MOST DIFFICULTY IS THE ONE OF EPISTEMOLOGY, THAT IS, WHAT IS THE NATURE OF REALITY, THE REAL WORLD, ESSENTIAL OR PERMANENT WORLD? I THINK THAT IT SHOULD BE FIRMLY STATED THAT THE BASIC EPISTEMOLOGICAL POSITION OF CSG IS ONE WHICH WAS CALLED BY AN EDITOR OF THE CYBERNETICS AND SYSTEMS LIST AS "RADICAL CONSTRUCTIONISM" IT IS THE SAME POSITON TAKEN BY VON FORESTER (my spelling is wrong here) WHICH STATES VERY SIMPLY THAT HUMANS MAKE THE WORLD OUT OF THEIR EXPERIENCES AND IMPOSE WHATEVER FORM, SHAPE, MEANING, SENSE, AND SO ON "IT" (yes, there is a good reason for the quotation marks there). THAT IS IT IN A BRIEF FORM!

WE ALWAYS SEEMS TO GET INTO THIS ISSUE IF WE CONTINUE TO TALK FOR EVEN A DAY OR TWO (it happens in every meeting held by the CSG that I have attended - usually between Wayne and someone else{this is not a flame against Wayne}) IT IS A MINORITY POSITION TAKEN BY PERHAPS LESS THAT .001 OF THOSE WHO DARE TO CONSIDER IT - IN MY EXPERIENCE OF TRYING TO CONVINCE MANY PEOPLE OF THIS POSITION MY SUCCESS RATE IS EXTREMELY LOW SINCE, IN PART, FOR EVERY SINGLE STATEMENT I MAKE FOR THE VIEW A PERSON MIGHT HEAR AT LEAST 50 STATEMENTS FOR THE OPPOSING POSITION - NEEDLESS TO MENTION THE REALIST POSITION IS THE DOMINATE ONE IN THE WESTERN WORLD. SO LET US JUST STATE THE POSTION CLEARLY AND POINT OUT THAT IF THESE EPISTEMOLOGICAL ASSUMPTIONS ARE NOT USED THEN A PERSON WILL HAVE A DIFFICULT TIME UNDERSTANDING WHAT WE ARE DOING. IF FACT, IT MAY BE IMPOSSIBLE TO UNDERSTAND AT ALL WHAT WE ARE UP TO WITHOUT USING (note my word is 'using') THIS APPROACH. AGAIN I HAVE FOUND THAT THOSE WHO DO NOT USE THE CONSTRUCTIONIST APPROACH FAIL TO UNDERSTAND CYBERNETIC CONTROL THEORY AND ALL OF ITS PRESENTATIONS. THIS IS A MAJOR PROBLEM ENCOUNTERED BY THOSE WHO ATTEMPT TO UNDERSTAND AND USE THE APPROACH. OF COURSE, I WOULD ADD THAT UNDERSTANDING THE EPISTEMOLOGICAL POSTION IS NECESSARY BUT NOT SUFFICIENT FOR UNDERSTANDING ALL OF THE RAMIFICATIONS OF CCT.

I HOPE I HAVE NOT OFFENDED ANYONE BY THIS POST BUT I THOUGHT THAT IT SHOULD BE STATED.

OK LET US CONTINUE THE TALK ABOUT EPISTEMOLOGY!!!!!

CHUCK

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Date: Tue, 4 Dec 90 15:02:14 -0800
Reply-To: "Control Systems Group List (CSG-L)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group List (CSG-L)" <CSG-L@UIUCVMD>
From: marken@AEROSPACE.AERO.ORG
Subject: Epistemology

Some random musing-- probably stimulated by Chuck Tucker's post so blame him!

While sitting here at my computer, prototyping a satellite ground control system, I realized I am neither a materialist or a vitalist (or whatever the two poles of epistimology might be -- realist--idealist?).
I believe in models. That's my epistimology. I like Bill's point -- there is a "reality" out there but all we have, and all we will ever have, is the map. We have no direct access to the territory. But we can make up models that produce the kind of experiences that are caused by "reality". Thus, we can build models of reality -- models that we can understand. The modeler's epistimology is something like this:
"I would have the following experiences if reality were like this model."
What is amazing is that,lo and behold, some of these models not only explain what we experience but predict experiences that we have never had -- implying that reality is a lot like the model.

A metaphor for science: A modern version of Plato's cave shadows -- our experience is like the shadows of objects on a backlit, translucent screen. All we see is the image on the screen. But we can make up models of what the objects are like that we see. We can test these models by pushing on the objects. If we see what we expect we gain more confidence in the model. We may never "know" what is actually causing our experience. But we are in the fortunate position of being 1) smart enough to invent stories (models) that have testable consequences and 2) capable of doing things that affect our experience (being that we are control systems) in order to test
for the consequences predicted by the model. These two capabilities, in combo, give us a wonderful approach to truth. Some people only use method (1) -- religious types -- and others only use (2) -- dust-bowl empiricists. But in combination things get very beautiful.

Just some thoughts on a Tuesday afternoon. Now back to work.

Rick.

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Date: Tue, 4 Dec 90 17:40:44 CST
Reply-To: "Control Systems Group List (CSG-L)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group List (CSG-L)" <CSG-L@UIUCVMD>
From: Bill Powers <FREE0536@UIUCVMD.BITNET>
Subject: Human CS, Principia

Izhak Bar-Kana and Robot Builders:

When an engineer designs a control system there is a control problem defined in advance. The engineer can see what effectors are needed, what sensors are needed, and even (sometimes) how control might be achieved by open-loop means (if there aren't any unpredictable disturbances of the controlled variable). That's because the engineer has an internal reference defining the desired result and has the ability to shape a device that will have known effects on the physical world. The engineer can see all the inner details of the control system, and also has advanced knowledge of the properties of the physical world with which that system will interact.

An evolved organism like a human being, in the process of becoming organized, doesn't know any of that. The environment is known only through sensory inputs, and through direct physiological effects of the environment on the body (the state of which is also sensed or at least sampled). Actions that affect the environment are produced only by sending signals into muscle systems. There is no a priori information in a growing brain concerning physical properties of the environment or the body, or any "laws of nature" or general principles. The brain can't use any of the engineer's knowledge in building up its own control systems as it matures.

Whatever the brain does, it must do on the basis of available information and whatever amount of organization it has and has acquired at a given stage of development. It discovers properties of the external world relevant to control only by acting and sensing the result. No information is available about what happens between action and sensed result: only the result is experienced. Many consequences of acting are insignificant and unrepeatable; only some are consistent and therefore possible to control. The organism learns about the consistent sensory results, selects remembered states of those results as targets to repeat, and by trial and error finds the combinations of output acts that will tend to restore the sensed world to any former state, starting in any other state. That is called control.

It does not need to know, in most cases never does learn, WHY performing a given act results in a given effect on perception. It does not need to know the true nature of Reality that explains why perceptions behave as they do. Only when higher-level cognitive systems develop does a brain begin to acquire a symbolic understanding, a model, of the external world, so it can explain why acting in a certain way is necessary if control of a certain kind of experience is to be possible. Only at that stage can someone become a control-system engineer -- and by that time, the vast majority of the engineer's own personal control systems has been in place for years.

If you ask a child, or for that matter almost any adult, WHY turning a certain knob on a television set makes the picture get brighter, the answer is going to be something like "Because that's the brightness control." But nobody has trouble with adjusting the brightness to whatever level seems "right." Control is not based on understanding of the physical world. That's lucky for you, if you are a bacterium or a baby.

Cliff Joslyn:

Principia Cybernetica sounds like a major project, but it's not for me. I'm

trying to learn how an individual person works, and I want to do it by testing models experimentally. So I think I'll just stick to my little niche and see what good I can do here. I can't speak for others in the CSG, but my impression is that control theory doesn't agree with many of the premises that you seem to be taking as foundational. But maybe as you become familiar with the CSG approach (when you have time) you will be able to correct me on that.

Among the members who won't let me speak for them is my wife, Mary, who has recovered enough to have her own opinion, faithfully (almost) rendered

here:

FROM Mary Powers:

What better way to debut on the net than to thank everyone who sent me notes & cards and especially Clark and Tom for the flowers. I love you too, guys.

Gary: January is too soon for Bill. Maybe March. [Hey, I was going to say that. Let's try for March, if February is out. Back to Mary].

Cliff: My reaction to your cybernetic postings is to get kind of crabby -- what is this cyberbabble doing on OUR network?

We used to meet at cybernetic conferences until we realized that most of our conversation was with each other, that no one else much cared about doing science, and that no one remembered, knew, or cared that control theory was fundamental to the original idea of cybernetics. So we spun off the Control Systems Group in 1984 and have been happy ever after, especially with CSGnet this year.

Checking back with one or another cybernetics meeting most years since '84, there seems no change in our outsider status or the widening divergence in our goals. The Principia posting doesn't do much to narrow it.

[I get the last word. That's a little rough, but I guess I agree, all but for the part about OUR network. Anyone who wants in is in. The only question is whether any of us who are in care to be lured out. It's a free country, isn't it, so far? Lure away.]

[Gary Cziko: I did mention the demo disks, didn't I? Did my posting of Nov. 27th get through, starting with the news that Mary was home?]

Best -- Bill

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Date: Tue, 4 Dec 90 19:33:24 CST
Reply-To: "Control Systems Group List (CSG-L)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group List (CSG-L)" <CSG-L@UIUCVMD>
From: Bill Powers <FREE0536@UIUCVMD.BITNET>
Subject: Parameters; LEvels; Eyeballs

Rick Marken:

The random-walk-control-system method of matching the model might work better if each control system sensed the type of difference between model and person that is most affected by the associated parameter. The integration constant would probably affect the direct amplitude difference the most. The transport lag might affect the RMS difference in first derivatives the most. I don't know what the leakage would affect the most, but it could be in the outer loop and just be stepped through likely values each time the other two find best values. But I really don't want to make a research project out of this, as long as we have a way that works.

The values you get are very close to mine. My frame rate is 70 per second, and I get a transport lag of 11 frames, or 0.157 sec. Not bad, since you get 0.15. That's as close as we can resolve, isn't it? For integration factor I get anywhere from 0.08 to 0.09 -- per 1/70 second. Multiplying by 70/30, that's 0.186 to 0.21 -- again, right on compared to your 0.2. My leak is around 0.001 to 0.0005, but the results are very insensitive to the exact value. And again, you'd have to multiply me by 70/30 to compare with your number. I'd say you've replicated my results.

Maybe one way to solve this damned hierarchy thing is to start with a high-level task and work down. I suspect that we're having trouble because all of our tracking experiments involve RELATIONSHIPS. It's hard to think of one that doesn't. But maybe we could separate configuration from relationship by using shape: a figure that changes from a flat diamond to a tall diamond, say, to match a second figure that changes shape at random the same way. Or better yet, that changes oppositely: when the other figure is tall, the controlled one should be flat, etc. That's a relationship, I think. And shape is certainly a configuration variable. I hope.

We ought to be able to find tasks in which the control parameters are clearly different from those in tracking tasks. I wish we had torque motors on the control handles; then we would probably see second-order control clearly, with much faster time-constants than we see in the whole tracking task. Maybe we'll just have to go to a cognitive control task.

[For onlookers, Rick and I have been sweating over finding experimental methods for demonstrating multi-level control unequivocally, without a lot of luck. The problem is that subjects pick the easiest way to control, which is the lowest-level way, and we don't see the levels we expect. We would see them by finding different control parameters when we disturb the controlled variables associated with different levels. We're probably missing something obvious here, but neither of us can see what it is (other than wondering if levels exist at all). Maybe one of those high-powered observers out there can come up with some practical ideas (i.e., cheap). One constraint is that we want to do this in a continuous control task -- no reaction-time stuff. Although we may be driven to that. Also I should admit that when I say what "we" should try, it's Rick who actually sets up most of the experiments and really does them. I usually just write a quickie and try it out on myself to see if we get the same numbers. We're both experienced trackers, of course. Tom Bourbon down in Texas is doing all the work that involves many subjects. He's exploring multiple-person tracking tasks and getting wonderful results. The models behave just like the participants. But all one level at a time.]

Eyes: In one experiment, subjects wear a contact lens with a stalk on it, and fixate on a point ahead of them. When the experimenter pulls the stalk to one side with a thread, the visual scene seems to swing the other way. If the pull is hard enough, subjects feel their eyes rotating 180 degrees (looking out the back of their heads), although the eye actually remains fixated on the target. Some subjects have to call a halt out of sheer horror.

Gary: Greg Williams has all my oculomotor control system materials. Ask him for the references.

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Date: Tue, 4 Dec 90 21:55:02 EDT
Reply-To: "Control Systems Group List (CSG-L)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group List (CSG-L)" <CSG-L@UIUCVMD>
From: Cliff Joslyn <cjoslyn@BINGVAXU.CC.BINGHAMTON.EDU>
Subject: Re: Human CS, Principia
In-Reply-To: Message from "Bill Powers" of Dec 4, 90 at 5:40 pm

Bill and Mary:

> I can't speak for others in the CSG,
> but my impression is that control theory doesn't agree with many of the
> premises that you seem to be taking as foundational. But maybe as you
> become familiar with the CSG approach (when you have time) you will be able

> to correct me on that.

I suspect we have more in common than it seems at first glance. Or rather, the difference between our views and yours is much less than our collective difference from the traditional disciplines. (See my reply to your wife, also, below). One critical thing that we share is the focus on the evolution of emergent levels of control, and I suspect that (when we have time) we'll find a very strong ressonance with your work.

> Cliff: My reaction to your cybernetic postings is to get kind of crabby --
> what is this cyberbabble doing on OUR network?

I agree with you that Control Theory, and likely your brand of control theory, is at the core of cybernetics, and THAT's why my cyberbabble is on your list: it is relevant. (Not that I mean to monopolize this list, and I'm sure I'll shut up soon).

You should know that I share your frustration with the "traditional" cybernetics community. They're mostly navel-gazers, lost in their own second-order reflections in the mirror, never questioning the distinction between recursion and regression.

I also agree with Chuck Taylor that the epistemology you express is "radical constructivism" (he quotes me as saying that, although I don't remember it), but that you SHARE with traditional cybernetics (perhaps they're on your "left wing" and we're on your "right wing").

[To Chuck: A quick gloss on epistemology: we hold neither a realist nor an anti-realist view, but rather an agnostic, a-realist view. To assert that "we construct the world" is absurd; but to assert that "we construct our knowledge of the world, and that is all we have" is profound. To take Kant's observation to heart, that direct perception of the world is impossible, is indeed the first step away from naive realism. But it is a deep error to proceed too far away from realism, and state that our knowledge of the world, although filtered, and all we have, IS the world. This is logically equivalent to saying that just because we cannot know that X is true, therefore X is false. Instead we must stop at "critical realism": "the world" is a hypothetical entity which we infer from perception.

BTW, the idea of an "Introduction to CST" article is wonderful. Please send it to me!]

To return to the Cyb/Sys/CST community: one idea that motivates our Principia work is to try to move back towards the promise of the early systems theorists on unification, attempts to integrate the communication among us trans-disciplinaries. The distance between the systems scientists, the cyberneticians, the control theorists, the systems dynamicists, etc., is deplorable: we're all doing the same thing! And we all suffer from the fragmentation, and continue to toil away unorganized and unsupported. Yes, their ignoring you is short-sighted; but retreating into an isolated community of your own is no solution. You may recall that Kirstie Bellman (also of Aerospace, Rick) and I spoke to this issue at Tilton this summer.

> The Principia posting doesn't do much to narrow it.

Of course I disagree. We may very well adopt something very close to your view of what "control" is. At the very least we will do a thorough analysis of the concepts of "control", "emergence", and "hierarchy", including your work. This will provide a link to the rest of our work on evolving systems, metacognitive epistemology, and the ethics of survival.

> [I get the last word. That's a little rough, but I guess I agree, all but
> for the part about OUR network. Anyone who wants in is in.

Actually, I agree with Mary in part. This list is still small, and with exceptionally high quality (except for my cyberbabble). But it can easily be flooded by either poor quality postings or irrelevant material. We found moderation on CYBSYS-L critical early on. While I will hopefully continue to post general information about our greater work here, I have tried to make my written comments relate directly to CSG-L. Although I have been drawn into other areas, I disagree with Mary in that I hold that my comments are important (or should be important) to CSG members.

O-->
| Cliff Joslyn, Cybernetician at Large, cjoslyn@bingvaxu.cc.binghamton.edu
| Systems Science, SUNY Binghamton, Box 1070, Binghamton NY 13901, USA
V All the world is biscuit shaped. . .

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Date: Tue, 4 Dec 90 21:22:00 EST
Reply-To: "Control Systems Group List (CSG-L)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group List (CSG-L)" <CSG-L@UIUCVMD>
From: LO0745@ALBNYVMS.BITNET
Subject: Intro & Misc.

CSG Network:

Greetings and a very belated intro. My name is Lynne Oetjen-Gerdes and I am working on my Ph.D in Anthropology at the State University of NY, Albany. My systems/modelling training is primarily with George Richardson. I am interested in evolving hierarchical systems...particularly the role of mind in diverse types of more global change; i.e. biological, etc. I am developing a model to test the hypothesis that "reality is socially constructed." More specifically, I believe that "convention" and "relativity" may be two different ideas which can and must be teased apart. I support control theory (in particular Bill Power's work), but I don't believe it has to lead to a constructivist theory of reality. I hope the network has no plans to issue such a manifesto. I'm afraid I can't be more specific at the moment...I am still working on my ideas and my theory (like so many others here). I am also cautious about making unsupported claims.

Re: Reorganization - I strongly suggest the writings and research which surrounds Milton H. Erickson. Dr. Erickson was a psychiatrist who was a master at confusion techniques, reframing and multiple imbedded metaphors aimed at bypassing conscious control and instituting behavioral change. Some of
the other names associated with his work are E. Rossi, and S. & C. Lankton. Gregory Bateson spent some time with Dr. Erickson as did Margaret Mead. An excellent book on learning and change is Aesthetics of Change by Bradford P. Keeney (Gregory Bateson was his mentor).

Bill: George would like to get you to Albany to speak...possibly for a combined group of Anthropologists, psychologists and systems people. Has he spoken to you lately?

Small Concerns....I left Cliff's network because the mail volume became intolerable. Will that eventually be a problem here?

Lynne <LO0745@Albnyvms>

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Date: Wed, 5 Dec 90 12:42:56 -0800
Reply-To: "Control Systems Group List (CSG-L)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group List (CSG-L)" <CSG-L@UIUCVMD>
From: marken@AEROSPACE.AERO.ORG
Subject: Hierarchy

Bill P.

It's very nice to know that we get a match in our models -- very nice. So there is a 150 msec transport lag when controlling the cursor/target relationship in a tracking task. Thanks also for the great suggestions on studying control of relationships. I will try your idea and several that have occurred to me based on your suggestion. I do want to make one point. I think that the problem we are having with studying the hierarchy with the tracking type tasks is that people are controlling a configuration -- not a relationship. I thing people control the distance between cursor and target and have little interest in how movements of the cursor relate to those of the target. So the problem, I think, is getting people to control a relationship perception
rather than a configuration. I think your suggestion might help -- but then people might match two configurations again (I think area is a configuration) and the difference between areas (it may be a sensation -- of brightness -- rather than area) is just another configuration. What we have to do is design a task where differences or sums are irrelevant; where the subject can control the "relationship" only in terms of the perception of a relationship.

Those of you who are listening in -- the above may sound crazy but, trust me, it's not. I will try to make this more coherent later in the week or next week after I've tried some tests of my ideas about how to get people to control relashionships as relationships. How will I know that they are doing so, you ask? Two possibilities: look for nested control relationships -- that, I have found, will be very difficult to get. The other is to look at the models of the task and see if the model of what is presumed to be relationship control requires different parameters -- particularly, a longer lag.

Hasta Luego

Rick

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Date: Tue, 4 Dec 90 09:34:55 GMT
Reply-To: "Control Systems Group List (CSG-L)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group List (CSG-L)" <CSG-L@UIUCVMD>
From: Chung-Chih Chen <chen%arti1@VUB.VUB.AC.BE>
Subject: Brooks' and Powers'

Mark Nelson:
Thank you for the detailed reply.

> It is true that the Brooks hierarchy is defined in terms of navigational
> competence and that the Powers hierarchy is defined in terms of data
> abstraction. When you say "difficulty of the control function" do you mean
> the control function controls a variable which is abstracted from the real
> world. Is an example of a `simple' control function one that controls a
> muscle while a `difficult' control function is one that controls an aspect
> of personality.

Yes, that's what I mean.

> I believe that integrating the two approaches is a very interesting idea.
> The approach would be to use the Brooks hierarchy of navigational competence

> to define the evolutionary path of the control system. That is, the behavioral
> development of the system can be performed by implementing the lowest level of
> behaviour and then incrementally building each higher level in succession. The
> implementation of each level would be in terms of a Powers hierarchy. This
> approach combines a model of how biological processes perform control (i.e. the
> Powers hierarchy of control systems) together with a model of the behavioral
> evolution of biological systems (i.e. the Brooks hierarchy of competence).

One thing in Brooks' architecture that I don't like is that it's too redundant. Because each level has to manage its own input (and everything else) without using any information from lower levels. Although it's claimed that such redundancy is exactly its advantage (which makes it more fault-tolerant). Another problem about Brooks' decomposition of behaviors is that HOW MANY behaviors you need for a human beings. Although using only some behaviors may be enough to achieve interesting work (as in robotics).

> My own work started with the Brooks behavioral paradigm. In the last 12 months
> since I encountered the Powers philosophy I have been able to describe my
> control architecture in terms of control theory. That is, I have a distributed
> network of control processes that each have a reference and perceptual input,
> an error output, etc. I have not explicitly applied the Powers hierarchy though
> one of the dimension of my control architecture is a hierarchy of data
> abstraction that seems to parallel the Powers hierarchy. My hierarchy is
> defined according to the data being modeled within the system. I have found
> that all the control processes and data models (stored in Long Term Memory)
> form disjoint groups where all the processes that share models do not access
> data from models in other groups. The models in each group seem to contain
> data that is of a similar level of abstraction and hence the hierarchy. My
> hierarchy is as follows:

Level	Model	Description	Example
4	Precepts	Rules of behaviour	If lost then find a landmark
3	Maps	Relationships between objects	Spatial map of landmarks
2	Objects	A structure of percepts describing an object in the environment	A perception of a landmark such as a hill top or a fork in a stream
1	Percepts	A sensory pattern used to match and interpret a sensory experience	A prototype for the visual perception of a hill top
0	Reality	The real world	The physical robot (sensors, motors, etc) and the static and dynamic environment

It seems that your Level 4 corresponds to Brooks' behaviors. Each behavior is a control process (what you called), right?
The control processes may use (share) Level 1 (or 2, or 3).
So it's not as redundant as Brooks'.

Does your work have anything to do with the cognitive map of W. K. Yeap (also from NZ)?

Bill Powers:
> Another big difference between my hierarchy and Brooks' is the way the
> hierarchy is built. His higher-order systems seem to sense the same
> input variables that lower-order systems sense. I've followed the design
> principle that nothing should ever be done more than once. If a lower
> system has already sensed force for its own purposes, then a force
> signal is available and higher systems can use it as input. The higher
> system doesn't need its own sensors. It can simply use signals from the
> sensors that are already involved in lower systems. At the lowest levels
> there are probably many exceptions to this rule, because there are
> inputs (like sound and vision) that aren't involved in more than a few
> simple low-level control systems like the iris reflex. But at the higher
> levels, I think that essentially all sensory signals are derived from
> existing signals at lower levels. This makes a nice design because the
> higher systems whose perceptions are derived from sets of lower level
> perceptions would naturally control their inputs by varying reference
> signals entering the same lower systems where the sensory signals
> originated. If kinesthetic limb position in space is derived from
> sensory signals representing joint angles (to pick one example), then if
> those joint angles are individually under control, it makes sense for
> them to be combined to make a limb- position signal, and for the errors
> in limb position to be converted into adjustments in the reference
> signals for the individual joint-control systems. If I were in charge of
> evolution, that's how I would have done it.

I agree.

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Free University of Brussels
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1050 Brussels, BELGIUM
(email: chen@arti.vub.ac.be)

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Date: Wed, 5 Dec 90 21:58:00 CST
Reply-To: "Control Systems Group List (CSG-L)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group List (CSG-L)" <CSG-L@UIUCVMD>
From: TJ0WAH1@NIU.BITNET
Subject: epistemology; eye movements in the dark

Regarding epistemology: I, for one, share Lynne Oetjen-Gerdes' reluctance to issue a manifesto on this, or any other, subject. Let's continue the conversation.

Chuck Tucker: What is the difference between Representationalism and Radical Constructivism? The Representationalism which Hume reduced to Radical Skepticism involved a correspondence theory of truth: REALITY<-->REPRESENTATION. What is RC's theory of truth? That is, how does one determine the truth value of a "construction"? I submit that scientists do not compare their constructions or models with REALITY. Rather their models are attempts to DEFINE reality (however well or ill). The truth value of a model may, perhaps, be determined by the degree of correspondence between the phenomena (data) that the model generates and the phenomena (data) it is supposed to explain. That is why generative models are good--as Bill Powers is wont to assert--and non-generative models are bad (i.e., worthless). BUT NOTE!!! This "correspondence theory of truth"--if that's what it is--is not the traditional one Hume was critiqueing. The degree of correspondence assessed here is a correspondence between sets

of PHENOMENA (not between model and REALITY)! Does it avoid Radical Skepticism? Is it a legitimate theory of truth, do you think? Which particular phenomena are to be used in these tests? Are all phenomena so trustworthy?

Gary Cziko: M. J. Steinbach (in several different studies) has investigated the ability of persons to look toward their hand in the dark. They do so very well. Also, A. A. Skavenski found that persons maintaining a direction of gaze in the dark will actually compensate a bit for disturbances such as the one Bill Powers was describing in his recent posting: Bill was describing another one of Skavenski's experiments.

Warm rgards, Wayne

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Professor of Psychology
Department of Psychology Home: (815) 758-3747
Northern Illinois University
DeKalb IL 60115 Bitnet: tj0wah1@niu

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Date: Thu, 6 Dec 90 08:49:58 -0600
Reply-To: "Control Systems Group List (CSG-L)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group List (CSG-L)" <CSG-L@UIUCVMD>
From: g-cziko@UIUC.EDU
Subject: Williams on CSG Net Digest

(Introductory Note: The following is from Greg Williams which I have uploaded to CSG-L since Greg does not yet have direct access. Public responses to his note should be sent to CSG-L. Private ones should be sent directly to him via U.S. mail or telepone. He will see the public ones at the end of the month when I send him via diskette the logs of December traffic.--Gary Cziko)

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Topic: TOWARD A CSG NET DIGEST (?)

Date: December 1, 1990 (Happy Holidays!)

From: Greg Williams, Rt. 1, Box 302, Gravel Switch, KY 40328, voice (only) 606-332-7606

To: Everybody

I'm told that rumors are flying around the Net regarding the possibility of -- more or less -- concretizing the conversation into hard copy appearing at (semi?)regular intervals. Lucky me, I don't hear the rumors. But at least some of them are probably true.

History: Back in October, when the Net was just taking off, Bill Powers and Tom Bourbon and I talked about publishing a sort of patchwork newsletter stitched from the Net's conversational threads. I agreed to consider the feasibility of such an undertaking and, if the potential seemed high, to start it off with the understanding that Tom and perhaps other folks could provide aid or take it over as their time and other resources allowed. Now, at the start of December, having seen a couple of months of threads, I am quite convinced that a "Digest" is viable. Standing back from the hue and cry of the conversation, I'm quite impressed by its highly creative and substantive (both, of course, are important) content. And I believe that its quality will continue to flourish as more participate in the discussions. Some of you will recall my earlier enthusiasm

for a control theory journal -- an enthusiasm tempered by the realization that there wouldn't be enough articles per unit time coming from CSGers to support a journal. There is PLENTY of material on the Net for a "Digest."

Logistics: It would be reasonably easy for me to "desktop publish" a trial issue "Digest" to see whether or not it has an audience. I could pay for printing and postage of the initial issue with the idea that if people want more issues, they'll subscribe. If enough subscribe, I'll do more issues. What I'd need to continue to do it is to at least break-even on the first few issues, then gradually make profits (presumably by increasing circulation). The idea is to work toward a reasonable income for the Editor/Publisher. I'd send the first issue out pretty far and wide, to libraries and non-CSG individuals. Maybe I could do a little advertising promotion, too. At any rate, I'd be willing to write off issue #1 to experience if it flopped. Just what "flops" means, I'm not sure -- if everybody in CSG got the "Digest" (covered by membership dues?), I suppose I'd be happy for awhile and consider the extras pure gravy, but for the long term, we want to support someone (i.e., Tom B.) from this.
I can see starting off with two fairly thick issues each year, moving to more timely quarterly publication (each less thick, with about the same total pages annually) within a couple of years if all goes well. For an issue with 50 8.5"x11" pages, the printing and mailing costs would be around $4.00 -- so we could charge CSGers $5.00 per issue and non-CSGers $10.00 per issue and make out fine. Two issues per year: $10.00/$20.00. Going to quarterly would boost postage costs a bit, but I think we could hold annual subscription rates to $15.00/$30.00. That's a net of about $10.00 to $20.00 per non-CSG subscription, bringing in around $5000 to $10,000 total each year from a circulation of only 500, which I think is entirely reasonable within a few years... enough for a modest desert livestyle (hint, hint, Tom).

Format: Here there is a huge range of possibilities, as listed below.

TRIVIAL [for the editor/publisher] - Reproduction of excerpts from the log, unedited.

EASY - Reproduction of threads, edited for continuity.

HARD - Edited threads with clarifications added in consultation with the authors.

I would like to lobby for the HARD approach. For one thing, it would hold my interest -- I'd LOVE to pump you folks for extra info ("Whaddya mean, ----- ?!?!?!") and ruminations one-step-removed from the thick of the postings. And it would put some delayed feedback into the net (sounds wonderful for fostering reorganization, doesn't it?). AND it would provide significant "value-added" to the "Digest" for Net participants and non-participants alike. Of course, I would need cooperation from the participants -- in particular, timeliness in replying to my queries (pretend you're writing it for the net, then send it to me instead).

Last: "CSG Net Digest" is a lousy title.

I await (from a considerable distance, thank goodness, having no direct connection to the electrons and photons you hurl) your comments, questions, objections, and Loving Kindnesses.

From: Greg Williams, Rt. 1, Box 302, Gravel Switch, KY 40328, voice (only) 606-332-7606

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Date: Thu, 6 Dec 90 10:31:44 -0800
Reply-To: "Control Systems Group List (CSG-L)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group List (CSG-L)" <CSG-L@UIUCVMD>
From: marken@AEROSPACE.AERO.ORG
Subject: Philosoply

Wayne: That's what I meant to say! You just said it better. Models are a representation of what reality might be. We compare the predictions of our models (one kind of experience) to our perceptions (another kind of experience). When they match, we say that the model is our current best guess about the nature of the reality that causes our experience. Control theory just happens to be a model of that aspect reality that makes it possible to create models of reality (WOW!). Anyway, I'm sure glad I'm not a philosopher (I'm sure everyone else is too).

To Greg via Gary -- The journal would be a wonderful experiment. I agree that the "hard" approach to editing would be the best. I'd be willing to kick in a few bucks, perhaps as a surcharge on the CSG dues for 1991, to defray your expenses on the initial issue. I trust your editorial judgement implicitly and I promise that I would be responsive if you wanted clarifications of my posts.

Bill -- I tried several versions of the relationship control experiment. I can tell (based mainly on subjective experience, not real data) that I am not controlling the relationship between the dynamic variables; preservation of the relationship is a side-effect of my efforts to control some configuration. I'm sure that a simple, single level model controlling the state of some configuration (area difference, ratio difference, etc) can handle it, with the same 150 msec lag as that found with simple position control. But I have some other ideas for testing relationship control, which I'll tell you about tomorrow (if they work!). Any thoughts on this from anyone would be most welcome. I think investigation of control of "higher level" variables like relationships, programs, categories or even principles will be the kind of thing that gets the attention of the current trendy group of psychologists (if we want such attention -- a big if).

Best wishes to all

Rick M.

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213 336-6214 (day)
213 474-0313 (evening)

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Date: Thu, 6 Dec 90 13:37:00 EST
Reply-To: "Control Systems Group List (CSG-L)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group List (CSG-L)" <CSG-L@UIUCVMD>
Comments: Warning -- RSCS tag indicates an origin of SMTPUSER@UBVMSC
From: Hugh Petrie <PROHUGH@UBVMS.BITNET>
Subject: Re: Williams on CSG Net Digest

CSG DIGEST. I am enthusiastic about the possibility of a digest. The options outlined by Greg Williams sound correct and the cost very reasonable. I also vote for the HARD format. Sign me upd!

Hugh Petrie

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Date: Thu, 6 Dec 90 13:45:00 EST
Reply-To: "Control Systems Group List (CSG-L)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group List (CSG-L)" <CSG-L@UIUCVMD>
Comments: Warning -- RSCS tag indicates an origin of SMTPUSER@UBVMSC
From: Hugh Petrie <PROHUGH@UBVMS.BITNET>
Subject: Re: Philosoply

Rick and Wayne--Just one slight addition to the philosophy discussion. The relating of phenomena(models) to phenomena(data) would likely be considered a "coherence theory of truth" because its criterion is how well our various experiences cohere. At the same time if one adds almost any reasonable notion of evolution, I think that one can argue that coherent systems which do not at least roughly match reality will be weeded out. A realist can never know for sure that reality has been accurately mapped, defined, constructed, but somehow that doesn't then seem so important.

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Date: Thu, 6 Dec 90 14:34:51 EST
Reply-To: "Control Systems Group List (CSG-L)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group List (CSG-L)" <CSG-L@UIUCVMD>
From: "Barry A. Edelstein" <U21B4@WVNVM.BITNET>

Gary, I am afraid just cannot keep up with the volume of e-mail from the csg group, although I have certainly enjoyed much of the dialogue. At this time I would like to become disengaged from the network and would appreciate your indicating the commands I would use to do so. Thanks for the help and the opportunity to learn more about what others are doing in the control area.
Barry Edelstein
U21b4@WVNVM

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Date: Thu, 6 Dec 90 14:38:26 EST
Reply-To: "Control Systems Group List (CSG-L)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group List (CSG-L)" <CSG-L@UIUCVMD>
From: "CHARLES W. TUCKER" <N050024@UNIVSCVM.BITNET>
Subject: Re: epistemology; eye movements in the dark
In-Reply-To: Message of Wed, 5 Dec 90 21:58:00 CST from <TJ0WAH1@NIU>

Read George Herbert Mead's "Pragmatic Theory of Truth" (1929) and you will have the theory of "truth" that I follow which is not similar to any of those you have described; truth is a solution to a problem. No problem = no research
= no solution = no truth.

That is it.

I don't understand your reluctance to issue a Manifesto since we already have one - it was issued by Bill several years ago and can be found on file on the Cybernetics and Systems list that Cliff does.

I was right - Wayne did answer the epistemological post. i like that don't you

Chuck

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Date: Thu, 6 Dec 90 15:16:12 EDT
Reply-To: "Control Systems Group List (CSG-L)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group List (CSG-L)" <CSG-L@UIUCVMD>

From: Cliff Joslyn <cjoslyn@BINGVAXU.CC.BINGHAMTON.EDU>
Subject: Re: Philosoply
In-Reply-To: Message from "Hugh Petrie" of Dec 6, 90 at 1:45 pm

> Rick and Wayne--Just one slight addition to the philosophy discussion.
> The relating of phenomena(models) to phenomena(data) would likely be
> considered a "coherence theory of truth" because its criterion is how
> well our various experiences cohere. At the same time if one adds
> almost any reasonable notion of evolution, I think that one can argue
> that coherent systems which do not at least roughly match reality will
> be weeded out. A realist can never know for sure that reality has
> been accurately mapped, defined, constructed, but somehow that doesn't
> then seem so important.

Well, I AM glad I'm a philosopher. . .

And I agree completely with this version of the model/modeled relation.
But note: this leaves us saying NOTHING about reality. Coherence of the
output of the model with the input from perception does not entail that
the model is "correct" with respect to "reality".

But also: this does NOT say that "the model creates reality", or that
"we create reality" with our models, or any of the other seeming
foolishness that the radical constructivists enjoy. This is an AGNOSTIC
view, that all we have is perception and the outputs of models
(otherwise called knowledge). Any view that either of those relates to
"reality" is based on nothing but faith.

If anybody really cares about what I I'm talking about, I could email
you a more detailed article about our cybernetic epistemology.

O-->
| Cliff Joslyn, Cybernetician at Large, cjoslyn@bingvaxu.cc.binghamton.edu
| Systems Science, SUNY Binghamton, Box 1070, Binghamton NY 13901, USA
V All the world is biscuit shaped. . .
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Date: Thu, 6 Dec 90 15:01:19 EST
Reply-To: "Control Systems Group List (CSG-L)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group List (CSG-L)" <CSG-L@UIUCVMD>
From: "CHARLES W. TUCKER" <N050024@UNIVSCVM.BITNET>
Subject: Digest and Language

I think the Digest idea is a great and I would vote for the HARD approach
for the same reasons that Greg mentions. I agree the "Digest" is a lousy
title but we should have a "contest" for a title with the winner mentioned
only on the first issue of the document and not noted thereafter. I just
thought of one: "Collective Cybernetic Conversations" (can be called CCC not
to be confused with CC or "Three C's" as contrasted with "Two C's")

On lanaguge. I suggest the we follow the suggestions of Dewey and Bentley in
"Knowing and the Known" (which is also pragmatic) and eliminate centain words
from our talk. The word top on my list as it was on theirs is the word
'reality' and all of its forms, variations and synonyms. I would like to see
someone have a conversation without that word. This would be a great advance
toward a constructionist epistemology.

After eliminating the word 'reality' then we can work on eliminating all words
which even imply dualist experiences. Try this and see what advances are made
toward a constructionist epistemology.

WARNING: The above should only be tried by serious students of living control
systems not by beginners or those without serious reference levels.

I must return to the revolution I am doing here. More later. Chuck

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Date: Thu, 6 Dec 90 15:27:36 EST
Reply-To: "Control Systems Group List (CSG-L)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group List (CSG-L)" <CSG-L@UIUCVMD>
From: "CHARLES W. TUCKER" <N050024@UNIVSCVM.BITNET>
Subject: Some more on language

We should all try to be very careful about our input when reading words. I say these two sentences are NOT the same: "We create our world" "We create reality". I would never use the word 'reality' except to say the the word should be eliminated from ones vocabulary if it is your purpose to fashion a constructionist epistemology. It may not be eliminated from the cybernetic epistemology and that would be one of the differences between the two epistemologies. I bet there are more but this one is good enough for me.

Chuck

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BITNET: N050024 AT UNIVSCVM

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Date: Thu, 6 Dec 90 09:38:20 -0600
Reply-To: "Control Systems Group List (CSG-L)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group List (CSG-L)" <CSG-L@UIUCVMD>
From: g-cziko@UIUC.EDU
Subject: Re: epistemology; eye movements in the dark

Wayne Hershberger:

>M. J. Steinbach (in several different studies) has
>investigated the ability of persons to look toward their hand in
>the dark. They do so very well. Also, A. A. Skavenski found that
>persons maintaining a direction of gaze in the dark will actually
>compensate a bit for disturbances such as the one Bill Powers was
>describing in his recent posting: Bill was describing another one
>of Skavenski's experiments.

When I tried this informally on myself in the dark, it seemed as if I could also maintain a given direction of gaze. So now I don't know what Bill Powers meant when he said that there is no oculomotor feedback. How can there be control if there is no feedback? In fact, when I asked Bill this summer what data would refute his theory, he said it would be the demonstration of control without feedback in the presence of disturbances. So I suppose if Wayne is right about eye control, and Bill is right about no oculomotor feedback, then control theory has been refuted. In that case, I suppose I should pull the plug on CSG-L and go on to other things (but to be fair I'll give you guys a couple of days to convince me to keep the plug in)!--Gary

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217/333-4382

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of Educational Psychology
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1310 S. 6th Street-Room 230 Bitnet: cziko@uiucvmd
Champaign, Illinois 61820-6990
USA

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Date: Thu, 6 Dec 90 15:21:04 CDT
Reply-To: "Control Systems Group List (CSG-L)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group List (CSG-L)" <CSG-L@UIUCVMD>
Comments: Please Acknowledge Reception,Delivered Rcpt Requested
From: RLPSYU08 <TBOURBON@SFAUSTIN.BITNET>
Subject: FEEDBACK AND EYEBALLS

Gary Cziko,
Close your eyes, then roll your eyeballs around for a while. Notice any "feedback?" Not all feedback is "muscular." (For that matter, keep your eyes open and roll them around.) Still tempted to pull the plug?
Many people like to use the special case of the oculomotor system to refute a role for feedback of any kind in any system. That is probably a mistake.

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Dept. of Psychology
Stephen F. Austin State Univ.
Nacogdoches, TX 75962 Ph. (409)568-4402

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Date: Thu, 6 Dec 90 13:44:34 -0800
Reply-To: "Control Systems Group List (CSG-L)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group List (CSG-L)" <CSG-L@UIUCVMD>
From: marken@AEROSPACE.AERO.ORG
Subject: Philos Sophia Ocularis

Gary -- Don't pull the plug yet. Even if eye fixation in the dark is really an example of control (achievement of reference state in context of random disturbance) without feedback (and I seriously doubt it) we should probably delay throwing out control theory before we have a better explanation of that phenomenon as well as all the rest that control theory does handle. So I think it might be nice to try to think of a model that will do what you claim that the eye fixation system does (and, of course, does not use feedback, since the eye supposedly doesn't). This is an important step in science. The phenomenon of control creates a real problem for all input-output models. Control theory takes care of the problem by explaining control in terms of circular causation -- input control. The phenomenon of control without input regarding the controlled variable would certainly cause a problem for control theory. So what's the new theory? It can't be input-output theory because it can't handle the phenomenon of control itself.

I think the "dynamical attractor" theories are trying to do what you believe the eye can do when fixating in the dark -- they reach an equilibrium point and return to this point after disturbance. But they don't really control. Does the eye really control fixation in the dark? Does is resist a CONTINUOUS disturbance to its position? If not, the claimed disturbance resistance may be no more than dynamic equilibrium.

Best Regards

Rick (still a control theorist) Marken

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213 474-0313 (evening)

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Date: Thu, 6 Dec 90 19:05:41 CST
Reply-To: "Control Systems Group List (CSG-L)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group List (CSG-L)" <CSG-L@UIUCVMD>
From: Bill Powers <FREE0536@UIUCVMD.BITNET>
Subject: Too many things to enumerate

Lynne Oetjen-Gerdes:

Welcome, and hello to George. Would you ask him to propose some dates in February or March? I expect to be busy moving in April or May, so we have to coordinate. He might want to do this before the rates change (we're moving to Durango, CO).

It's nice to have an anthropologist on the net. The CSG has only one anthropologist (retired), and he never attends meetings (P. J. Bohannon). I taught a course (on guess what) for his students for one quarter in the early 70s, at Northwestern.

I hope that Chuck Tucker and Clark McPhail will engage you on the subject of "socially constructed" reality. How does a society manage to do that, when it has no perceptions, no brain, no means of acting, and no intentions? (Just a hint of what you're in for).

Brad Keeney uses control-theory ideas in his work; I've met him. I'll let someone else comment on Erickson et. al.

Before you jump on "constructivism" remember that the control-theory view is not based on anyone's philosophical position, but on a model of how the brain works. The term "constructivism" is only an approximation, and probably carries much other baggage with it that would not be needed under the control-theory view. If the brain works as all the evidence seems to indicate, it has no way of getting information about the outside world (or its own body) except from electrochemical sensory endings. Sensory endings report only the intensity of local stimulation -- in fact, only the state of the sensory endings themselves, however they got in that state. I claim that we have no way of determining the true state of the world by examining only the states of sensory endings, as the brain must do. Those states carry AMBIGUOUS information. The world that affects sensory endings has more degrees of freedom than there are sensory endings.

Finally, when you say "model" do you mean a working model, i.e., a simulation like those that George Richardson constructs? You're going to take some flak in this forum if that isn't what you mean.

Cliff Joslyn:

Your response to criticism is graceful.

Have you noticed that when someone admits to one tiny fault, the next thing that the accuser does is to say, " ... and furthermore --"? Well, I won't do that. There are lots of things in Newsletter 0 with which I could take issue, but I think that when you have time to grasp the structure of CSG-type control theory, you will reach the right conclusions and make the appropriate revisions yourself.

Hugh Petrie & Education:

We need to spend more time thinking about processes of change. As I've defined reorganization, it can't be directed by any systematic procedure toward any predetermined goal. It's a random process. Maybe we could separate the reorganization effect from what follows it: trying out the

result of the reorganization. Reorganization, generated completely by the organism, produces new possibilities of perception and action. But then something else has to evaluate the result and see if it actually works better (i.e., cures the problem) or makes matters worse. If it makes matters better, it should be explored further without any more reorganization. If not, another reorganization is called for immediately.

I have guessed that the site of reorganization in the brain follows attention. And we do know that through communication we can influence other's people's focus of attention. Does this suggest something about teaching?

My scenario for acquiring a new control system is as follows. First you have to develop the ability to perceive something new. Then you have to experience the world in this new way and record the new perception in many different states. Then you select (or maybe a teacher suggests) one state that might be interesting to recreate. Then you learn how to translate the difference between the current actual state and the selected reference state into an action that will make the difference smaller. Finally, you try different reference states and practice until each error always leads to an action that leads perception toward the specified state. All these stages, it seems to me, involve reorganization.

One of my earliest insights was that the problem of control is the problem of repeating a result in an environment that is not the same as it was the first time. Repeating a result implies remembering it. Learning to control in a variable environment means learning to convert error into action, not learning any specific action. The conversion rule is what we learn (although probably not consciously). Shouldn't this apply directly to education?

Maybe the evaluation part can be taught. I don't see how the error signal that starts reorganization can be taught. Maybe the real problem is that we don't know what students value. Some of them are satisfied in a fundamental way by seeing a proof come out right. Failure to find a proof is then quite sufficient to kick off reorganization. But another student is only happy when pole-vaulting higher than the last time. That student can fail to find a proof in geometry class and won't feel the slightest personal disappointment (except about a grade) and won't start getting new ideas. But kick the bar on the way over...

I think Gary said something a week or so ago about the input-output orientation of education. Apply the procedures known to be effective for teaching reading (input) and you will spew out students who can read. If we try to develop ways of finding out what students value, we ought to be able to find out how to teach ONE student how to acquire skills that will accomplish what THAT student needs to be accomplished. Or would that require restructuring our whole society?

Not a bad idea.

Wayne Hershberger:

When subjects "look at their hands in the dark," does this involve JUST eye movements, or can they turn their heads, too? There ARE proprioceptive

sensations of head direction. Also, a hand subtends roughly 10 degrees at arm's length, so "looking at a hand" doesn't imply very precise control. Even saccades do better than that, don't they? You can get SOME sense of

eye direction from sensations in the eyelid and sclera. Does received wisdom still say that there are no stretch receptors in the eye muscles?

HUGH GIBBONS describes control theory to his students as the study of "self-animating systems."

Help! I turn my back for five minutes and I'm three threads behind!

The epistemology seems to be in good hands so I pass this time.

Greg Williams: The hard way, of course. Mary will look at the CSG books to see h

Eyeballs: Rick, I believe has the right idea. (Wayne can check up on this). When a person holds a constant eye position in the dark and you pull on that thread attached to the stalk on the contact lens, the eye deviates as if on a spring, the spring constant being that of the lateral rectus muscles. So there is no active (control-type) resistance to the disturbance. That only happens with the lights on. Also, the subject does not feel the eye deviating. So there is still no control without feedback, as far as I know. Is it all better, Gary? Hello? Gary?

Some day I'll publish my model of oculomotor control (as I said, Greg has all my materials). It's possible to show that the position sensed is the INTENDED position, with reflex adjustments NOT being sensed. This is a pecularity of the combined tendon and stretch reflexes, under the assumption that voluntary action takes place through alpha efferents and reflex adjustments through gamma efferents. The reason I haven't published, some 8 years after the model was finished, was that there aren't any stretch reflexes in the eye muscles (or so said my last look at the literature). The basic parts of this model are incorporated into the Little Man arm-control demo version 3, which languishes on a back shelf now and for a while yet. I won't publish until I figure out whether there's a way to get around the lack of muscle-length feedback in the eye. If there isn't, I'll probably junk the eye model and look for similar phenomena in the arm model.

Best -- Bill

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Date: Thu, 6 Dec 90 19:53:19 CST
Reply-To: "Control Systems Group List (CSG-L)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group List (CSG-L)" <CSG-L@UIUCVMD>
From: Bill Powers <FREE0536@UIUCVMD.BITNET>
Subject: Pragmatism; relationships

Chuck Tucker:

The "problem" orientation sounds good at first, but when you think of all the kinds of problems people are interested in solving it seems to merge back into the way science is done anyhow.

For example, suppose that the problem you want to solve is how to talk to people in Europe from the United States without wires. What you end up with is Maxwell's equations, electrons, inductance, radiative efficiency, and all that theoretical stuff that we like to think is REALLY OUT THERE. I think your approach sounds best when you're thinking about particular problems that would interest a sociologist.

There's a difficulty in doing away with the notion of Reality. Take the control-system model. Just trying to solve the problem of how people manage to behave the way they do, we can put together a control-system model that

doesn't mention anything going on inside a person. But that sort of model is good only for one behavior at a time. What we want to know is HOW A HUMAN BEING IS REALLY ORGANIZED INSIDE, so we can generalize to situations we haven't stumbled across empirically. This means quessing at how the nervous system might be hooked up and what functions its parts might perform. The wierd thing is that when you put together a plausible model of the reality inside the skin according to the easiest way to construct a working control system, and then open up the organism and start tracing circuits, you find pretty much the connections and functions that the model suggested. So the model, that only tells us what MIGHT underlie observation, leads us to discovering that it pretty much told us what DOES underly observation. We're still in the world of observations, of course, but what prickles the back of my neck is that the model predicts details of observation before we observe them. It gets pretty tempting to conclude that the model may be telling us something about what's actually there, and not just about experience. Sometimes you come up with the answer to a problem, in this way, before you even realized there was a problem.

To me, the concept of reality is simply the assumption that there's more here than meets the eye. We may never know it directly, but thinking it's there keeps us trying to peek behind the scenery to find the machinery.

Rick Marken:

What we need is a relationship that isn't tied to ONE PARTICULAR configuration, but is achievable through many different configurations. That way you can disturb configurations in one way that doesn't alter the relationship, and a different way that does. Or are you already working on that track?

Best -- Bill

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Date: Thu, 6 Dec 90 20:58:06 CDT
Reply-To: "Control Systems Group List (CSG-L)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group List (CSG-L)" <CSG-L@UIUCVMD>
Comments: Please Acknowledge Reception,Delivered Rcpt Requested
From: RLPSYU08 <TBOURBON@SFAUSTIN.BITNET>
Subject: Paper calls and relationships

FOR ALL MODELERS AND DATA GATHERERS: I have two paper calls, one from American Psychological Society, the other from International Society for Theoretical Psychology. APS has a deadline of 25 January for a meeting 13-16 June 1991, in Washington, D.C.; ISTP, a deadline of 1 February for a meeting 24-28 June 1991, at Clark University, Worcester, Massachusetts. Both organizations invite individual papers (poster sessions, for APS) and proposals for symposia.

If anyone desires information about either meeting, I will forward copies of the calls. If any of you are interested in participating in a symposium on control-theoretic research, or modeling, or both, I am willing to coordinate the development of a proposal. If you are interested, let me know, and give me your preference for which meeting. (Or for both?)

BILL POWERS and RICK MARKEN:Help me out. I thought I knew what relationships were -- at least in some instances: things like "over" and "under," whether by a smidgen, 10 cm., or 30,000 feet, Now, when I put the middle (relationship?) of three marks 15 cm above (relationship?) the outer (relationship?) two, the result is an apparent configuration -- an approximation of a triangle. When I put all three "even with one another" (relationship?), the configuration is of a line. To preserve one of those configurations, I must maintain the relationships

among the constituent lines. So which is the "lower level" perception, relationship or configuration? Is there an element of conflation here? Also, each of the marks I am describing is is itself a small line -- a configuration.
    If each of the elements we manipulate in a trackiing task is itself a configuration, then a particular arrangement of them, selected by whatever criterion and defended against whatever disturbance, seems to be a higher-order "config-uration" -- a relationship? Where am I off?

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Date: Fri, 7 Dec 90 12:50:51 +0800
Reply-To: "Control Systems Group List (CSG-L)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group List (CSG-L)" <CSG-L@UIUCVMD>
From: mark@CS.UWA.OZ.AU
Subject: Re: Brooks' and Powers'

Chung-Chih Chen:

> It seems that your Level 4 corresponds to Brooks' behaviors. Each
> behavior is a control process (what you called), right?

There seems to be some misunderstanding. I define a hierarchy of navigational behaviors in the same way that Brooks defines his levels of competence. Therefore my behaviors are very similar to Brooks, though not entirely the same. In the following I'll summarize the Brooks approach and present an overview of my approach to try to highlight the differences.

Brooks' Subsumption Architecture is layered where each layer provides the additional control necessary to enhance the existing architecture so that a new level of competence may be realized. A new layer of control architecture subsumes the existence of functions in lower layers. However, a new layer duplicates some of these functions in lower layers and so there exists a conflict between these duplicated functions. The higher layer resolves this conflict by connecting to the communication lines of the lower layers and interfering with the flow of data and in effect inhibiting their operation.

My control system has a completely different structure to the Subsumption Architecture. Each of my behaviors is implemented by a network of control processes (I actually call them `agents') which are organized according to a well defined control infrastructure. This infrastructure has various dimensions of which data abstraction is only one. Given an existing control system, the implementation of a new (i.e. the next) behaviour involves adding some new agents to address new aspects of control. All existing agents are still necessary for this new behaviour though some of them will require their control algorithm to be modified so that they can function in a manner that is consistent with the new behaviour. Thus agents consist of a set of control algorithms called skills where each skill is applicable to at least one behaviour. An agent utilizes the skill which corresponds to the current behaviour being exhibited by the robot. If the control system is not operating at the highest behaviour to which it has been developed then only a subset of the existing agent network will be active. If the robot changes the level of behaviour which it is exhibiting then the active part of the agent network will also change so that only those agents which possess a skill relevant to that behaviour will be active.

> The control processes may use (share) Level 1 (or 2, or 3).

A behaviour requires agents that reside within various abstraction levels.

An agent can exist in only one level but may interact with other agents that are in the lower, the same, or higher levels. Remember that agents are grouped along with the data models that they use to form a level of abstraction. An agent can only access data models that reside in the same level.

> So it's not as redundant as Brooks'.

This is correct. A higher behaviour utilises all existing agents as well as the new ones introduced specifically for it. A small redundancy exists in the set of skills contained by each agent. But this redundancy is the strength of my system as they are used in the context of adaptation. That is, if the skills for the current behaviour fail to generate a satisfactory response then the skills of lower behaviours may be used.

> Does your work have anything to do with the cognitive map of W. K. Yeap (also from NZ)?

I have a copy of this paper "Towards a Computational Theory of Cognitive Maps".
I was intending to use cognitive maps that are described in this paper to form the basis of the navigation techniques required by the highest of my navigational behaviours. But time restrictions on my PhD have caused me to stop researching and complete my thesis. Tod Levitt's work on "Qualitative Navigation" and D. Zipser's work on "Biological Plausible Models of Place Recognition and Goal Location" are others attempting to construct maps based upon perceptions of `landmarks' and `places'.

I hope this helps :-)

Mark.

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       _
      o     Mark Nelson
     < -    PhD Student
     / >    Computer Science Department
    '  ~    University of Western Australia
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Date: Fri, 7 Dec 90 07:22:00 CST
Reply-To: "Control Systems Group List (CSG-L)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group List (CSG-L)" <CSG-L@UIUCVMD>
From: TJ0WAH1@NIU.BITNET
Subject: Epistemology; eye movements

EPISTEMOLOGY:

Hugh--- Exactly! Science employs a coherence theory of truth. It is only the coherent (or replicable) phenomena which must be matched by the phenomena our models (theoretical constructions) generate. For example, I recall one of us--I think it was Bill-- recently explaining that it is a waste of effort to try to model a phenomenon that is replicable only statistically.) The more coherent or replicable the phenomenon (across moments in time, positions in space, type of observer, etc.) the greater the truth value of the match. Good theories or models not only generate data which match previously replicated phenomena, they also "conceive," "embody," "define," or otherwise suggest the possibility of even more coherent phenomena, as yet unrealized by us. As long as these "new" phenomena prove to be ever more coherent we tend to regard both ourselves and our models as being "on track" in the quest for truth. This quest for truth, however, is not a tropism toward some supposed REALITY; the reference value for our quest is coherence (i.e., replicability and parsimony). As you imply: REALITY (noumena), who needs it? All science needs is phenomena--and a

good reference signal.

Rick: I agree. I believe that we are both trying to say the same thing. However, as I would say it, theoretical constructions, or models, are NOT representations of a REALITY. Theories are presentations not re-presentations. Theories are abstractions for which there need be no ontological counterparts. Further, if and when an ontological counterpart is observed, that counterpart is but another phenomenon, not a noumenon. The computer models you are developing simulate or re-present living organisms only in the sense that the data (phenomena) they generate correspond to some of the data that those organisms provide; your model is a presentation; only your model's DATA is a re-presentation (of the data provided by the simulated organism).

Cliff: I believe I share your epistemological perspective exacltly, including your antipathy for the "seeming foolishness that the radical constructivists enjoy."

Chuck: Did I get it right?

EYE MOVEMENTS:

Gary--Touche! The matter is in fact controversial. I tried to finesse the matter by saying that the oculomotor system controls for disturbances in the dark "a bit." Perhaps I should have said "almost not at all." The oculomotor control system is generally recognized as being a closed-loop control system, but one which uses estimation of the plant as feedback. Skavenski's isolated finding is a paradox.

Warm regards, Wayne

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Date: Fri, 7 Dec 90 09:36:36 -0600
Reply-To: "Control Systems Group List (CSG-L)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group List (CSG-L)" <CSG-L@UIUCVMD>
From: g-cziko@UIUC.EDU
Subject: Refuge from the Deluge

Everybody:

I arrived in my office this morning before 8 and it's now well after 9 having done nothing other than go through my electronic mail, almost all of it from CSG-L! Included in my mail were messages from two subscribers asking (begging?) to be cut loose from the network because of the sheer volume of words it spews forth.

It seems to me that this amount of recent traffic is a problem for all but the most enthusiastic. It is therefore a quite welcome development that Greg Williams is offering to pre-digest what is happening here and publish it in a more manageable, if somewhat less interactive, form. I say to Greg, go for it and see what happens (the hard version, of course; what's hardest for you will be easiest for the readers).

For those of you who are interested in control theory but starting to drown, Greg Williams may your way of keeping up without dedicating hours each week reading your email. Let him know if you'd be willing to

subscribe to the Digest. Greg is a priceless CSG resource. Let's exploit him to the fullest.--Gary

P.S. For those of you who may want to be cut loose from CSG-L but still kept on a "supplementary" mailing list for info on developments such as conferences, new important publications, and the "Digest," make sure you let me know. I am creating a separate list of such people to whom I will attempt to forward only such items.

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Date: Fri, 7 Dec 90 11:59:31 -0600
Reply-To: "Control Systems Group List (CSG-L)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group List (CSG-L)" <CSG-L@UIUCVMD>
From: g-cziko@UIUC.EDU
Subject: Estimated Feedback

Wayne Hershberger:

>The oculomotor control system is generally
>recognized as being a closed-loop control system, but one which
>uses estimation of the plant as feedback.

I'm not sure what you mean by "plant," but if you consider a system "closed-loop" because it uses estimation as feedback, this makes the terms "closed-loop" and "feedback" pretty meaningless to me. How does "estimated feedback" allow the system to achieve control in spite of unpredictable disturbances? How is basing behavior on "estimated feedback" any different from computing output without feedback?--Gary

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Date: Fri, 7 Dec 90 13:32:54 EST
Reply-To: "Control Systems Group List (CSG-L)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group List (CSG-L)" <CSG-L@UIUCVMD>
From: "CHARLES W. TUCKER" <N050024@UNIVSCVM.BITNET>
Subject: Re: Refuge from the Deluge
In-Reply-To: Message of Fri, 7 Dec 90 09:36:36 -0600 from <g-cziko@UIUC.EDU>

I REALLY HATE TO MENTION ANOTHER PROBLEM BUT I DON'T SEEM TO GET THE MESSAGES THAT I SEND TO THE LIST BACK TO MYSELF RATHER I GET SOME MESSAGE THAT I SENT A MESSAGE TO A NUMBER OF PEOPLE. I ONLY ASK BECAUSE THE REFERENCE LEVEL I AM U
SING DOES NOT SPECIFY THAT INPUT AND THE MESSAGE GETS TREATED BY ME AS A DISTUR
BANCE AND MY RESOLUTION TO THIS PROBLEM IS TO WRITE YOU AN ASK WHAT IS GOING ON
CAN YOU HELP ME HERE? I HOPE SO.

CHUCK

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Date: Fri, 7 Dec 90 15:36:18 -0800
Reply-To: "Control Systems Group List (CSG-L)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group List (CSG-L)" <CSG-L@UIUCVMD>
From: marken@AEROSPACE.AERO.ORG
Subject: Philosopy,Relationships, Equilibrium

Gary -- I'm puzzled about why people want to drop off the list. Are we posting too much? Are we supposed to post at a rate that we imagine to match the reference rate of others on the list? It seems like, when you sign up to get mail, you get it. You can scan through articles to see if you really want to waste any time on them. But how can there be too much mail? I just don't get it. Clearly, something about the mail is a disturbance to some variables these folks are controlling. Perhaps they are really controlling for the amount of mail. If so, that amount can be cut down by de-subscribing to the sources of mail. Then one wonders why this source in particular is selected. Again, it might be because this is perceived as a source of a lot of mail. That's fine. I guess, since this is the only list I subscribe to, the amount of mail seems quite low, actually. But, then, this is just another example of people controlling their own perceptions. It is hard to avoid the inclination to say "you are perceiving things wrong" or "you have the wrong reference for that perception" (the amount of mail perception). I have to force myself to remember that, what I really mean is that these things are "wrong" only with respect to my own control structures.

Wayne, Chuck et al -- I guess I go along with Bill on this reality thing. His example was the best -- when we make models they often predict not only other observations but observations that are parts of the model itself. What could be more startling than the fact that we are now getting scans of the atomic components of matter. The pictures taken with these super duper electron microscopes show things that look a lot like the little spheres that we imagined atoms to be like. I know that the pictures are just another perception and there is coherence between one set of perceptions and another. But it seems like there is more; there is some kind of reality that our models are capturing. Why the desire to dispense with reality anyway? -- just because we're always isolated from it does not mean that we are not getting some strong hints and approximations to its nature. It's like my shadowbox analogy -- we'll never know what the reality behind the screen "really" is; but we can do experiments on it, get consistent results and model it more and more accurately. And there is something that is really behind the screen. But, unlike with a shadowbox, people cannot go around to the side of their perceptions and see if they got it right.

Ultimately, does it matter whether or not we assume a reality out there? Well, I kind of think so but I don't mind if people think that they are just modeling the internal consistency of their own experience -- as long as their modeling helps me understand the cause of my own experience; I'm happy to call that cause "reality"

On pragmatics: One of the nice things about control theory is that it is a model of modelers (as is cognitive psychology) that explains why they might want to do modeling in the first place. At the moment, the only kind of organization that we know of that can want to try to understand the cause of
it's own experience is a control system -- this is independent of whether or not it is capable of doing so. Cognitive psychologists seem to assume that people will carry out programs and look for relationships (that is, do science) just because they are smart (cognitive). Well, a computer is pretty smart but it doen't want to understand anything -- it is not organized in a way that allows it to want anything. So my understanding of doing science is quite pragmatic, simply because I imagine that scientists, like other people, are control systems.

Tom and Bill -- On relationships. I think that a configuration is still a configuration even if it is made out of simpler components. In the tracking tasks you can see (and control) either a configuration of a relationship given the same display. The problem is detecting (from the "outside" so to speak) what another person is actually controlling.
For example, I can keep a line "next to" another line (or "under" another line). These are relationships. You can subjectively experience them as relationships or as configurations. The problem is that when you disturb the relationship you also disturb the configuration and it's tough to separate them.

I almost have a pure relationship control experiment. I have two dots moving around the screen in a circle. The subject is to keep the rate of movement of one dot equal to the rate of movement of the other. The subject's handle influences only the rate of movement (not the position) of one dot. The subject can control the relationship between rates without any configuration information because the phase of the two dots is always changing -- so when I am keeping the rates equal there is no stable configuration on the screen. I am probably just controlling rate -- not relationship yet. But there is still a problem -- the subject can turn my task into a configuration control task because it is possible to speedup the variable dot until it is "even with" the constant dot. (The disturbance, by the way, is to the rate of movement, not the position of the variable dot). So I can keep one dot a fixed distance from the other and then it turns back into a configuration control task. So I'm still working on it.

I have yet another idea for control of relationship -- I will report next week if it works. But I think I'm starting to get a feeling for this hierarchical perception stuff. I actually experience the relationship that exists between two dots moving "in phase" or "out of phase" on the screen. The perception of this relationship is different than the perceptions of the movements of the dots (transitions) and the distance between the dots (configuration).

One last point -- I just got the little not from Jay Mittleman(sp?) about feedback and dynamical systems. It made me realize that it is important to distinguish control not only from cause but from dynamic equilibrium. I think many psychologists imagine that a system that returns to a particular state after a disturbance is a control system -- it ain't. The simplest example is a mass-spring system. A mass hanging from a spring will return to the same position after a transient disturbance (and after some oscillation). This seems like what a control system does -- the "equilibrium" position of the
mass-spring system being the "reference" state of the mass position. There are people who think muscle/skeleton systems are mass-spring -- muscle is spring, bone (and stuff) is mass. But a mass-spring system doesn't control. That is very easy to show. Just pull down on the mass and keep pulling. Now the mass stays in a new position. There is no return to equilibrium until you let go. This is not what happens with an arm. Point straight ahead. If someone "bumps" your arm the arm acts a bit like a mass on a spring, oscillating a bit and returning to the straight-ahead position. But if someone (or yourself with your other arm) apply a constant push to the pointing arm, it keeps pointing forward. The control system ACTIVELY applies just the right compensating force needed to keep the pointing arm pointing straight ahead (within the limits of the forces you can generate). So, control and equilibrium are completely different phenomena based on completely different organizing principles (both principles being well understood).

Well, that should overflow a few mailboxes.

Have a nice weekend. With any luck I will have a modem at home soon so I'll be able to stuff those mailboxs on the weekends too.

Regards,

Rick M.

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Date: Fri, 7 Dec 90 17:56:57 CST
Reply-To: "Control Systems Group List (CSG-L)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group List (CSG-L)" <CSG-L@UIUCVMD>
From: Bill Powers <FREE0536@UIUCVMD.BITNET>
Subject: meeting, hierarchy, epististuff, eye

Tom Bourbon:

I think that with the foot Rick Marken has sticking through the APS door, that might be the right place to give a symposium. By that time, Greg and I ought to be able to have a presentable Arm-Model package with which to confound the motor-program/coordinative-structure guys -- or at least present a serious alternative. And in Washington, D. C., Mary and I have a free place to stay (Mary's brother, a dean at GWU). Should be able to handle Greg, too, as he is perfectly content to sleep under a table.

Tom, and Rick Marken:

The configuration-relationship thing is a puzzler, but I think there are some solutions. The biggest problem, as I just realized last night (see my post) is that the relationship in tracking depends on parts of just one configuration. The measure of the relationship, relative distance from target to cursor, is the same as the measure of the configuration -- you can't change the configuration without changing the relationship and you can't change the relationship without changing the configuration. So there can't be any evidence of a hierarchical relationship even if it exists. The measured transport lag, 0.15 sec, points to a low-order system, which is consistent with my previous experience that a task is always done at the lowest level possible.

Maybe we just have to have more than one dimension of control to get what we want here. How about this? Given a circle centered on the screen. Given a dot that moves randomly in two ways: the angle of a line from the dot to the center of the circle varies under a smoothed disturbance, and the dot's radial distance from the center of the circle (regardless of angle) is affected by the one-dimensional handle position plus a test disturbance. The subject is supposed to keep the dot somewhere on the circle. Now there is a continually-varying relationship between the radial error direction and the direction the handle moves. You have to learn to distinguish between spatial position (configuration) and spatial relationship (I hope).

Mark Nelson:

I really like what you're doing. I think it may well introduce new principles. The hierarchy problems (see above) that we're having are probably caused by trying to get too much out of a structure that grew in one person's head without much in the way of experimentation. Now that we're trying experiments we're seeing difficulties. Your approach may help get us out of them. I know that you have only one overriding project right now, but I hope that eventually you'll be able to help us get sorted out here. Nobody here has any big emotional investment in any particular form

of hierarchical control, or even in hierarchical control per se. Our overriding project is to figure out how people work, not to defend models just because they're pretty.

When the PhD pressure is gone, let's see what model-merging we can do. You're going to have to take an academic job, of course, to maintain your network connection!

Wayne Hershberger, Hugh Petrie, Chuck Tucker, Cliff Joslyn:

I think a "coherent" grown-up statement of epistemology is taking shape. I want to play out my rear-guard strategy, though. Let's not forget the role of action in finding a coherent world-view. Coherence of interpretations alone isn't enough -- that leads to Scholasticism and the domain of Pure Reason. It's just as easy to construct a systematic delusion as a systematic model. Without bringing action into the picture, you can't tell the difference. At some point the model has to be used to specify an action, the action has to be taken, and its consequences have to be compared with what the model says the consequences ought to be.

From the third-person point of view, we have a model of the nervous system and a model of the physics of the world. Putting these models together, we see that the nervous system can experience only the indirect consequences of its own actions. Between its actions (outbound neural signals) and its perceptions (inbound neural signals) there are many detailed processes that are the subject-matter of physical models. The naive nervous system knows nothing of these intervening processes. It can, however, infer them, although it has no way of testing the validity of its inferences but experimental tests of models that it constructs within itself. It looks at relationships between directly willed perceptions (actions) and indirectly affected perceptions (consequences), and compares that with an imagined picture of what the relationships ought to be, according to a model.

With this picture that we get by constructing a model of an organism in a model of an environment, we can now move our point of view back inside the model of the person -- the first-person point of view. Now all we know are our actions (perceptions that infallibly and immediately obey the will) and perceptions that are consequences of acting. But the third-person model we have just left informs us that our actions affect the consequences through intervening processes. There is, according to this model, a level of process that we do not sense: the physics of the outside world. The links between perceived action and perceived consequence exist in a part of the world model outside sensory experience.

I think it's interesting that we can also switch to the physical-model point of view, and go through similar reasoning concerning what goes on inside the skin of another person: the second-person view. That's basically the way I started when I first got interested in control theory.

Maybe this kind of model-hopping is what Cliff means by a "metacognitive" level of thought. I think it gives us a sort of bootstrap conception of what we're trying to find out that isn't naive realist or naive anything, but something rich enough to be interesting. See? I didn't say "reality."

Actually, the real reality is what we're looking at, isn't it? All the rest is models.

Eyes: The eyeball is practically the only moving part of the body that is not subject (normally) to mechanical disturbances. The optical control systems don't need to verify that the eye moved. It always moves. So the control loops can skip the first order. I agree with Gary: no feedback, no control. Eye position doesn't need to be controlled; it follows the activating signal closely enough without it. The higher systems take up any

slop that's left.

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Date: Fri, 7 Dec 90 10:03:34 GMT
Reply-To: "Control Systems Group List (CSG-L)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group List (CSG-L)" <CSG-L@UIUCVMD>
From: Chung-Chih Chen <chen%arti1@VUB.VUB.AC.BE>
Subject: abnormal behaviors and positive feedback

Tom Bourbon, Gary Cziko, Bill Powers and others:

Last month Tom said something about positive feedback:
> I think a lot of the talk about benefits of positive feedback
> and about how you see it in arms races and evolution is as loose
> as the notion that, in a crowd, people go out of control -- an
> idea with which Clark McPhail deals most effectively -- it
> ain't so! Your musings on giraffes and trees and on prey
> and predators seemed right on the mark -- everybody was
> controlling all the while -- the things that appeared
> "out of control" as a result of positive feedback were
> variables that didn't matter to the controlling systems.
> Control systems will let ANYTHING vary, in any way, if at
> the "anything" is not something the system is controlling.

Bill said:
> Various commentators: I think we have a good case for saying that organisms
> work by negative feedback. In a collection of organisms mixed in with a
> passive environment, you can have relationships AMONG THE ELEMENTS of any
> sort: negative feedback, positive feedback, or no feedback. If you know
> that some elements of a "social" system are control systems, and understand
> the properties of the environment, then the kind of "social" feedback that
> exists isn't a matter of opinion. Solve the equations and see what kind it
> is. It could be any kind in any particular case. As to positive feedback
> and evolution: sure, maybe. But where's the test? (The same question
> applies to my guesses).

It seems that nobody agrees that there is positive feedback in our body
(I don't talk about evolution!). I want to ask how some people have some
abnormal behaviors, such as becoming madness or committing suicide?
My explanation is that some abnormal behaviors are the results of
positive feedback. Positive feedback drives some people to extreme
behaviors (madness, suicide etc.). This notion is not loose. It
corresponds to the property of positive feedback: Driving the output
(behavior) to extremity. Some people can not control themselves to
follow some references!!

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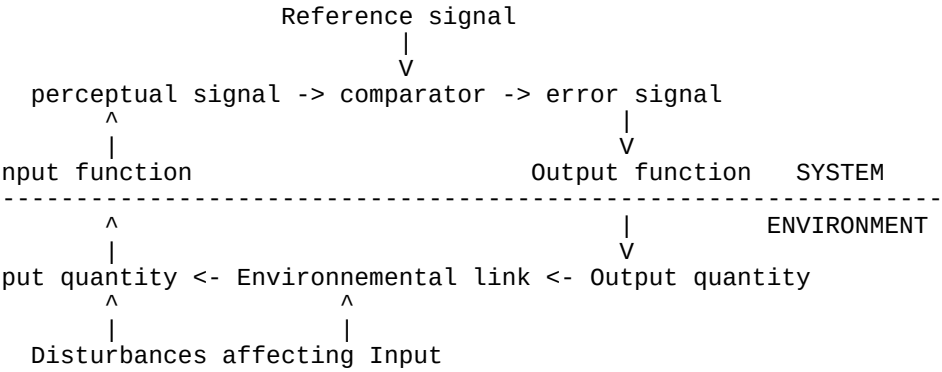
==========

Date: Sat, 8 Dec 90 08:48:26 CST
Reply-To: "Control Systems Group List (CSG-L)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group List (CSG-L)" <CSG-L@UIUCVMD>
From: Bill Powers <FREE0536@UIUCVMD.BITNET>
Subject: Replies, control map, reality

I'm going to try to shorten my messages to help make this more like a multilogue.

Chen: You're right: positive feedback, when it occurs in the body, is usually a sign that something has gone wrong. There are some normal positive feedback phenomena: e.g., sneezing, sexual climax, scratching an itch. Once the sensation starts you have to make it larger in order to stop it. But that's part of a larger negative feedback loop, isn't it? Also, oscillators can be part of control systems: breathing, walking, and various circadian rhythms. Negative feedback is involved in governing the amplitude and rate of these oscillators. Oscillators require positive feedback (nonlinear, if their amplitude is to be limited). But positive feedback, by itself, can't produce resistance to disturbances of controlled variables.

Izahk Bar-Kana: Here's my nomenclature for a living control system:



"Quantity" = physical variable outside system
"Signal" = physical variable inside system

"Output function" = effector
"Input function" = sensor + sensory computations

"Output quantity" = direct measure of effector action, not subject to external disturbances.
"Input quantity" = physical variable directly affecting sensor

"Disturbances" = Independent contributions to state of input quantity
"Environmental link" = Path by which effector contributes to state of input quantity.

The observable controlled variable is the input quantity. The output quantity varies as disturbances vary, and cancels their effects. Note that the reference signal originates inside the system.

Will you draw a map of your model for us? Will you indicate correspondences and differences in comparison to the above map?

Reality: a proposal for retaining a useful word.

What we experience and control are APPEARANCES. Our models tell us that

behind these appearances are PHYSICAL PROCESSES AND ORGANIZATION. Other people APPEAR to be stimulus-response organisms: they respond to disturbances. The control system model of the physical processes in other people tell us that they are REALLY control systems: they control inputs. "Real" is a term we apply to a model of underlying processes. "Apparent" is a term we apply to the observations that the model explains. So the dichotomy is apparent/real. This is what a physicist means by saying that a table-top is "really" mostly empty space. The implied follow-on is "whereas

it appears to be solid." We use "really" when we believe that appearances are misleading. It does not imply that the model is correct: only that it contains the best explanation of appearances in terms of underlying forms and processes. It tells us that the explanation of appearances is to be found at a level of organization other than that of appearances. "Real," in this sense, really means "imaginary" because all models are products of the imagination (whereas it appears to mean "objective").

Refinements, anyone?

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Date: Sat, 8 Dec 90 14:13:47 EDT
Reply-To: "Control Systems Group List (CSG-L)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group List (CSG-L)" <CSG-L@UIUCVMD>
From: Cliff Joslyn <cjoslyn@BINGVAXU.CC.BINGHAMTON.EDU>
Subject: Re: Replies, control map, reality
In-Reply-To: Message from "Bill Powers" of Dec 8, 90 at 8:48 am

> Chen: You're right: positive feedback, when it occurs in the body, is
> usually a sign that something has gone wrong. There are some normal
> positive feedback phenomena: e.g., sneezing, sexual climax, scratching an
> itch. Once the sensation starts you have to make it larger in order to stop
> it. But that's part of a larger negative feedback loop, isn't it? Also,
> oscillators can be part of control systems: breathing, walking, and various
> circadian rhythms. Negative feedback is involved in governing the amplitude
> and rate of these oscillators. Oscillators require positive feedback
> (nonlinear, if their amplitude is to be limited). But positive feedback, by
> itself, can't produce resistance to disturbances of controlled variables.

My understanding here is more from traditional cybernetics than from CST. But it seems to me that negative and positive feedbacks are mutually interlinked in all complex systems (e.g. organisms). Positive feedbacks are typical of action and growth; negative feedbacks of stability. Now in general the system in question must be globally stable or it will self-destruct and will not be observed. Hence negative feedbacks tend to dominate the higher levels of the systems that we end of studying.

Consider this general characterization: a positive feedback process begins which leads to exponential growth and some qualitative change. Eventually this change will appear as a disturbance to a higher-level system and be controlled through that negative feedback. An example in ecology is logistic population growth; in neurology in the development and propagation of the wave train; in practically all of embryology.

Now in the absence of that higher level of control, the system will destruct. But that is not to say that the positive feedback is any less "important" than negative, because in the absence of positive feedback there will be only global stability. Put another way, the positive feedbacks of lower levels can be the source of disturbances at higher levels; they appear as transients before leading to stable behavior.

O-->
| Cliff Joslyn, Cybernetician at Large, cjoslyn@bingvaxu.cc.binghamton.edu
| Systems Science, SUNY Binghamton, Box 1070, Binghamton NY 13901, USA
V All the world is biscuit shaped. . .

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Date: Sat, 8 Dec 90 22:33:26 -0600
Reply-To: "Control Systems Group List (CSG-L)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group List (CSG-L)" <CSG-L@UIUCVMD>
From: g-cziko@UIUC.EDU
Subject: Re: Refuge from the Deluge

Chuck:

>I REALLY HATE TO MENTION ANOTHER PROBLEM BUT I DON'T SEEM TO GET THE
>MESSAGES
>THAT I SEND TO THE LIST BACK TO MYSELF RATHER I GET SOME MESSAGE
>THAT I SENT
>A MESSAGE TO A NUMBER OF PEOPLE.

That's exactly how the network is supposed to work. So please re-organize and reset your reference level.--Gary

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Date: Sat, 8 Dec 90 22:51:42 CST
Reply-To: "Control Systems Group List (CSG-L)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group List (CSG-L)" <CSG-L@UIUCVMD>
From: Bill Powers <FREE0536@UIUCVMD.BITNET>
Subject: Positive feedback

Cliff Joslyn --

Most of the "traditional cybernetic" concepts of feedback were formulated by people who had only (to be kind) an intelligent layman's understanding of control theory and system analysis. As a result, they kept searching for "insights" about feedback that were based mainly on verbal symmetries. If negative feedback was important, then positive feedback must be important, too. If feedBACK was interesting, then why not feedFORWARD? So what you got was a lot of wild guesses and bold pronouncements based on not much of anything but tricks of rhetoric. Of course positive feedback and feedforward do have meaning to engineers, but nothing like the meanings that were bandied about by cyberneticians trying to prove how bright they could be without actually studying the subject matter.

I think that it's moderately well known now that growth WITHIN an organism is not simply an exponential runaway process, but a tightly controlled progression. Cells in a healthy growing organism don't just proliferate until they start to be poisoned by their own waste products. Each cell in the zygote can, potentially, become any kind of tissue. But as control processes regulated by genes (this is my interpretation) detect a lack of concentrations of certain controlled variables, they turn on and begin raising the level of the variables. When enough cells are producing enough of a given kind of substance, the rising concentration begins to shut down some of the genes with lower reference levels, so that specialization begins. The geometry of cellular arrangements in the zygote probably has a lot to do with which genes in which cells detect a lack and produce more output and which detect a surplus and shut down. And there are probably mutually-inhibitory processes like those in the retina that favor the differentiation.

Well, that's my fairy tale, but the point is that we don't HAVE to let positive feedback into this picture at all. I would have to see some real examples before I'd even consider it. We know of plenty of negative feedback processes at the genetic level. But I can't see how positive feedback could do anything but disrupt growth and maturation.

When you start talking about whole organisms as elements of a system, the

story changes. Yes, whole organisms DO proliferate until they start to drown in their waste products and exhaust their food supplies. There are all sorts of feedback relationships BETWEEN organisms -- anything imaginable, because there is no super ordinate system regulating the interactions. Nor is there any control. There are only limits. And these interactions can be experienced by individual organisms, which, being control systems, will modify their behavior to cope with them. This leads to an enormously interesting kind of study, which is the study of phenomena that emerge from interactions between true self-contained negative feedback control systems. Because there is no super ordinate system, no supervisor, the outcome is not governed by the same laws that apply within the individual organism. Social laws are not simply a higher level of the laws of individual behavior. They are not analogous to the laws of individual behavior in any but the most superficial ways. A real understanding of how organisms interact is going to tax our capabilities for modeling for a very long time to come, and can't really get started until we have brought our models of individual organization to a much higher level of competence.

I think that negative feedback totally dominates all processes inside an individual organism, including the processes of growth and learning. I think that positive feedback may well be important in the way you visualize, but only in the realm of interorganism relationships. Those relationships, I think, are the major source of evolutionary pressure: the passive physical environment is, comparatively, a pushover. If it weren't for all those other organisms there would be plenty to eat, plenty of shelter, plenty of safe places to mate and rear young. Of course in that case we'd all still be at the bottom of the food chain, so perhaps I shouldn't complain.

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Date: Sun, 9 Dec 90 10:28:13 GMT
Reply-To: "Control Systems Group List (CSG-L)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group List (CSG-L)" <CSG-L@UIUCVMD>
From: Chung-Chih Chen <arti1!chen@VUB.UUCP>
Subject: Re: Replies, control map, reality

Cliff Joslyn:

Your viewpoint corresponds to the self-organizational (order through fluctuation) type as I posted before. But I am afraid of talking about such stuff again.

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Date: Sun, 9 Dec 90 14:58:45 GMT
Reply-To: "Control Systems Group List (CSG-L)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group List (CSG-L)" <CSG-L@UIUCVMD>
From: Chung-Chih Chen <chen%arti1@VUB.VUB.AC.BE>
Subject: Chuck

Chuck:

Please try to use 'small' letters in your text.
Don't use capital letters in all your text.

It's difficult (for me) to read 'ALL CAPITAL LETTERS TEXT'.

Chen

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Date: Sun, 9 Dec 90 20:12:00 CST
Reply-To: "Control Systems Group List (CSG-L)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group List (CSG-L)" <CSG-L@UIUCVMD>
From: TJ0WAH1@NIU.BITNET
Subject: eye movements

Eye movements:
Gary: Normally, there are no external loads to disturb eye position (and being spherical the eye is relatively immune to the effects of gravity), so the feedback loop need not utilize a peripheral position receptor. However, this does not mean that the oculomotor system does not control eye position--as opposed to eye movements. Mays and Sparks (1980), investigating saccadic eye movements in rhesus monkeys, used electrical stimulation of the superior colliculus to move the eyes of a monkey just before he began a saccade to a spot of light flashed previously in the dark (i.e., flashed just before the electrical stimulation). Despite this electrode-induced perturbation, and the fact that the flashed target was no longer visible, the monkey's subsequent saccade brought his gaze to the target location, something clearly impossible had the movement been determined solely by retinal information. Clearly, the oculomotor system controls eye position, not eye movements. That is, in the last analysis, eye movements are driven by oculomotor error signals, not retinal error signals. The muscular innervation driving the eye from one position to another depends upon the difference between the neural signals corresponding to the two eye positions, the current position and the intended position. This error signal must, of course, be self-limiting if the eye movement is to be finite. This is the negative feedback comprising the loop. The exact nature of this feedback loop is not fully understood, but some suppose that the loop neurally simulates the controlled plant's transfer function, generating what they call an "efference copy" or "corollary discharge". There is evidence that this may be the case. Hallet and Lightstone (1976) found that humans can accurately saccade toward a spot of light flashed very briefly DURING a prior saccade. This implies that the oculomotor system knows where the eye is during a saccadic eye movement. Muscle spindle afferents could conceivably provide this intrasaccadic information, but efference copies currently have the edge, and have done so for centuries. Why? For one thing, because a negative afterimage does not appear to move in the dark when you press on the eyelid at the corner of your eye.

Warm regards, Wayne

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Date: Mon, 10 Dec 90 09:04:56 -0800
Reply-To: "Control Systems Group List (CSG-L)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group List (CSG-L)" <CSG-L@UIUCVMD>
From: marken@AEROSPACE.AERO.ORG
Subject: Monday morning ruminations

Happy Monday All.

There were some great posts waiting for me this morning. I sure hope Greg Williams is going to archive these things in a little journal. Anything new on that idea Gary?

Tom, Bill et al. I think I have it!! The subject controls a relationship called "following". The cursor follows (relationship) the target at a fixed distance (configuration). Right now I have this set up one-dimensionally so the target just moves back and forth on the screen. When the target changes direction, it is a disturbance to both the configuration and the relationship. But the subject must correct the relationship in a different way -- the cursor must be moved to the other side of the target.

Both target and cursor are being driven by a disturbance. The disturbance is not our typical smooth, random one but a sequence of linear segments that have different lengths and slopes. I have moved to this kind of disturbance because it changes target direction abruptly -- with the smoother random disturbance the subject can anticipate the change of direction (possibly a program level perception -- if slower then (probably) direction change). The same "linear segment" type disturbance is applied to the cursor and target. It looks to me (though I have not analyzed it in detail yet -- I just got to this point on Sunday evening) like the time to respond to the change in target direction (in order to maintain the "following" relationship) is on the order of 400-500 msec. The time to repond (with the handle) to these abrupt changes in the disturbance to the cursor is invisible -- it is probably part of the 150 msec transport lag that we now know exists when controlling the position of the cursor. Tom, Bill et al. I think I have it!! The subject controls a relationship
called "following". The cursor follows (relationship) the target at a fixed distance (configuration). Right now I have this set up one-dimensionally so the target just moves back and forth on the screen. When the target changes direction, it is a disturbance to both the configuration and the relationship. But the subject must correct the relationship in a different way -- the cursor must be moved to the other side of the target.

Both target and cursor are being driven by a disturbance. The disturbance is not our typical smooth, random one but a sequence of linear segments that have different lengths and slopes. I have moved to this kind of disturbance because it changes target direction abruptly -- with the smoother random disturbance the subject can anticipate the change of direction (possibly a program level perception -- if slower then (probably) direction change). The same "linear segment" type disturbance is applied to the cursor and target. It looks to me (though I have not analyzed it in detail yet -- I just got to this point on Sunday evening) like the time to respond to the change in target direction (in order to maintain the "following" relationship) is on the order of 400-500 msec. The time to repond (with the handle) to these abrupt changes in the disturbance to the cursor is invisible -- it is probably part of the 150 msec transport lag that we now know exists when controlling the position of the cursor.

I think I can work this experiment to show three levels of control operating simultaneously. I can have the target change shape, for example, as a signal to change the relationship -- from following to, say, mirroring (it would have to be a relationship where the target direction change disturbance requires a change like that required for the following relationship -- where a change in configuration alone will not do). Right now, two levels is OK with me.

I think I can work this experiment to show three levels of control operating simultaneously. I can have the target change shape, for example, as a signal to change the relationship -- from following to, say, mirroring (it would have to be a relationship where the target direction change disturbance requires a change like that required for the following relationship -- where a change in configuration alone will not do). Right now, two levels is OK with

me.

By the way, I've lost some interest in the "Marken effect" because it seems that this system of user aiding only works when the "opposing" control system has the same reference as the person's (controller's). This discovery was rather disappointing in as much as it reduces what I perceived as the practical
advantages of this control aiding system. And I guess I'm not really that interested in getting involved in research on control dynamics since that aspect of control theory is, for me, not the most salient.

Regards

Rick M.

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213 336-6214 (day)
213 474-0313 (evening)

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Date: Mon, 10 Dec 90 17:33:47 CST
Reply-To: "Control Systems Group List (CSG-L)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group List (CSG-L)" <CSG-L@UIUCVMD>
From: Bill Powers <FREE0536@UIUCVMD.BITNET>
Subject: Eyes, 400 millisec, feedback

Wayne Hershberger:

Efference copies are not feedback of eye position no matter how you slice the logic. At best they are models: as you say, the efference copy must activate a mental model having the same angle/innervation sensitivity as the real eyeball-muscle combination. That is not impossible. What is impossible is for a mechanical disturbance of position to be corrected without feedback of mechanical position (in the absence of visual feedback). The efference copy contains no information about externally-originated deviations of the eye.

If stimulation of the superior colliculus causes an eye deviation to the right, removal of that stimulation will result in return of the eye to its resting position -- an equal deviation to the left. So applying a stimulation after the target appears but before the saccade starts, then removing it during or at the end of the saccade, will leave the eye where it would have been without any stimulation, assuming linear superposition of effects. Unless the stimulation remains on during AND AFTER the saccade, we can't infer that any error correction has taken place, either model-based or through position feedback. More details needed here.

The stimulation could enter as a disturbance of output. If so, the model-based control system will correct for it.

Rick Marken:

400 milliseconds isn't 150. Good. I think you're looking at "transition" control -- velocity. If you can get stable data on the parameters (first step), the next thing is to put in some step-disturbances of configuration -- make the target jump now and then, then continue. I think we're getting into what the engineering psychologists have called "non-deterministic" control, meaning that you're sensing and controlling higher-order

parameters instead of moment-by moment configurations. Now that you mention it, a sine-wave target motion with variable phase or amplitude, about 2 Hz., should call on transition control, too. Sounds like you're getting there.

Norman Packard (via Gary Cziko):

Positive and negative CONNECTIONS don't imply positive or negative FEEDBACK. To find the sense of feedback, you have to trace all the connections ALL THE WAY AROUND a closed loop. The product of all the signs is the sign of the feedback. So a negative feedback loop has to contain an odd number of negative connections.

Best -- Bill

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Date: Tue, 11 Dec 90 17:16:58 CST
Reply-To: "Control Systems Group List (CSG-L)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group List (CSG-L)" <CSG-L@UIUCVMD>
From: Bill Powers <FREE0536@UIUCVMD.BITNET>
Subject: Musings

Quiet on the net tonight. Mary is spending much time out of bed and is feeling good. I'm learning to do things I probably should have been doing for 30 years.

Here's a little musing from this afternoon.

Sometimes when I rise into the rarefied air inhabited by theoreticians I am overcome by a peculiar giddiness called epistemology. In this state, it seems to me that diagrams of nervous systems lift themselves off the page and become solid realities, that equations begin to whisper secrets in my ear, that the mind's eye has acquired a clarity and sharpness never attained by the transparent globes through which I normally peer. I speak in tongues; I am on the threshold of a mystery.

And sometimes, just in time, I settle down with a book like John McPhee's "Rising from the Plains" and taking a good deep breath of Wyoming oxygen I awake to find that the world is still here and the phantoms have dissolved. Here is the reality we are looking for. We live in it. It's beneath our feet and over our heads, filling our lungs, our eyes, and our bellies. This is where all knowledge begins and ends. All the rest is imagination and possibility. We do not see it all, by any means. But when we look patiently and try to understand, we can imagine usefully, and follow the slopes of rock beneath the surface where, the story goes, there were once lush swamps -- and we can drill a hole four miles deep and suck up the oil that they became.

There is nothing mysterious about the parts of the world that are hidden to our eyes. They are just more of the same. When we look at them more closely we see them better and understand them better. When we try to guess at what is beneath the surface, sometimes we guess right and sometimes wrong. When we excavate to see what the truth is, sometimes we find what we expected, which is gratifying, and sometimes we find something unexpected, which is enlightening. But we always find something, and it is real. At the moment, I can't remember why reality seemed to be such a problem.

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Date: Tue, 11 Dec 90 19:56:45 -0800
Reply-To: "Control Systems Group List (CSG-L)" <CSG-L@UIUCVMD>

Sender: "Control Systems Group List (CSG-L)" <CSG-L@UIUCVMD>
From: marken@AEROSPACE.AERO.ORG
Subject: HomeTest

If this works it will be my first post originating from my home. There are still some problems: I seem to be limited to 300 baud at the moment (though I have a 2400 baud model) and I can't seem to use my editor on the main frame so I can only line edit this post. If I get it to work (probably by friday, if at all) then it should be easier for me to start cluttering up the mailboxes again. I would like to thank Bill for the lovely posts and for the encouragement on my hierarchy research. It motivated me to go back and read his stuff on thus in Behavior: Control of Perception. Bill, you got everywhere already. It's going to be tough going past what you have already done but I'll try.

Well, here goes.

Since I don't have the editor I can't add the signiture file but this is from me

Rick Marken

==========

Date: Wed, 12 Dec 90 12:30:47 -0600
Reply-To: "Control Systems Group List (CSG-L)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group List (CSG-L)" <CSG-L@UIUCVMD>
From: g-cziko@UIUC.EDU
Subject: Optical Illusions

Bill Powers & Rick Marken:

Here's wild idea, probably useless, but maybe not.

I was thinking this morning about the curious fact that optical illusions remain illusive even after we realize that they are illusions. An example is the Mueller-Lyer illusion: two line side by side of equal length but one with normal "closed" arrowheads at each end and the other with open ones. The line with the open arrowheads looks longer than the other even when we ascertain with ruler that they are the same.

Could this somehow be used to get at higher levels of control in a tracking task? There would be a comparison line segment (A) with closed arrowheads and next to it another (B) whose length can be controlled but it subject to disturbances. The task is to keep the controlled figure the same length as the comparison one. Would results differ between naive and sophisticated subjects?

You could even manipulate the openness of the arrowheads so that closed ones on A would start to open as the open ones on B started to close.

The basic idea is that I can adjust for the Mueller-Lyer illusion only through "higher" knowledge that it exists and how it works, although this knowledge never shows up in the lower level perception: A and B look different even when I "know" that they are not. So I will have to reset my bottom reference levels based on higher knowledge of how the illusion works with no bottom-level shortcuts possible.--Gary

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USA

Date: Wed, 12 Dec 90 16:12:52 EST
Reply-To: "Control Systems Group List (CSG-L)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group List (CSG-L)" <CSG-L@UIUCVMD>
From: "CHARLES W. TUCKER" <N050024@UNIVSCVM.BITNET>
Subject: Re: Refuge from the Deluge
In-Reply-To: Message of Sat, 8 Dec 90 22:33:26 -0600 from <g-cziko@UIUC.EDU>

I am re-organizing, Gary.

Thanks Chuck

Date: Wed, 12 Dec 90 16:15:13 EST
Reply-To: "Control Systems Group List (CSG-L)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group List (CSG-L)" <CSG-L@UIUCVMD>
From: "CHARLES W. TUCKER" <N050024@UNIVSCVM.BITNET>
Subject: Re: Chuck
In-Reply-To: Message of Sun,
9 Dec 90 14:58:45 GMT from <chen%arti1@VUB.VUB.AC.BE>

I did not realize that the large type was difficult to read; it is much better for these old eyes. I will only use large type for special emphasis. Chuck

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Date: Wed, 12 Dec 90 15:55:02 -0800
Reply-To: "Control Systems Group List (CSG-L)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group List (CSG-L)" <CSG-L@UIUCVMD>
From: marken@AEROSPACE.AERO.ORG
Subject: Stirring the pot

Gary -- I'll look at your post more carefully tonight but it seems to me that using an illusion doesn't really help get a picture of the hierarchy. The illusion itself may reflect an inability to perceive lower level perceptual variables (the sensation of line length) independent of the configuration information. I think this is what is going on in many illusions, we experience a particular configuration although we "know" (modeling level?) that the sensations in the configuration are inconsistent with our perception of them in context. Maybe there is some interesting way to approach the hierarchy with illusions. What is an illusion anyway, from the point of view of control theory, other than one kind of perception that is somehow inconsistent with another. The typical idea that an illusion is a discrepancy from "reality" just
won't wash since we have no direct access to reality. The moon illusion is an illusion because the sensation (of a large moon at the horizon, small moon at zenith) conflicts with other models (that work) of optics and planetary size. I don't think that you suggested control experiment with the muller/ lyer gets at anything above configurations; but maybe it could.

Here is a question for Bill and anyone else -- since I'm thinking about perceptual hierarchies all the time now, how is it that we seem to be able to control lower level perceptions by controlling higher order perceptions? Shouldn't a lower order perception be considered lower order because it can only be used as the means for controlling higher order ones? For example, don't we carry out a very complex program (lifting, moving,etc -- ie cooking) just to produce some nice sensations in the mouth?

I'm doing this from work so here's the sig.

Rick M.

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Date: Wed, 12 Dec 90 22:23:25 CST
Reply-To: "Control Systems Group List (CSG-L)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group List (CSG-L)" <CSG-L@UIUCVMD>
From: Bill Powers <FREE0536@UIUCVMD.BITNET>
Subject: Illusion; Hierarchy

Rick Marken, Gary Cziko --

Remember in the Byte article where I drew the hierarchy two ways? One had the control systems laid out "normally;" in the other all the input functions of a given level were bunched together, all the comparators and output functions likewise. The connections were the same; only the topology was changed. The second way is more like the real nervous system: sensory nuclei and motor nuclei. Each sensory nucleus receives a lot of signals and out of it come just as many signals. For each outgoing signal you could trace backward to the inputs it depends on, so you can define a single input function for each resultant signal. But the truth is probably that there are many interacting computations going on, so that the different perceptual functions at a given level are not completely independent of each other. This could be the explanation of some phenomena like the Necker Cube and the various Gestalt figure-ground reversals, where you can perceive one configuration or the other one, but not both. If the composite perceptual computer computes one signal it can't also compute the other: some computations are mutually exclusive. This will eventually tell us something about the computations, but I don't know what.

This isn't directly relevant to the Muller-Lyer illusion, but may lead in the right direction. I'd be curious to know (a) if all cultures use arrowheads to indicate direction, and (b) if those who don't experience the illusion. If the answer to (b) is no, we may have a hierarchy phenomenon.

I guess I'm saying that these illusions tell us something about how perception works, but probably not about levels. Flip a coin.

Rick --

When I was single (AFTER the last ice age, please), I went to restaurants, but I guess it's the same problem. A big complicated high-level pattern is produced, apparently just to control a low-level perception. Einstein suffered through school and the patent office and went through thousands of hours of mathematical thought just so he could write down the configuration of marks "E = MC^2". That sure would make hash of the hierarchy.

If the ultimate goal of eating is just to produce a sensation, then it shouldn't matter WHICH sensation (eat your acid or go to bed), should it? The pattern of sensations shouldn't matter (chocolate in your grapefruit juice, honey?), or the rate at which they occur (the lunch-minute), or the temporal packages called breakfast, lunch, and dinner, or the relationships of feeding someone or being fed, or what you call the categories of food (fried fungus OK with you, dear?), or the sequence (eat your dessert and you can have your turnip greens), or the logic of eating for good nutrition, or the principle of three squares a day for everyone and not just me, or the system in which lighted candles, glowing red wine, delicious smells, and smiling people add up to something more than shadings and blobs of color.

It really seems to be true that we human beings can look at the world as if from the viewpoint of any level in the hierarchy. Dinu Lipatti made a

living by wiggling his fingers over a keyboard and Rembrandt by poking a gucked-up stick at a piece of cloth. A human being is a featherless biped. A spinal reflex works because of relationships between its signals.

Everything is a TOTE unit. Beethoven's Fifth consists of honks, beeps, twangs, tweets, scrapes, and bangs. Really.

Because we have, maybe, eleven levels of perception and control, we have ten ways to indulge in reductionism and ten ways to attribute characteristics of the whole to its parts (what's the word for that?). This is just one of the difficulties in being the same kind of system that we're trying to understand. Does this answer your question?

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Date: Thu, 13 Dec 90 13:05:23 -0800
Reply-To: "Control Systems Group List (CSG-L)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group List (CSG-L)" <CSG-L@UIUCVMD>
From: marken@AEROSPACE.AERO.ORG
Subject: Memory

Mark Olson:

Great question about the tip-of-the-tongue. In fact, memory is something that we control theory researchers have not spent nearly enough time on. The basic phenomenon of memory fits into the control model pretty nicely -- memories are just perceptual signals that are caused directly by output signals in a control loop w/o going through the environment. The controller of the perception is still the reference signal entering the system from a higher level. Different reference signals are like addresses of the different perceptions that can be produced by the system. The higher order system sending
the reference signal is probably the one that wants to experience a particular perception (which we call a memory when it is produced by the "short circuit" method). The higher level system must be able to send the appropriate references to several lower level systems in order to produce the perception it wants. Maybe this is where memory ability comes in -- you have memorized something when you can send the appropriate references to all the lower level systems so that the desired perception results.

The "tip of the tongue" would then be a situation where you are able to produce
enough of the requested perception so that you know that you can produce it -- but you are not able to properly address all the lower level systems needed to produce the perception in full. This is an interesting phenomenon, especially for a control theorist because it brings up the problem of "how do you know you are right?". I mean, you can presumably produce any perception you want if you don't have to go through the constraints of the external feedback path. How do you know that the perception you are providing yourself "from memory" is correct? Yet we do seem to know when we have recalled something correctly (though we make false alarms) and we even know (as in the t-o-t-t phenomenon) that we COULD remember it correctly if given enough time to "try". What are you trying to do? The fact that you are trying to do something suggests that there is a desired end and, thus, we are dealing with a control phenomenon -- but what is the reference (goal) of this trying. What is "correct"?

It would be interesting to think about how a control theorist would model behavior in a simple memory experiment. Any suggestions? For example, how would a control theorist model a simple list leaning experiment. The subject reads a list of, say, numbers, knowing that s/he will be asked to repeat the list at a later time. After a few minutes the subject is asked to repeat

the list. My experience as the subject is that I can produce some of the elements on the list -- and I know I am producing them "correctly" -- ie. I know they were on the list. But I can't seem to produce other items (usually those in the middle -- a well known phenomenon) yet I know that there are more to be remembered. Aha -- so I am trying to produce a perception of a certain number of items. I can reproduce some and not others. the fact that I have perceived something about the length of the list helps me perceive how many I have left to recall. I can't just say any number if I want to perceive the experimenter say "correct" or have the feeling of "correct" for myself. I guess "correct" is my ability to produce perceptions (saying the numbers) that match components of the perceptions I have of the list of numbers that I just saw. Is memory just a matter of building up a reference level for the "to be remembered" event? Does it also involve learning the way to set the references for lower level perceptions (numbers, configurations) so that the newly constructed reference perception (of a sequence of configurations -- the list to be memorized) can be produced for the subject him/her self?

I must defer to our local brilliant theorist (that's you Bill) for a model of memorizing a list of numbers.

Again, thanks for the question Mark.

Regards

Rick M.

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Date: Thu, 13 Dec 90 21:45:54 CST
Reply-To: "Control Systems Group List (CSG-L)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group List (CSG-L)" <CSG-L@UIUCVMD>
From: Bill Powers <FREE0536@UIUCVMD.BITNET>
Subject: Miscellany

Mark Olson --

If you don't know the word or the name, you don't know the word or the name (for the time being). If you did, it would be there. When you try to think of a word, and succeed, the lower-level machinery provides it and you get the perception of knowing from that result. When you fail, you can sometimes imagine the sense of knowing that you would get if you actually did know the word. That's the experience my son Denny characterizes by putting "-ness" after it: you get knowingness. Perhaps that's the tip-of-the-tongue feeling. I think I mentioned "understandingness" in an earlier post. There are also many words (often italicized in philosophical writings as if emphasis can substitute for definition) that give a sense of meaningness: they hint at Important Things, but have no actual meanings. You're supposed to treat them with respect because they sound as if they must have great portent. But all they provide is portentness.

Gary Cziko --

Mulling over your question about the illusion, I think I underestimated it. If an effort is needed to correct for an illusion, that effort must come from a higher level -- I think, now, that this is what you meant. If that is so, then a control task involving making the two lines in the Miller-

Lyer illusion remain the same length (varying, perhaps, the length of one line or the slant of the arrowheads as you suggest), the transport lag should be typical of the higher-order system, not just that of a configuration-control system. Now it sounds as if it's worth a try. If there's no change in the parameters (from normal configuration control) we've still learned something -- approximately where the illusion takes place.

Speaking of illusions, I worked up a motion-illusion program for Pat Alfano (colleague & ex-student of Dick Robertson). Three types: dots moving laterally, spinning field of dots, converging dots. Worked fine at first. Now Pat can't get any aftereffect at all from it, and neither can others. I noticed during final trials that sometimes I didn't get any aftereffect, either -- it came and went. Ideas, any of you perceptual experts?

Rick Marken --

Stop telling everyone what a marvel I am. I already know it, but it's too much of a burden to have to keep proving it. And what about the times when I'm wrong? It might be better if we just didn't keep score. Friendly game, and all. There's always a faster gun somewhere. In my case, lots of them. Wait until all these young guys listening in take hold of control theory -- you'll see.

The biggest hole in the model, as you indicate, has to do with properties of memory and how they work with the rest of the system. How does association work? What connections must exist in order for a little bit of a forgotten skill, upon being tried again, to call forth (from where?) all the other subsystems involved in it? Exactly how do symbols get turned into their non-symbolic meanings? What we need is a neural network modeler who understands CSG control theory. Don't they work with associative memories?

Loose end --

The word was "synecdoche" (syn ECK duh kee). Using a whole to stand for a part, or a part to stand for a whole (either one). Trust Mary to find it.

> "All hands on deck!" the captain cried;
> But he was wroth to find
> That when the hands arrived on deck
> they left the men behind.
>
> and the whole for the part:
>
> "The law is upon us," cried safecracking Lardner,
> "For poundin' I hear at the door!"
> "The law it is not, lad," responded his pardner.
> "'Tis only Patrolman O'More."

Espy, Willard R.; "The Garden of Eloquence: a Rhetorical Bestiary". New York: Harper & Row (1983). P. 138. After Henry Peacham, 1577.

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Reply-To: "Control Systems Group List (CSG-L)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group List (CSG-L)" <CSG-L@UIUCVMD>
From: Bill Powers <FREE0536@UIUCVMD.BITNET>
Subject: Levels of control and Culture

Chung-Chih Chen --

I've been meaning to ask you some questions about control theory and

cultural/linguistic differences. You've been seeing us talk about supposed levels in a hierarchy of perception and control. I would like to know whether the names of the levels would have any significance when translated into Chinese. More important, I'd like to know whether they signify EXPERIENCES to a speaker of Chinese in the same way they suggest them to a speaker of English. Just consider the first four levels: intensity, sensation, configuration, and transition (lowest to highest level). My thesis is that what we experience as transitions is derived from successive configurations, or rates of change of configurations; that configurations are derived from arrangements of sensations; that sensations are weighted sums of intensities; that intensities represent the basic quantitative response of sensory endings to stimulation: the basic quantities of experience.

These definitions are not supposed to be just linguistic conventions in the English language. Each one is supposed to point to a type of experience that any human being can have without speaking or thinking of it in words. If you look at a light and listen to a voice, you can compare them (a higher-level operation) in terms of which is the more intense experience, regardless of kind. When you experience sensations, they are made of different elementary intensities, but are experienced as being different from other sensations in quality -- i.e., this sensation is clearly not that sensation; but they're invariant with respect to which subset of intensities is present (warmth is the same no matter where on the skin it is felt). When you see an object or hear a musical chord or harmony, a unitary sense of configuration is present, but on close examination it can be broken down into individual sensations (light, dark, edge, color, or for music, pitch, quality of sound). When you see an object in motion or hear a musical chord rising in pitch, there is a sense of change that remains constant (the magnitude indicating rate of change) while the change is in progress, but at any instant you can also experience the position, shape, chord, and so on. And for each of these classes of experience but the lowest, the existence of one depends on the existence of those of lower level.

I'm not trying to persuade you to agree, only trying to find out if these type-definitions make any sense to someone from a different linguistic and cultural background. If this theory is really about Genus Homo, it should make sense in any language -- or without language. If that's not true, we have a chance to learn something important.

P. S. to others on the net: If you have friends from other linguistic and cultural backgrounds, maybe you could ask them some similar questions. And I suppose I should also ask whether other English-speakers can make sense of this arrangement, based on direct experience.

Best regards -- Bill Powers

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Date: Fri, 14 Dec 90 08:48:25 -0600
Reply-To: "Control Systems Group List (CSG-L)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group List (CSG-L)" <CSG-L@UIUCVMD>
From: g-cziko@UIUC.EDU
Subject: Illusions

Bill (Powers):

I think you have a better idea of what I meant about using the Mueller-Lyer illusion to test for higher-level control, but perhaps still not exactly what I had in mind.

Let's call the figure with "normal" arrowheads on each end LS (for "Looks

Shorter than it "really" is) and the figure with the inside-out arrowheads (like when your umbrella gets blown inside-out) LL (for "Looks Longer). The figure on the left never changes its length. The figure on the right changes its length due to random of disturbances and the subjects movement of the mouse. We tell the subject that his task is to make the two line segments the same length.

1. Naive condition: First the reference figure is LL and the controlled one is LS. The naive subject (using lower order info) should make a consistent error by making the controlled figure too big (he compensates because it looks smaller). After getting some good data on this, you can then gradually change the angle of the arrowheads. As they both start looking like the capital letter I, the subject should not show his former bias. But now as what was LL becomes LS and what was LS becomes LL, the bias should swing the other way.

2. Re-organization: Now we take our naive subject and give him some Mueller-Lyer sets and a ruler. It will become quite apparent to the subject now how the illusion works, but this knowledge is only at a higher level.

3. Informed condition: Now we run him again. There should now be less "bias" (perhaps he will even overcompensate), but only because he is using higher-level knowledge to reset his lower-level reference levels. There is nothing at the configuration level that will allow him to adjust for the illusion. And now when we fiddle with the arrowheads he will have to compensate using his new higher-level knowledge of the illusion.

4. Feedback: We could now show the subject how well he did. He may find that he was overcompensating or under compensating for the illusion and therefore do better at matching the lengths the next time. But no matter how much practice or feedback he is given, this knowledge should remain at a higher-level only.

What seems to be of particular value in this task is that the naive and informed conditions of the task are exactly the same, except for the subject's higher-level knowledge (we could even use children if too many adults have already lost their innocence). Thus it should be easier to find clear evidence of the use of higher-level knowledge (subject as his own control).

Keeping with CSG tradition, since I was the first the predict the difference, it should be called the Cziko Effect. However, since I'm such a nice guy, if you are the first to actually find it, I would consider calling it the Cziko-Powers effect (I'll have to learn Pascal one of these days). If Rick Marken finds it first, it will probably remain the Cziko effect since he already has an effect.--Gary

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Date: Fri, 14 Dec 90 11:16:47 -0600
Reply-To: "Control Systems Group List (CSG-L)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group List (CSG-L)" <CSG-L@UIUCVMD>
From: Joel Bennett Judd <jbjg7967@UXA.CSO.UIUC.EDU>
Subject: memory

The question about the T-O-T-T phenomenon and reply reminded me of the psychological and psycholinguistic work done with memory, and I want to mention a couple just to see what thoughts those with a CT perspective might have regarding them. The statement which reminded me of these experiments was:

"The higher order system sending the reference signal is probably the one that wants to experience a particular perception..."

The work I'm thinking of is that done with witness memory that changes over time; with closure and the finding that a memory of open geometric shapes slowly closes those shapes after a while; and sentence memory tasks where even immediate recall doesn't affect the fact that we will replace uncommon or infrequent words or expressions with more common ones, or even complete incomplete actions or results from the original sentences.

So it seems that, lying aside, we purposely change memories to conform to an internal reference perception, is that right? And in general, it seems that these references become more rigid as we get older (conservatism, narrow-mindedness, you can't teach an old dog...)?? And then what would CT say about Critical Periods of development? Are these low levels of a hierarchy which are essential for higher order control?

Just some musings on the spur of the moment - Joel.

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Date: Fri, 14 Dec 90 11:46:36 -0600
Reply-To: "Control Systems Group List (CSG-L)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group List (CSG-L)" <CSG-L@UIUCVMD>
From: m-olson@UIUC.EDU
Subject: tongue and knowing

Bill and Rick,
I can't yet determine whether Bill's comments on the tip of the tongue are consistent with Rick's comments. Bill, do I understand you to say that one either knows something or doesn't know something (tautology, I realize) and that the perception of "knowing" the right name when one really doesn't know it is just an illusion. In other words, when I think that I really do know this person's name but yet cannot produce it, I really don't know it and am being deceived. (?) Rick's comments seem to suggest that I do in fact know it in some sense. Is this a semantic problem centering on the word "know." Bill, are you saying I have no basis for thinking that I do know the name--that the reason I think I do is that I just happened to get the "yes you know that name!" signal?

I'm taking a course on memory next semester and I'd really like to get a control theory framework on it before encountering the info--reorganization is a lot more difficult after the fact even for a young mind like myself.

Hopefully I'll get to my mail once more before Christmas Break--if not, happy holidays, God Jul, and all the rest. I'll be back in in January.

--Mark Olson

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Date: Fri, 14 Dec 90 15:55:45 EST
Reply-To: "Control Systems Group List (CSG-L)" <CSG-L@UIUCVMD>

Sender: "Control Systems Group List (CSG-L)" <CSG-L@UIUCVMD>
From: "CHARLES W. TUCKER" <N050024@UNIVSCVM.BITNET>
Subject: COMMENTS ON POSTINGS

TO ALL CSG'ers

I realize that you have not heard from me lately but I have been busy making a disturbance for the administration and the Board of Trustees around here. Two weeks ago we got over 400 faculty to sign a set of resolutions asking for more money for the faculty and less for the administration, then I called for and got a special meeting of the Faculty Senate to open the search for our new President and today the committee announced that the finalists would be named soon but it will not be open. More action is called for and we will take it soon. But I do have a question for those of you who are in academe: What can a faculty legitimately do that will not harm the educational process when requests for actions are refused by the Board of Trustees? An answer to this will make you realize what little "power" we have but I would like you answers [send them to my personal mail box rather that clutter the NET with them - thanks]

Now on to some postings that I have read over the last two weeks on this NET.

On reality

It seems that my mentioning of "reality" has occasioned much talk; it does this everytime I mention it to people who appear to use an Enlightenment view to manage their conduct yes, this is at the highest level of a control system - may even be above the "system" level - so a disturbance that is very difficult to manage. But my current characterization of the "positon" on the NET is:

> We would prefer to keep the word "reality" but use it in a way that differs from any other epistemological position since most views (even pragmatism and especially constructionism) have not developed a model of the living system to the extent that we have developed it [eventhough we claim there is much work to be done]. We recognize (WTP 12/6) that since the organism uses electrochemical discharges as information that this information is not of the experience "outside" the organism. We also understand that we are using a model which we claim we have found useful to make sense out of our experience and we recommend that others use it to make sense out of theirs. We also claim that our model solves problems that other models have not been able to solve; especially models which are dualistic and static (WTP 12/ #2). If we select any rules to use to judge the usefulness of our model we would suggest that they be at least one of the following: (1) the use of it "solves problems" in that actions that we were not able to accomplish can now be accomplished [this is action par excellence] or (2) the use of it offers a perception of coherence of activities; a reduction of disturbances to the point that action is continued since errors are corrected (Wayne; Marken 12/7) or (3) the model recognizes "negative feedback" and ways to correct it without proposing an equilibrium (even dynamic) procedure. [Some propose all these rules be used while some use just one] Finally, we all can recognize how very difficult it is to rid ourselves of the use of the word "real' and even the dualism that can go along with it but we must continue to guard against using a model whcih contradicts ours and is not as useful - it is very difficult (WTP 12/8; 12/11).

I don't know that what I just wrote is helpful but I still would encourage someone to put together a "position" [even tentative] statement for the CSG on this stuff.

On "Social Laws"

The statement from Bill of 12/8 on "social laws" is very important. He states: "Because there is no super ordinate system, no supervisor, the outcome is not governed by the same laws that apply within the individual organism. Social laws are not simply a higher level of the laws of individual behavior. They are not analogous to the laws of individual behavior." WOW!!!! First, I don't claim there are laws of individual behavior - there is a model that we use to put together to use - there are no laws of any experiences - there are rules, statements, models [similar] that we use and treat as "laws" only to be worked out. Even though the statement we use as "laws" for collective action may not be analogous to those we use for individuals they are just as useful and even more forceful than those for individuals but individuals have to use them to make them work - the person is always the last instructor- religion, science, ideology, the state are all collective phenomena which emerge from the continued interaction of individual organisms - the words "we" and "they" are used by "us" to use these collective creations. I believe the CROWD program and our own actions on this NET are nice illustrations of collective actions. Yes, we have to develop our model to deal with these experiences but that is a reason to have communication with those like Clark McPhail who appreciate and understand these matters.

On relationship level

The one matter that is forgotten by some modelers - when you use or have someone else use your joystick to do something you tell them what to do and the telling should indicate what reference you propose is used. My DEMO (in CC) takes off from Bill's rubber band illustration (look at Bill's video tape #2 for another example of this) by explicitly telling another what to do. Since you are modeling you are working back and forth between at least two sets of instructions - those you follow to write the program for the computer and those you give yourself when you "test" the program with your joystick. My guess is that when a person makes an error that can not be corrected easily at the relationship level then he/she moves to a level where the error can be eliminated. We do this all the time - we lower our sights when we run into a problem and so what we can do now (this is not a pragmatically aceptable solution by the way). Examine your own behavior and see if this makes sense.

On "positive feedback" and abnormal behavior

Several comments by Chen 12/7; WTP 12/7 & 8 and Cliff 12/12 deal with "positive feedback" and a problem with it. I don't have suggestions for moving toward solutions for all of the questions but I will make two points: (1) I see positive feedback as the present inability to correct an error and the error gets larger or remains the same over a time period (could be forever); depending on the level where the positive feedback is located (the lower the more difficult) it will have different consequences. [some have a positive feedback when they consider the issue of "reality" each time they deal with it but since it has little to do with eating breakfast they go on] (2) I see a very small portion of what is

called "abnormal behavior" [I am excluding those behaviors where there is acceptable evidence that there are electrochemical errors] as the outcome of positive feedback. I don't think it useful to see most conditions labeled by psychiatrists as the result of positive feedback. He I agree with Tom Szasz (who by the way agrees with the model that Mead and Dewey proposed which is the same as ours), Tom Scheff, Erving Goffman and Bill Glasser (his early works): madness is purposive behavior designed to disturb others and self - it is offensive not defensive as Freud would have it (Freud was a good observer/reporter but used a defective mechanical model to explain what he reported). Suicide is also purposive behavior designed to kill one's self - those who fail (called "attempted suicide") are just incompetent in the actions required to be successful at killing oneself [uncorrected positive feedback].

I close with a definition by Dewey and Bentley from KNOWING AND THE KNOWN (p. 300)

> REALITY: As commonly used, it may rank as the most metaphysical of all words in the most obnoxious sense of metaphysics, since it is supposed to name something which lies underneath and behind all knowing, and yet, as Reality, something incapable of being known in fact and as fact.

I expect that I will read something from y'all before the holiday so I will save my greetings until later but note that you are in my thoughts wherever they might be.

Chuck Tucker Sociology (803) 777-3123 N050024 AT UNIVSCVM

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Date: Fri, 14 Dec 90 14:48:30 -0800
Reply-To: "Control Systems Group List (CSG-L)" <CSG-L@UIUCVMD>
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From: marken@AEROSPACE.AERO.ORG
Subject: More memory

Mark Olson and Bill P.

First, let me apologize to Bill. I love your ideas but I don't mean to place you in a position where you feel that I expect you to be the top gun. I don't think of this in any way as a competition. I know that you are wrong sometimes. But even when you miss, your form is impressive. But after all, you were right about one big one that virtually no one else noticed (Behavior is the control of perception). But please accept my sincere apologies if what I said seemed to place you in competition with others; that was not anywhere near my intent.

Now, back to memory (if I can recall). Mark, my comments about the t-o-t-t phenomenon were based partly on my subjective experience with it and what I recall of Roger Brown's paper on it (mid 60s?). In answer to your question, I think that I do know that I "know" the word that I cannot recall. If I can't "get it" after some period of trying to recall it, it often comes to me at some later time. If someone tells me it before I recall it I know that that is indeed the word. In Brown's study he showed that people who had the t-o-t-t state were able to tell many things about the word -- like it's first letter (as I recall) or the number of syllables. These people were more often right about these things than not and they were more right the more confident they were that the did indeed know it. So I think the t-o-t- t phenomenon is more than a "meaning-ness" phenomenon (though I know that the latter is all too common). I may be wrong about this but it's a tough one to test. For example, I often know when I know (but can't recall) the answer to a crossword. The problem

with confirming this is that I sometimes overestimate the possibility that I will be able to recall it -- but when finally told I realize that I did "know" it after all.

Gary -- very funny post. If your Muller-Lyer prediction comes true I think it's definitely the Cziko effect -- much better than Marken effect. But it is great to have an effect; my kids loved it when I told them about it. But it puts you in kind of a bind if you discover another effect. I guess you just have to be clever and call it things like the Cziko room (like the Ames room) or Cziko's pendulum (Foucalt's pendulum). Or you could be a lttle Freudian and go for the Cziko tower (Trump tower -- shame on you Donald).

Have a wonderful weekend. I'm still trying to get my home modem to work. I need a new cable, I think. Too bad those folks left the net just before the mail level went down by what seems to be a factor of two. Maybe they made everyone more tentative. I'm not tentative -- just stuck at 300 baud.

Happy Holidays

Cho Cho Cho (We celebrate Chanuka and Christmas

Rick M.

(No signiture cause I'm at home)

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Date: Fri, 14 Dec 90 18:40:08 CST
Reply-To: "Control Systems Group List (CSG-L)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group List (CSG-L)" <CSG-L@UIUCVMD>
From: Bill Powers <FREE0536@UIUCVMD.BITNET>
Subject: Lots of stuff

Gary Cziko --

OK, I'll try it. Lacking naive subjects, I'll just pretend to be one. When I get a program running, I'll send it to you and you can do the whole experiment. Don't hold your breath until you get it, though. The Cziko Effect it will be, if there is one.

Tip-of-tongue --

I think a word is getting in the way here: "knowing." I often get the sense, when trying to remember something, that the information is in here somewhere. That is, I imagine that it is. But at the moment I don't know the information. So I use "know" strictly to designate present conscious experience, and I use "I" to mean the conscious knower. There is a lot of me that isn't "I"; for example, whatever it is that is making this sentence come out grammatical. I rely on that part, and often fool around with its output as it's forming a sentence, to make it more interesting, but mostly I just think of what I want said and out it comes. So basically I don't know the information that is going into sentence construction -- but something in here does. But I don't count that as knowing. When I get the t-o-t-t effect, I basically just put in a request and wait. Either the word is going to come up or it isn't. If it doesn't come up after a reasonable wait, I ask Mary. In any case I don't know the word until I know it. By my definition of knowing.

Chuck Tucker --

Nice bunch of thoughts. I think it's time to ask you about instructions.

1. Are self-instructions given in words?

2. What is the instructor and what is the instructed?

3. How does an instruction (from anyone) get turned into a reference signal for a specific act that will accomplish the instruction?

4. If instructions cause behavior, how do we account for a person who refuses to carry out the instruction?

Relationship level --

Usually we tell subjects what reference condition to maintain in a tracking experiment. That's not always the case. In Rick's mind-reading experiment, the subject is just told to move any object in some regular pattern. The computer never has to know what the pattern is -- it finds the controlled variable without that information. Of course to get a subject even to sit down and wiggle the handle, you have to describe the point of the exercise, and the subject has to agree to do it. And all this communication takes place in words -- usually. The whole interaction could be accomplished by demonstration, with no more instruction than "do what I'm doing now."

Going up and down levels isn't just a matter of what's easy or hard. There's no easy way to create a specific sequence of movements (up-down-down-up) by controlling a configuration; there's no way at all. The problem that Rick and I are having is finding a way to define a task so that the condition of the display corresponding to zero error can't be duplicated by seeing it at a lower level of perception. We're hung up on a principle, I think, or we've both fallen into some mental rut. It can't really be this hard.

Positive feedback --

I insist on a rigid compulsive-obsessive definition of feedback. Feedback isn't the effect of output on input. It's the condition in which output affects input while input is affecting output. Feedback is a characteristic of the entire closed loop, not just one part of it.

Positive feedback is a condition in which any small perturbation anywhere in the closed loop leads to a perturbation IN THE SAME PLACE and OF THE SAME SIGN. The more you eat, the more you want (and the more you want, the more you eat). That's positive feedback. Here's another example: "What do you mean, you feel stupid when I talk to you? That's the dumbest thing I've ever heard." That's positive feedback if the way you feel depends on what I say, and what I say depends on your description of the way you feel, and if feeling stupid implies error.

Errors do get larger when positive feedback is present, but they get larger because they produce action that makes perception deviate even farther from the reference condition, leading to a larger error, more action, and so on. Mere failure to correct an error doesn't indicate positive feedback. Positive feedback is a situation in which the actions driven by error make the error worse. There are undoubted psychiatric conditions that have this characteristic, as you propose.

Reality --

Remember, I'm basically a physicist and an electronics engineer. I'm used to building things made of devices I can't see the insides of. I'm used to touching the probes of meters and oscilloscopes to points in live circuits, seeing the indications, and perceiving something that in fact I can't see -- what is "really" going on in the circuit. This sort of activity made me acutely aware of the difference between the world I observe directly -- the meters and stuff -- and another world where something entirely different is, theoretically, taking place. I'm perfectly comfortable with knowing

that that world is imaginary, yet treating it as if it is real (but outside the boundaries of my direct experience). Pragmatically, pretending that this imaginary world is really there works extremely well. I am largely persuaded that there is really some sort of lawful coherent universe inside transistors, resistors, capacitors, and other components I experience only as shapes with wires sticking out of them. I'm pretty confident that I could take any one of those components, and dissect it down as far as a microscope can help, and find essentially what I would expect to find inside it. I can't dissect it far enough to see the electrons and holes, but there's plenty of indirect evidence that there is something going on behind the scenes that behaves a lot like the concept of an electron or a hole. Maybe another whole conceptual scheme would work just as well -- but that would only make my impression of an independent source of coherence stronger (as well as my impression that our imaginations aren't quite large enough to encompass it).

I guess that I treat the question of reality as a factual problem, not as a philosophical one. Is there, in fact, more to the universe than meets the eye? To me it seems obvious that there is. Maybe that's why my best friends keep telling me that I'm a lousy philosopher.

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Date: Fri, 14 Dec 90 19:51:32 EST
Reply-To: "Control Systems Group List (CSG-L)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group List (CSG-L)" <CSG-L@UIUCVMD>
From: Dennis Delprato <USERXEAK@UMICHUM.BITNET>
Subject: Ethics & the Law

REALLY FROM: Dennis <DELPRATO@UM.CC.UMICH.EDU>

It might be appropriate for me, as a representative from the fair State of Michigan, to point out that control theory has recently won a "victory" of sorts. I imagine most of you know by now that an Oakland Co. district court judge dismissed first-degree murder charge against Dr. Jack Kevorkian. Kevorkian was alleged to have supplied toxins, an apparatus, and instruction that enabled a 54-yr.-old Alzheimer's disease patient to kill herself. The basis of the judge's decision to dismiss the charges apparently was simply that Michigan has no law that applies to assisted suicide. The dark side of all this is that the forces of antiquity are stirring more than ever to have the legislature write a law to make assisting suicide a prosecutable offense. I'm sure that if they could get their hands on their nonspatiotemporal soul, then they would seek to prosecute this aspect of those who elect to kill themselves, as well.

This note leads me to suggest that control theory (by whatever name) is extremely unappreciated in terms of its respect for individual liberty. As Bill Powers put it previously, control theory ethics (or a fundamental ethical dictum of control theory) is that others are control systems, too. In other words, keep your cotton-pickin' hands out of other's business (i.e., lives). Along these lines, I get very discouraged when I note enormous inconsistencies among individuals' positions on various issues. Most prominent in line with the present topic is when the same individual touts "civil libertarian" positions, yet holds to one of the many extant versions of one-way determinism. It certainly is difficult to refrain from strong-arm tactics in interpersonal and other social relations, given certain "positions" in which we find ourselves and the various ways in which such tactics are encouraged. Then, it is all the easier to use authoritarian techniques when one assumes that others are simply subject to wind-weather vane operation, anyway.

You all better agree with me on this--or else!

Dennis Delprato
Eastern Mich. Univ.

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Date: Sat, 15 Dec 90 10:15:30 -0600
Reply-To: "Control Systems Group List (CSG-L)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group List (CSG-L)" <CSG-L@UIUCVMD>
From: m-olson@UIUC.EDU
Subject: ethics

Dennis,
You said alot in a short paragraph so I'm not entirely clear what you were saying. I agree that an ethical system derived from control theory would basically have the form of "keep your hands out of others people's buisness" as you stated. But I do think that if the output of one person is going to result in a large (maybe irreconcilable) error, then laws (or something) need to be there to ensure that that doesn't happen. If they are not there, then everyone will reorganize and reorganize until they have made their worlds small enough to protect themselves from the outputs of others. Go to New York or anywhere people are screwing around with other people's inputs and try to communicate with those people. (I'm not expressing this as clearly as I would like to but I think everyone understands--its probably already been stated before).

In regards to ethics and determinism, I am not exactly clear on what you are saying here either. Of course I understand and agree with what you are saying in regards to simple determinism, but what about when you get into the realm of chaos--completely determined, yet completely unpredictable. I think you can be a hard-core determinist and a control theorist with "keep your hands out..." ethics at the same time. Certainly the beauty of control theory is that organisms can achieve consistent goals despite chaos, but I am speaking of chaos within the organism itself--might there be chaos present (completely deterministic) in the formation of the control systems or driving THE reference level?

As I type I realize that I am speaking of determinism on a different level than you so I suppose the above isn't truly relevant to ethics and law--I'll send it anyway.

Bill and Rick,
Thanks for the comments. You have different definitions of "know," which is completely alright with me as long as I realize that. I would expect that I'll be doing a lot more postings on memory next semester.

Just for the fun of it I wrote a brief (4 page) summary of control theory and "why input-output doesn't work and should be discarded" paper. I wrote it so that I'd always have something to give to someone when phone conversations or informal gatherings don't quite provide the right occasion to explain it. So I sent it to a number of former profs and present friends in Psychology, AI, and Education--more young minds. We'll be a dominant theory in no time....

I will most likely not get to read your comments--happy holidays!

--Mark Olson

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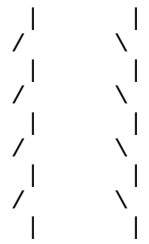
Date: Sat, 15 Dec 90 21:27:06 -0600
Reply-To: "Control Systems Group List (CSG-L)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group List (CSG-L)" <CSG-L@UIUCVMD>
From: g-cziko@UIUC.EDU
Subject: Another Illusion

Bill (Powers):

Here another illusion that may be useful in solving
>The problem
>that Rick and I are having [in] finding a way to define a task so that the
>condition of the display corresponding to zero error can't be duplicated by
>seeing it at a lower level of perception.



If I could get the vertical segments to join at the middle of each slanted one, it would work better, but you should get the idea. The vertical lines look like they're converging at the bottom when in fact their parallel. In this case, the disturbances would vary the "parallelness" of the lines and manipulating the angle of the intersecting segments would be analogous to manipulating the arrowheads of the Mueller-Lyer illusion. There must be lots of other illusions that could be used in this way.

No, I won't hold my breath until you write the program, but as listowner of the network, I could always hold the network hostage if it doesn't get done soon enough!

Rick (Marken):

Thanks for the warning me about not making the same mistake you did in giving your name to an effect which you don't like so much now (is that why more and more men are refusing to give their last names to their wives?). So if this works out, how about the Cziko Higher-Order Illusion Effect (other suggestions welcome)?--Gary

P.S. After my male chauvinist remark, I just realized that I don't think that there are any women on this network (except for Mary Powers who get's her remarks "editor commented" by Bill). Let's hear from the ladies if I'm wrong.

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Date: Sat, 15 Dec 90 11:16:17 GMT
Reply-To: "Control Systems Group List (CSG-L)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group List (CSG-L)" <CSG-L@UIUCVMD>
From: Chung-Chih Chen <arti1!chen@VUB.UUCP>
Subject: Re: Levels of control and Culture

Bill Powers:
Your definition of levels seems quite reasonable for me. I read your book. And I can understand. But are these levels my "experiences"? I don't know. I think I perceive everything at the highest level, it's difficult to say I can experience the first, or second, or ... level. We define these levels according to our reasoning.

Anyway, I am interested in designing a neural network which can perform the third-order control: configuration. I want it to recognize an object invariant of rotation, translation, and scale. Suppose we have a 2-D image. In the simplest case, each pixel (intensity) may be just 0 or 1. 1 will correspond to the object, 0 is the background. So the first level is the image (input). At the highest level there is an output to tell me if the image corresponds to the same object (or there may be several outputs corresponding to different objects).
How can I use your
theory to design the network? I need your suggestions (as practical and detailed as possible). I will do the simulation.

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Date: Sun, 16 Dec 90 09:12:37 CST
Reply-To: "Control Systems Group List (CSG-L)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group List (CSG-L)" <CSG-L@UIUCVMD>
From: Bill Powers <FREE0536@UIUCVMD.BITNET>
Subject: Illusions, perceptual levels

Gary Cziko --

You realize, of course, that by getting a person to control the elements of an illusion to eliminate it, you can measure its magnitude. This means that for every illusion, there will be a Cziko effect: GCa, GCb, etc. You're going to have an unfair number of effects. Maybe you could give some of them away as Christmas presents.

There's a bit of a technical problem in presenting these illusions on a CRT screen -- low resolution. Even on a VGA 800 by 600 screen, you'd have to sit back about five or six feet to make the jaggies invisible. I'll see how it works out. I can do 640 by 480.

Psychologists have been fascinated by illusions the way a rabbit is fascinated by a snake: they threaten the idea of the objective observer. And illusions are one of the few phenomena of individual behavior they can reproduce reliably in the laboratory. I hope, though, that we're going to focus on the big obvious control problems pretty soon, such as: why don't many students like school? What are teachers and students controlling for? Would it be a good idea to teach children and/or teachers control theory? Are there levels of control that can be identified during development, so we don't try to teach students to accomplish things they're not prepared to perceive? Can we use control theory to define a "natural progression of learning?"

Chung-Chih Chen --

>Your definition of levels seems quite reasonable for me. I read your book.
>And I can understand. But are these levels my "experiences"? I don't know.
>I think I perceive everything at the highest level, it's difficult to say
>I can experience the first, or second, or ... level. We define these
>levels according to our reasoning.

If you don't experience perceptions at these levels, but see them only as logically acceptable definitions, then my idea about levels of perception is wrong. But I'm not willing to give up that easily, so I'd like to explore this further to see if we're really communicating.

I assume that when you send messages to the network, you're using a keyboard and typing on it using your hands and fingers. And when you get messages, you see letters on a computer screen. I think that if you look very carefully, you will see that the keyboard is an object, that your hands and fingers are objects, and that the letters on the screen are objects. This is what I mean by the third level of perception: the experience of configurations, directly, without using logic.

If you pick any one object, like one of your fingers, you can see that it is made of smaller objects: fingernails, knuckles, wrinkles, and so on. But at the same time you are seeing the whole finger, and also the whole hand. You are seeing many configurations at once, in parallel. However, there is another way to look at the finger. For example, when you see the edge of the finger against the background, you can ask yourself exactly where that edge is, and look as closely as possible to find it. When you do, you will see that the edge is not an object. It is a place where the color of your finger ceases and the color of the background (or the shading) begins. When you look at a wrinkle just as closely, you will see that it is a place where the color of the skin between the wrinkles changes to the color inside a wrinkle. Now you are looking at sensations, not objects. The color sensation is generally the same all over your hand, but as you look closely, you will begin to see many differences -- differences in color, in surface texture, in shading. As you do this, more small configurations will be noticed, defined by places where the sensations of color and shading differ. So now you are seeing the finger at two levels: sensation and configuration. You can add another level by noticing colors that are the same except for their brightness; then you will also see that "shading" is created by a change in brightness as well as color. The dimension of intensity has been added (the lowest level in my hierarchy).

My claim is this: you can't see anything at one level if the lower levels are missing, but you can see it even if a higher level is missing. The whole world could become (for a short time) skin-colored, so you wouldn't see a hand, but you would still see the color. But if there were no intensity, you wouldn't be able to see a color. So the hierarchical sequence, from low to high, is intensity, sensation, configuration.

I think -- I hope -- that I am talking about direct experience here, not about strings of symbols that we can manipulate according to rules in order to talk about or describe (to ourselves) experiences. Obviously I have to use these letter-configurations strung together in left-to-right bunches and related according to grammatical rules of ordering, just in order to point out these experiences, but once you try looking at your finger in the way the words suggest, you don't need the words any more to see what I am talking about. You can look silently (inwardly and outwardly) at your finger and experience intensities, sensations, and configurations. You can do this without telling yourself that this is what you're doing, and without naming the categories. Or at least you can separate the naming from the experience being named.

This is what I mean by the word "perception." The reason I am interested in your experience of these things is that I'm not sure how much language contributes to this way of ordering the elements of experience. I thought that a speaker of a very different language might see them the same way I do, which would support the idea that these are truly levels of perception and not just of language, or that perhaps a speaker of a different language would experience a different set of basic perceptual elements, in which case I would have to re-think the entire meaning of my hierarchy. Either way, I would learn something.

In Chinese, are there sets of terms that go naturally with these levels? I assume that there are terms for objects, like "finger." What about sensations like color, and intensities like brightness? If we're really talking about universal human perceptual organization, then language should follow it (approximately). But if this organization is only apparent, then maybe a different language would create different categories into which perceptions could be analyzed, so that sensations would not derive from intensities, and configurations would not derive from sensations. The only way to check this out is to ask someone else to examine his or her private experience very closely, to see if alternatives appear.

All this will make a very big difference to anyone who is trying to model human perception.

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Date: Sun, 16 Dec 90 12:03:47 CST
Reply-To: "Control Systems Group List (CSG-L)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group List (CSG-L)" <CSG-L@UIUCVMD>
From: Bill Powers <FREE0536@UIUCVMD.BITNET>
Subject: Modeling perception

Chung-Chich Chen --

I can't solve your problem explicitly, but let me just muse on the subject without trying to say anything directly relevant to configuration perception. Maybe some principles will show up.

In my model of perception, each perceptual attribute is imagined to be represented as a single signal that can vary only in magnitude. I think this is roughly what the neural network people have concluded, too. So what we are looking for are functions that will generate a single output signal, given some set of input signals. People working with multi-level perceptrons are talking about "hidden layers". I've been talking about them for a long time: I call them levels of perception. But they're not hidden: we can observe them. But maybe not all of them. Maybe some hidden levels occur inside what I call a level, and all we experience is the final output signal. Or maybe in my attempts to notice levels in my own perceptions, I have overlooked some fine structure. But I don't insist that all intermediate processes be known to consciousness.

Take a simple object the shape of a computer case (a rectangular parallelopiped). This object has (among others) two interdependent attributes: distance and retinal size. There is a function of these two attributes that we normally assume to be invariant as we move closer to or farther from the object: absolute size, or retinal size (area) times distance. If the brain postulates a particular absolute size, then as the retinal image changes dimensions, the apparent distance of the object changes the other way. An increase of retinal size leads to a decrease in distance and vice versa. This is the main way to judge distance without binocular vision, for familiar objects.

In terms of a circuit, how would this work? We would implement it with a

control system. The control system receives two signals, one standing for retinal area and the other standing for distance. The input function multiplies the two signals to yield an absolute-size signal. The absolute-size signal is compared with a reference signal, the desired or hypothetical real size. The error signal is then routed back to the input function through what I call the imagination connection. Thus the distance signal is supplied by the control system, not by the outside world. The result will be to maintain the product-signal, the perceived actual size, in a match with the reference signal, the postulated size.

This control system does not have to be a whole behavioral control system. It could be part of the inner structure of a perceptual function. This leads to a very interesting possibility that only just now has occurred to me. This perceptual control system requires a reference signal that defines the hypothesized actual size. Where does it come from? Clearly, from a higher-level perceptual system that forms the hypothesis in terms of a reference signal of the appropriate magnitude. So it is possible for a higher-level system to insert a perceptual hypothesis into the workings of a lower-level perceptual function. This is quite separate from the behavioral reference signal that specifies an action that will bring the perceived size to some reference size.

No, I now realize that this would create a conflict. The behavioral control system has to be concerned with distance, not size. The output of this perceptual function is a distance signal -- the signal that is fed back to make the hypothesized size be correct.

Do you get the idea? The little control system in the perceptual function receives a signal from higher up that says "THIS IS THE REAL SIZE." The retinal-area signal coming from lower systems provides the retinal size information. The feedback loop then adjusts the distance signal to make the computed real size (the product of the two imput signals, one real and one imagined) match the reference or postulated size. A copy of that distance signal becomes the output of this perceptual function. Now the control system that uses this whole perceptual function can compare the computed distance with the reference distance, and the error signal can actuate lower systems that move the object (or the viewpoint) nearer or farther away until the perceived distance matches the desired distance. Of course the objective distance that an outside observer will see depends on the actual size that the subject in the experiment is hypothesizing.

This can certainly be done on a computer screen, where "distance" has to be imagined anyway. Just show a circle with a diameter adjustable by a joystick or mouse, tell the subject to imagine that it is three feet in diameter, and to move the control handle to place the circle twenty feet away. Maybe, behind the screen in a big room, you could place distance markers, so you could say "imagine that the circle is three feet in diameter, and place it HERE." Then tell the subject that the circle is three inches in diameter, and to place it in the same location. A semi-reflecting mirror could be used to superimpose the screen on the background. Or, quite likely, you could look up the results of an experiment like this in the literature on perception. It's probably been done, if not with a computer.

Well, that got pretty far off the track, but the idea of using feedback in a perceptual function, and of allowing hypothetical aspects of a perception to be supplied by higher levels, may be of general usefulness. It might even become part of the CSG model, if it seems to have explanatory potential. Just a couple more musings...

The computer case has a lot of attributes. There is parallelism of lines, distance from one corner to another, and so on, that can be represented as single signals. These attributes would be invariant with respect to rotation, translation, and size. But to model human perception, we'd have

to think of a way for the system itself to discover these invariances (I'd like to do away with the "teacher" in modeling perception).

One way is through visual scanning and/or manipulation of the object (or the viewpoint from which is is seen). Moving an object from one part of the retina to another will leave certain signals unchanged. Rotating the object or moving the observation point relative to the object will leave others unchanged. The trick, I suppose, is to construct an invariance-recognizer that can construct functions of lower-level signals that yield constant values under these transformations. Why couldn't a perceptron be used this way? Instead of trying to teach it to produce specific outputs by telling it whether it is right or wrong, why not build into it the goal of creating signals that are constant under various manipulations of the object? We wouldn't even have to know what those invariance's are. All we would have to do would be to supply a set of lower-order signals (sensations? edges? corners?) whose retinal locations depended on the way the object is presented, and give the system the ability to command changes of the object in orientation, distance, and so forth. Those command signals (really reference signals for lower-order control systems, but we don't have to model those) would also form inputs to the perceptron. What the perceptron-organizer wants is for the partial derivative of a given signal with respect to one of the manipulation-signals to be zero. That's the only thing you would have to "teach" the system, and even that is really just part of its own basic design.

You certainly can't just start simulating all this, but maybe there are some ideas here you can use. Just give credit to CSGnet if you use them.

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Date: Sun, 16 Dec 90 14:25:16 -0600
Reply-To: "Control Systems Group List (CSG-L)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group List (CSG-L)" <CSG-L@UIUCVMD>
From: g-cziko@UIUC.EDU
Subject: Reference Conventions

Chuck (Tucker) and all others:

Chuck (Tucker's) recent post (Tucker 901214) has demonstrated another way of interacting with CSG-L. Instead of responding to anything of interest as fast as possible, you can sit back, organize your faculty, watch what is happening on the network, and then put together a more reflective synthesis when the spirit moves you and time is available.

He also was the first to make actual references to other postings, as in WTP 12/6 for William T. Powers on December 6. This is probably not necessary when responding quickly to others, but I find it very useful for this "batch" mode of responding. I think that Greg Williams will also find references of this type useful in finding the "threads" for "digesting" our communications for his possible publication. (By the way, we've had several positive responses to his idea, but nowhere near the 500 he wants to make the venture feasible. Are there any other potential subscribers out there?)

Let me make some suggestions in this regard. First, let's keep the net accessible to newcomers by avoiding first-name-only citations. Full names are best since they let others know who the players are, but if it hurts too much (violates a reference level for addressing friends), you can always put the last name in parentheses to show it's just for information as in "John's" (Smith) comment is way off the mark."

Second, since I'm lazy and like to be able to sort dates on my computer, I like the yymmdd format in which today's date is 901216. So to refer to

Bill's (Powers) second posting received today I would cite Powers 901216b (as we do when we cite journal articles and books and someone has two of them for the same year; final letter not needed when only one post exists from that person on that date).

I don't want to spoil the spontaneity of our discussion, but these conventions could make CSG-L more accessible and more comprehensible make us more accessibobble (I like Pogo). Remember, these discussions are not just enjoyable gossip. Every word is being archived and these archives may well turn out to be as interesting as Darwin's much studied notebooks. Let's make it easy for the future historians of science!--Gary (Cziko)

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Date: Sun, 16 Dec 90 13:56:33 -0800
Reply-To: "Control Systems Group List (CSG-L)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group List (CSG-L)" <CSG-L@UIUCVMD>
From: marken@AEROSPACE.AERO.ORG
Subject: Various Topics

Gary -- I will use the referencing system you suggest but right now I just want to make some quick points. I'm still working at 300 baud and I couldn't upload a text file using Kermit (which I can do at work) so I'm kind of frustrated watching the new posts and having no coherent way to respond -- so here is the incoherent way.

Dennis -- I agree that individual liberty is fundamental to a control theory ethics (if there were such a thing) but I also think that one of the most interesting (and calming) thinks that control theory does is help us understand that the behavior of the "forces of antiquity" (an excellent appellation) is an expected consequence of the fact that they are control systems, trying to control variables that they feel are very important (I guess any variable we are trying to control is important to us). Control theory gives us some sympathy for the devil (and these forces of antiquity can be quite devilish if you are controlling for a variable that they think should be kept at another level of not controlled at all). You cannot control the controllers any more than they can, in the long run, control you. I don't know the solution to this mess -- but the one's who think they have one are usually the most obnoxious and dangerous controllers of all. I guess there really is no solution other than education and, after seeing what some educated people think it is important to control I'm not even real optimistic about that. For some reason all this doesn't depress me -- I think that, at a personal level, understanding the nature of control systems has helped me get along with other control systems; and, of course, there is great "spiritual" satisfaction to be had from learning a little, weensey bit about how living systems work. But I would be surprised if learning about control would help reduce the amount of un-necessary conflict in the world. People just want to control (subg sung to the tune of Girls just wanna have fun).

After almost abandoning the Marken effect, I returned to it to see if it really doesn't improve control when then operator has a different reference than the conflicting system. Well, as I had originally thought, it DOES help. It doesn't only of the disturbance is too easy. The Marken effect aiding system seems to help out when

there are high frequency components in the disturbance. So I think I will return to the research on this effect. That was a nice discovery this weekend -- I almost though I had lost my effect!

Bill and Mark (re: memory) I was trying to compose a response to Bill's post about the Tip-of-Tongue effect. Bill says you don't know the word to be recalled until you experience it. But I think there is more. I think we know (have an experience) of when it might be reasonable to "put in the request" to memory for a word and when it is not. In other words, I realized that I experience (know) a memory in a different way than I know an imagined event. I can picture my brother's face and I know it is an image that I am remembering. I can imagine a face that I have never seen before -- just to be creative. I am putting the face together with remembered components but the face itself is not a memory. I think this is what the Tip-of-tongue points to-- some aspect of us that marks images as replays of events that we have experience (through the environmental loop) and other images as those that we have not experienced. Another phenomenon that turns on this distinction, I think, is deja vu. Perhaps this ability is already part of the hierarchy of perceptual control. But I don't think so. I think there must be something in the model that makes it possible to distinguish remember ed from purely imagined events. Schizophrenia might be another example of a phenomenon which turns on a breakdown of whatever it is about is that makes this distinction.

I have no editor -- the "is" above should be "us".

Hasta Luego

Rick M.

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Date: Sun, 16 Dec 90 16:19:54 -0600
Reply-To: "Control Systems Group List (CSG-L)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group List (CSG-L)" <CSG-L@UIUCVMD>
From: g-cziko@UIUC.EDU
Subject: Telecommunications Software (Eudora)

Rick (Marken):

>I'm still working at 300
>baud and I couldn't upload a text file using Kermit (which I can do
>at work) so I'm kind of frustrated watching the new posts and having
>no coherent way to respond -- so here is the incoherent way.

If you are using a Mac at home and your mainframe is running Unix with POP (Post Office Protocol) I have a fabulous telecommunications application for you which was developed by Steve Dorner on this campus called Eudora (Eudora Welty wrote a play called "Living at the P.O") which is free!

Eudora lets you do everything on the Mac. After you've written your messages, you click a box and it dials your mainframe, transfers the mail, and sends it on its way. It also checks your mail for you and downloads it automatically to Mac. You keep your messages in as many mailboxes as you like and can retrieve messages by time of arrival, subject, text strings, etc.

You can also use this if you have a hard-wired Mac at work using MacTCP as the telecommunications protocol.

If you used Eudora, we could even attach documents and application files to our notes and send them to each other via email!

Perhaps others on the net could use this program as well.--Gary (Cziko)

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Date: Mon, 17 Dec 90 02:23:30 CDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Comments: Please Acknowledge Reception,Delivered Rcpt Requested
From: RLPSYU08 <TBOURBON@SFAUSTIN.BITNET>
Subject: Adaptive cooperation

The presence of adaptive control modelers on CSG-L has nudged me into trying something I have avoided since 1988. That was when I began modeling interactions between two people (or two hands, or two models, or a person and a model -- between any two control systems). My articles in Wayne Hershberger's book and in the American Behavioral Scientist issue on CST describe two of the simple interactions that can occur between two control systems -- while each system attempts to control its own controlled relationship, one or both of the systems might interfere with the other.

In 1988, one of my thesis students ran another of my tasks in which two systems cooperate to produce a result neither can produce alone. The task, which is a take-off on tracking, complete with control sticks and cursors on a computer screen, is roughly similar to two people holding a book vertically between them by each pressing the book toward the palm of the other person. Each new pair of people to perform the task goes through a period during which one person, or both of them, adjust their gain factors for performing the task. Initially, one person is often "trying harder" than the other, which leads to unstable performance. Things work best when the two people adopt nearly equal gains, which I model as the traditional "integration factors" seen in so many reports on control-system modeling.

What I wanted to do in 1988, but avoided because I thought the task would be too difficult, was model both aspects of the performance of each person, the relationship-control (which we modeled back then well-enough to account for over 99 % of the variance) and the adaptive change in the integration factors. During the past few days, for a break from reading a few hundred pages of undergraduate psychology essay questions, I sat down to program the adaptive control. It works, far more easily than I expected.

The interaction between the two models is similar to that between systems in my *ABS* article. Two targets move up and down at the sides of the screen, each flanked by a different cursor. Each cursor is controlled by a different CST model like those in the ABS article. Simulated movements by the "handle" of each model affect both cursors. In this setting, the systems (models) can interact in a number of ways, ranging from interference, to cooperation to pure and destructive conflict. So far, I have modeled a version of cooperation, or of "Assisting," in which the system on the LEFT adopts the same reference as that on the RIGHT -- both systems have references to keep the right cursor even with the right target (i.e., for [cursorR - targetR = 0]).

Each model is equipped to sense the relationship between the right cursor and target, to compare the immediate value of the relationship to the reference value, and to multiply any resulting error by its (the model's) integration factor to determine its simulated handle movement. On the screen, the position of the right cursor is: cursorR = handleR + handleL.

In addition, the LEFT model contains a loop with a reference for "seeing the right handle move," i.e., for sensing changes in the simulated position of the right handle. Absence of movement over whichever sampling period I select results in the second loop altering the integration factor for the output function to the LEFT handle -- the model reduces the integration factor by some small amount from its initial high value. (When the integration factor for the LEFT model is sufficiently high, relative to that for the RIGHT, the RIGHT model "sits and watches" the cursor remain where it (the RIGHT model) says it should. Whe the LEFT model "backs off" sufficiently that it no longer eliminates all of the error in the cursor-target relationship, the RIGHT model begins to move its handle and the two of them achieve precise control.)

All of this is set up to display the results on the screen on the same time-scale as runs by "real people."

When people run tasks like these, in which they knowingly adjust their gains to achieve a match in their degrees of influence over shared variables, I have no doubt that they employ several levels of the hierarchy. With the two models, I am not sure. Clearly, the LEFT model has two distinct references, one for a cursor-target relationship, the other for movement of the other model's handle, which it (the LEFT model affects only via the cursor-target relationship on the right. But I am not sure this need be thought of as two levels of control in the model.

If anyone is interested, I will let you know what happens when both models adapt. I already know I will have to differentiate between them on features such as their sampling intervals, or the increments or decrements they apply to their integration factors, else both will make the same adaptations simultaneously. And the time may come when I will give in and use transport lags, which could differ between models, but I want to employ the oldest and simplest version of the model for as long as possible.

Best wishes,

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Date: Mon, 17 Dec 90 09:32:04 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: g-cziko@UIUC.EDU
Subject: S-R Again

Last week I ran through parts of Powers' Demo1 and Demo2 with a colleague who uses an operant conditioning paradigm in his work with mentally handicapped students. He found it of some interest, but attempted to assimilate what he saw into an S-R framework (which is perfectly natural and understandable). All you have to do is add feedback and a reference level!

For feedback, CT feedback is just a "smoothing out" of the S-R-S-R, etc. chaining that he already considered important. You still have a response to a stimulus, even if there is constant feedback.

Second, it may be useful to have reference levels to explain this tracking behavior, and perhaps all behavior, but what are reference levels other than the result of prior experiences and reinforcement for certain behaviors. He pointed out that he wouldn't even do the tracking task if he had not been rewarded in similar situations in the past to follow the instructions of a colleague in situations like this.

This got me thinking again about how control theory differs from S-R theory. Couldn't you say the following from the perspectives of both?:

"We have inherited certain needs. We find ourselves in an environment in which these needs can usually be met, but we must figure out how to get them. Attempts which are successful in meeting these needs are somehow remembered, while those unsuccessful are eliminated. The resulting accumulation of knowledge is based on initially blind attempts to satisfy our needs. When our needs are satisfied, there is less of this blind attempt to learn a new way to satisfy a need (the E. coli doesn't need to tumble). All of our behavior (and knowledge) is the result of the interaction between our biological inheritance (which determine our basic needs) and our own particular experiences. Since we all share the same basic human needs, it is the latter (experience as stimuli) which is primarily responsible for the differences between individuals and between cultures."

Yes, I realize that there is a big difference between behavior as the control of perception and perception as the cause of behavior, but I'm concerned here about presenting control theory in such a way as to disturb an existing S-R schema sufficiently to result in reorganization rather than assimilation.

Any ideas? Can people share experiences with attempts to "disturb" S-R types? The Old Behaviorist Friend of two months ago is still the same, only a little older. My students who have taken the time to do some reading on control theory find it all quite exciting and insightful. My colleagues so far don't budge, just dig deeper trenches. Don't tell me they're too old. Look at Phil Runkel.--Gary

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Date: Mon, 17 Dec 90 10:52:57 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: Joel Bennett Judd <jbjg7967@UXA.CSO.UIUC.EDU>
Subject: applying CT

"Psychologists have been fascinated by illusions the way a rabbit is fascinated by a snake: they threaten the idea of the objective observer. And illusions are one of the few phenomena of individual behavior they can reproduce reliably in the laboratory. I hope, though, that we're going to focus on the big obvious control problems pretty soon, such as: why don't many students like school? What are teachers and students controlling for? Would it be a good idea to teach children and/or teachers control theory? Are there levels of control that can be identified during development, so we don't try to teach students to accomplish things they're not prepared to

perceive? Can we use control theory to define a "natural progression of learning?"

I second the motion to discuss these kinds of questions.

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Date: Mon, 17 Dec 90 10:06:58 -0800
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: marken@AEROSPACE.AERO.ORG
Subject: Teaching behaviorists

Reply to Gary Cziko post of 12/17/90 on teaching control theory to behaviorists

Ah, the problem of teaching control theory to behaviorists. I sympathize with your interest in finding a way to present control theory so that it doesn't lead to an adverse reaction in behaviorists. But I think that it is just impossible to communicate the essential features of the theory without creating problems for a behaviorist (or cognitivist). My resolution of the problem is contained in my article in Behavioral Science (1988, v. 33, p 196) called The nature of behavior:Control as fact and theory. Behaviorists (and cognitivists and, psychologists in general, I guess) are just not studying the same phenomenon that control theorists are studying. Behaviorists try to understand "behavior", by which they mean any observable result of an organism's actions; control theorists try to understand control. Control refers to intended or purposively produced results of action. So the difference between control theorists and behaviorists emerges at a point well before the point that behaviorists imagine there to be a problem.

Behaviorists talk about behavior as though it were an objective phenomenon. In fact, behavior is a subjective phenomenon -- it is controlled perception. My mindreading experiment illustrates this fact. But the subjectivity of behavior is a conclusion that comes from theory; the theory designed to explain control. The theory is not relevant until we realize that control is a fact. The fact of control (as I note in the article) is evidence by the fact that certain results of an organism's actions are produced consistently despite inconsistencies in the environment in which they occur. We (CSGers) call these inconsistencies disturbances or constraints. The upshot is that consistent (controlled) results occur because organisms vary their actions appropriately so that, when their effects are combined with prevailing environmental effects a consistent result occurs. In the same article I also point out the difference between control and superficially similar phenomena such as equifinality.

Once you understand that control is a fact and that behavior (at least the behavior we care about) is control then we need an explanation for that phenomenon and that's where control theory comes in. Behaviorism is just irrelevant, really, because it doesn't deal with control but with the observed results of control. Behaviorists take advantage of the fact that organisms control -- for example, they know (unconsciously) that organisms control for certain kinds of experiences -- they call these experiences reinforcements. And they know how to set up the environment so that the organism has only one way to produce these experiences for itself. They call this shaping or stimulus control -- but these ideas can be ignored because we already know that what we are seeing is control.

The mouthings of behaviorists are as silly as the mouthings of a flat earther. How do you convince a flat earther that he (or she) is living on a globe? Well, if they want to believe in a flat earth (like for biblical resons) then there is probably not much. The behaviorist wants to believe that behavior is just emitted output because they want to imagine that they can shape

behavior -- they want to CONTROL it. Given this goal, there isn't much one can do to teach the behaviorist the control perspective is to write about it and discuss it with them politely. (And show them the Demos, but don't expect it to help much. A flat earther has an explanation for ship masts disappearing as the move away from shore also).

The only hope for change, I think, will come from trying to get the behaviorists to look at the fundementals -- and this means getting them to understand the nature of the phenomenon of control as it is evidenced in the behavior of organisms. Again, this is something they are not used to looking at and they will probably consider it irrelevant. After all, they don't really need to understand control. They can control their own perceptions from the context of their current perspective; the same is true of the flat earther. Euclidean geometry works great when you want to build a house. The problems arise when you want to do bigger things -- like fly from LA to London. Then its good to know that a straight line is really a curve. The behaviorist is OK with their error (stimulus causes responses) until they want to do something bigger, like help people get along with each other or help people deal with life more effectively or understand why they would even want to do what they are doing in the first place.

Note to Gary on the Modem issue:
I don't know if my Mac sends the correct comm protocol but I am connected to a unix system. So if you can send Eudora it would be nice to have a copy. thanks for the suggestion.

Regards

Rick M.

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Internet:marken@aerospace.aero.org
213 336-6214 (day)
213 474-0313 (evening)

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Date: Mon, 17 Dec 90 18:20:31 CST
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: Bill Powers <FREE0536@UIUCVMD.BITNET>
Subject: Modems, miscellany, behaviorists

Rick Marken (901217) --

I think there's a step beyond the "keep your cotton-pickin' hands off my control systems" prickle. We do have to live together and therefore we have to respect each others' wills as well as defend our own. This often means letting other people control our behavior. Look at the reference number above: Gary Cziko (901216) "made me do that." But how could he make me set a reference level for writing dates in a particular way? Mechanically, physically, he couldn't. I wrote it that way all by myself. But I chose a way of writing it that was suggested from outside me, not because Gary has more clout than I do but because I basically don't care how I write dates and I do care about making our system of communication work better. I like communicating with Gary and all the rest of you, and I like seeing myself as one member of a bunch of people who are trying to support each other and encourage each others' efforts toward a noble end. Noble ends are the ones I like, of course. The higher-level system adjusts the lower-level goal.

Cooperation is harder to achieve than individual action, especially under The Axiom [Each person's will is entitled to respect, another way that Hugh Gibbons has put it].

As to your 300 baud problem, what communication package are you using on what machine? I assume that you've set up your package for 2400 baud and that the phone number you're dialing isn't dedicated to 300 baud.

I did come across a similar problem in dialing up U of Illinois at Chicago. That line first responds with a 300-baud tone, then if it gets no result, a 1200 baud tone, then finally a 2400 baud tone. If you have your package set up for "auto baud rate", it will see that the answering modem wants to talk at 300 baud, and will obliging switch to 300 baud instead of whatever you had originally set up. The solution is to turn off the "auto baud rate" option if there is one. This is software, not a modem parameter. Also, I turn off "auto answer" in the modem setup screen (include "S0=0" in the modem setup string). This disables auto answer, so if you forget to turn off the modem (or computer), or someone calls you while you're computing, incoming calls won't result in a whistle in the caller's ear.

Concerning communicating with behaviorists, see below:

Gary Cziko (901217a) --

I don't think we can ALL sit back and survey the network activity, responding only in batches. Think about it. It's good that some people do it. I'll just continue to babble until I get started on programming again.

Chuck Tucker and Clark McPhail --

I have actually made a start on Crowd Version 3. The "Seek centroid of group" control system seems to be working. Other items on wish list coming up. Slowly.

Tom Bourbon (901217) --

The adaptive control stuff is exciting. I assume that there are disturbances as usual -- you didn't mention them in your "equations." One way to differentiate between the two systems is to subject each one's handle effects to a different independent disturbance.

For the adaptive part, would you have time to try the "E. coli" method? Set up a reorganizing system as follows:

1. Initialize a LastError variable to 0. Initialize a timer T to 0.

2. Sense the absolute value of error signal (AbsError) in the system to be adapted.

3. ErrorChange := AbsError - Lasterror.

4. Lasterror := Error

5. Set up a timer so that T := T + ErrorChange. If T < Tmin then T := Tmin.

6. When T greater or equal to a fixed magnitude Tmax,
    a. reset the timer T to 0 and
    b. add or subtract (at random) a small increment to the integration factor: i.e.,
       if random(200) >= 100 then intfact := intfact + Correction else
        intfact := intfact - Correction.

This should result in a random walk of the integration factor, biased so it approaches the value that will minimize the absolute error. With this

method you don't have to propose any specific relationship between the integration factor and the error. Of course you have to play with Tmin, Tmax, and Correction to get the time-scale right.

Correction: in the "distance computer" (my 901216b) the retinal size should not be area, but linear spacing of points on the retina.

Communicating with behaviorists (Gary Cziko 901217b)--

I would like to know how your behaviorist colleagues explain the part of Demo 1 in which the person's action goes through two wandering pulleys before affecting the controlled variable. I claim flatly that no S-R or S-R-S-R or SRSRSRSRSRSRSRSRSR... approach can possibly explain that result (particularly with the pulleys covered by a piece of cardboard). I challenge any behaviorist to come up with an analysis of that behavior that will, by its own rules, generate it. I mean it, Gary. I want to hear what they come up with. I am extremely tired of this dance between serious modelers who can create explanations that work in detail and the arm-wavers and word merchants who still think they can sell 19-Century models in a late-20th-Century world. In the aforementioned part of Demo 1 there is a clear and simple behavioral phenomenon. Control theory explains it and can generate a model that accurately reproduces the behavior. Behaviorists can't. You can communicate this challenge to your behaviorist colleagues, suitable abridged for academic consumption. Or unabridged.

You can also say this to your colleagues: In explaining a phenomenon, it is better to explain what you observe than what you imagine. The suggested S-R chaining in Demo 1 is entirely imaginary. It does not happen. The stimulus changes while the response changes, and vice versa. There is no alternation, I guarantee it. The input is changing while the output is changing; even during the transport lag this is true. If this doesn't sink in, stand very close to the person's right ear and say loudly, "THE INPUT AND THE OUTPUT ARE CHANGING AT THE SAME TIME." If that doesn't do the trick, you're talking to a deaf ear and you might as well forget the whole thing. The other ear won't work any better.

Let THEM come to US when they finally figure out that the world is passing them by. You might, perhaps, detect that the last post did not leave me in a totally tolerant frame of mind. I've had 35 years of this crap and that's enough.

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Date: Mon, 17 Dec 90 21:00:58 EST
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: Dennis Delprato <USERXEAK@UMICHUM.BITNET>
Subject: From Punctate to Continuous Response Measures

REALLY FROM Dennis <DELPRATO@UM.CC.UMICH.EDU>

Elliott Bonem recently has brought to my attention a book by J. M. Notternam & D. E. Mintz (N & M--thank goodness it is not "M & M") entitled _Dynamics of Response_ (New York: Wiley, 1965). I recall looking at this book years ago, but at that time, I categorized it as a report of some off-beat research in operant psychology. Our discussions of control theory led Elliott to detect the possible usefulness of a reassessment of it. I offer the following comments on N & M.

First, it may be that N & M provide background work on instrumentation for establishing one animal (i.e. nonhuman) preparation for control theory research. Although nonhuman

research is best not viewed as yielding observations and data that are analogous to those pertaining to human behavior, I suggest that such research can make important contributions to behavioral control systems science. The key aspect of N & M's instrumentation is that they used a strain-gauge manipulandum that permitted _continuous transduction of the force_ rats applied to it. Clearly, this technique for transducing the animal's motoric activity is well-suited for control-systems research.

Second, the unorthodox measurement procedure took N & M to a most interesting interpretative/theoretical step. The authors point out that operant researchers using conventional recording equipment, whereby a "response" is defined by contact closure in a microswitch, are not interested in these responses per se. Rather, the basic datum is taken to be the the length of time elapsing between a response occurrence ìswitch closure+ and the response occurrence immediately preceding it--in other words, rate of responding, or response frequency. N & M's research was designed to explore if there are "significant experimental or theoretical questions best answered by examination of the dimensional characteristics of the response itself rather than by study of the time interval between responses" (p. 3). They concluded that "time-between-occurrence" measures do not fully illuminate a number of issues.

Moving to the end of the book, we find the following:

> "In the conventional ìoperant research+ situation, the use of a microswitch to record responses indirectly gives rise to the view that the experimental organism behaves as if it were a switch. What originated as a counting convenience becomes an implied characteristic of the organism.
>
> The conception of lever-pressing behavior thus tends to take its form from the microswitch; accordingly, the response is conceived of as being 'ballistic,' 'open loop,' and nondimensional in nature. The research here reported leads us to conjecture otherwise; we surmise that the response, rather than being ballistic, is regulated by feedback and is therefore closed loop rather than open loop." (p. 264)

Control system theorists will no doubt be amused by N & M's attempts to concoct feedback loops; however, when the history of behavioral control systems is written, I suggest they deserve recognition as protobehavioral control systems theorists. Furthermore, some future researchers may pick up on their idea for transducing the animal's operations on its environment and move forward the study of organisms as control systems.

Dennis Delprato
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Date: Thu, 13 Dec 90 14:03:19 GMT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: Chung-Chih Chen <chen%arti1@VUB.VUB.AC.BE>
Subject: I have no purpose

Bill Powers, Gary Cziko and others who think I got a funny idea:

I have no purpose of starting another debate on the universe and

intelligence. I just want to show that
there is a new book which may prove that my idea is
not funny:

%A M. Kafatos
%A R. Nadeau
%T The Conscious Universe
%I Springer-Verlag
%C New York
%D 1990

The authors are two physicists from George Mason Univ. They postulate
that the universe is conscious by arguing about the cosmological models.
It seems that my idea has got some verification. (Of course, conscious
is intelligent. Right?) I am trying to get this book eagerly. But I
haven't found it in Brussels.

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Date: Sun, 16 Dec 90 08:46:51 GMT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: Chung-Chih Chen <chen%arti1@VUB.VUB.AC.BE>
Subject: Mark Nelson

There seems to be problems in the email path reaching me. I receive
today emails which were sent one week ago and one day ago by the list.
And I haven't seen the emails which I sent 3 and 1 days ago.

Mark Nelson:

> This is correct. A higher behaviour utilizes all existing agents as well
> as the new ones introduced specifically for it. A small redundancy exists
> in the set of skills contained by each agent. But this redundancy is the
> strength of my system as they are used in the context of adaptation. That
> is, if the skills for the current behaviour fail to generate a satisfactory
> response then the skills of lower behaviors may be used.

It seems that each agent has several skills. I still don't understand
why you need agent and skill as two different things? Why don't you use
one agent as one skill? Isn't that simpler?

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Date: Mon, 17 Dec 90 22:57:31 EST
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: BARKANA@DUPR.OCS.DREXEL.EDU
Subject: A late response: close-loop, open-loop, etc.

Hello, everybody, it was hard coming back from Paradise, but any good thing seems to have an end ("apparently?").Then, I had to grade some finals, the most difficult task I know, for a guy who does not want to control human beings.

Again, trying to read and understand the discussion that is going on, I have the feeling that I brake into a subject where i cannot call myself even a novice. I am very interested in your discussion, because I want to use any information related to biological intelligence for our "engineering" intelligent controls. I am pleased and surprised with the nice and patient tone of this discussion, and with the detailed explanations I get.

Bill Powers , Rick Marken, Gary Cziko, and everybody:

1. My position in control is actually very similar to yours.
I am an engineer, a control one, and mainly, a feedback control one. In what I called "Simple Adaptive Control" I have lots of fights with some colleagues, BECAUSE I show that some simple feedback loops with "appropriate" adaptive gains can perform much better and can me more dependable than many sophisticated schemes.
2. I cannot afford mixing control with philosophy. For the control job I take the external "reality" for granted. When my car does not crash into the wall or another car, I deduce that my sensors approximate interpretation of this "reality" was not very bad, which I am not sure of if I am drunk. In both cases I control my sensorial perceptions. I am glad that I am around here and now, even though this phenomenon was not present some time ago and will vanish sometime in the future, an in spite of the fact that I am "mostly void" and from what is left, more than 90% is just plain water.The car that comes against me may discontinue this experiment too soon, even if it (the car) is also "mostly void."
3. I take for granted that you are there, and that I read is the translation of your thoughts, even if my perception may be very poor. I would not even dare to think that the partners are "mostly void." This is a second or third order approximation that is not needed for most of my jobs, as well as Math is not needed for most of your tasks. I don't think one uses Einstein's formulae to calculate the speed error, in spite of the fact that they are more precise.
4. As I understand, the great idea of Bill Powers is the observation or the discovery of FEEDBACK control in behavior, as opposed to simple SR interpretation. I could not agree more.
5. I may make people angry, but because feedback is closed-loop, it is not very important for me how you call what. I only need the dictionary.
6. Especially for Rick Marken: I do not confuse effect and control. I assume that in order to control, one must be able, first of all, to affect, and in a desired mode, too. If the thermostat can not control the temperature in the room, how can it control the measured temperature, which is its input? I hope you do not translate "is affected by" into "controls."
I assume the thermostat system is designed to perform a reasonable job, as our behavioral control-loops are "designed" or have developed reasonable tasks. The others are not around to testify.
7. One of my problems is that I am, in general, in the middle, and I try to understand all sides, and get the main point of the opposite idea. I am afraid that this group, trying now to explain every behavior as closed-loop, may ignore some very nice and intelligent open-loop controls, based on the splendid property of the brain: learning. Garry Cziko remarked it today, but still tries to explain everything as a closed-loop system. Yes, closed-loop is dependable, and when one learns skating, one uses very stiff closed-loop control. With time, after one learns one's own behavior and the response of the skates, one uses much of an open-loop control, based on the learning process and modeling of this behavior. Closed-loop control is still there, but not alone, and not that stiff as before ("lower gains"). A predictive closed-loop control is also existent, comparing the predicted desired position to the actual present position, but this loop is weaker and weaker with the training. How much is open and how much is closed is only a question of control gains. It is not a cause-effect interpretation. It is very intelligent open-loop control. In my humble opinion, one misses something if one ignores

this aspect of control, in particular when learning (I wish I knew how) is involved.
8. About "controls output" or "controls input." As I said, it is not very important (for me), if I know what we are talking about. Yet, much of your conclusions is based on modeling. Control experience tells me that I cannot derive conclusion about the behaviour (or transfer function) of components of a control loop from the behavior of the closed loop, BECAUSE of the nice property of feedback, namely, lack of sensitivity to variations in the parameters of various components.
Therefore, to find the transfer-function of the CLOSED-LOOP, I must first OPEN the loop, test the input-output behavior of the OPEN-LOOP, when any variation of the components expresses itself one-to-one, and only then I close the loop. The Math also shows that analysis of (at least gedanken) open-loop system provides conclusions relevant to the behavior of the c closed-loop, but this is only secondary here. Anyway, this may explain why I have an input and an output (or more outputs on the way, but only the controled variable is the output of interest). Now, I measure the output, and the result of measurement becomes the feedback input, and the other input is the reference input. Along with the closed-loop scheme, which is mainly identical to Powers' scheme, I add a function of the desired (the reference) input that drives directly the control system, without passing the comparator.

Chung-Chih Chen:
I am sorry if my first remarks on Universal Intelligence came together with all other remarks that discouraged you. I thought I was joking. To correct the impression to some extent, I would like to remind all of us that the best proof on the existence of Universal Intelligence is the Thermos(?!)
"How come?"
"Well, you put it in cold, it keeps it cold. You put it in hot, it keeps it hot !"
"So?"
"Well, how does it know?"

Looking up to my long letter, I think that I aded enough clutter for one day.

With best regards, Izhak.

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Date: Tue, 18 Dec 90 11:49:04 EST
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: "CHARLES W. TUCKER" <N050024@UNIVSCVM.BITNET>
Subject: I will reflect on the latest and write soon but just a

note on another technique that I use for the NET since I don't get my messages back to me. I type my posting on a disk with Wordstar then convert the file to ACHII and then put the file on the mainframe and send it to y'all. Thus I have a WS file and a file on my mainframe that I can refer to in the future. I also have them available to send to others not on the List when they have an interest in some of my ideas and a copy that I can use for papers that I may write or to mail by snail mail to others. Perhaps you think that this is too much of a fuss but I often find myself asking myself "What did I do with that comment I wrote last month?" Now I have a copy as a reference.

Chuck
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Date: Tue, 18 Dec 90 11:17:13 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: g-cziko@UIUC.EDU
Subject: S-R vs. CT

Responding to Rick Marken (901217) and Bill Powers (901217):

I noticed that neither of you addressed my "quote" of 901217 which seems to me could be accepted by both S-R behaviorists and control theorists. So let me repeat it here:

"We have inherited certain needs. We find ourselves in an environment in which these needs can usually be met, but we must figure out how to get them. Attempts which are successful in meeting these needs are somehow remembered, while those unsuccessful are eliminated. The resulting accumulation of knowledge is based on initially blind attempts to satisfy our needs. When our needs are satisfied, there is less of this blind attempt to learn a new way to satisfy a need (the E. coli doesn't need to tumble). All of our behavior (and knowledge) is the result of the interaction between our biological inheritance (which determine our basic needs) and our own particular experiences. Since we all share the same basic human needs, it is the latter (experience as stimuli) which is primarily responsible for the differences between individuals and between cultures."

I agree with the points made by Marken and Powers, but the level of sophistication needed to explain the effects in Demo1 is not often approached in the types of discussions I usually have with colleagues and students. The paragraph above is more typical of the level of discussion, and this level of discussion I'm not sure how I can argue that S-R is wrong and control theory (CT) is better. Perhaps at this level, the differences are not important, as the difference between Newtonian and Einsteinian physics is not important at normal subluminal velocities. If this is the case, fine, I will just need to move the argument to phenomena that S-R can't handle. But if there is a way even at this level to make an argument for CT, I'd like to be able to do it.

Maybe other CSGers would find such an argument useful as well.--Gary

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(This file must be converted with BinHex 4.0)

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Date: Tue, 18 Dec 90 11:20:25 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: g-cziko@UIUC.EDU
Subject: Inadvertent Attachment

I'm sorry that I inadvertently "attached" a file to my last posting (Cziko 901218a). I don't know how this will show up (if it does at all) on must people's systems, but if it does, the attachment (a list of addresses) should just be ignored.

Sorry for any inconvenience. Even an e-mail whiz like me can make mistakes!--Gary

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Date: Tue, 18 Dec 90 12:34:48 -0800
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: marken@AEROSPACE.AERO.ORG
Subject: behavior and open loops

Responding to Gary Cziko (901218):

Re: Your description of an agreeable description of learning for sr and control theorists:

"We have inherited certain needs. We find ourselves in an environment in which these needs can usually be met, but we must figure out how to get them. Attempts which are successful in meeting these needs are somehow remembered, while those unsuccessful are eliminated. The resulting accumulation of knowledge is based on initially blind attempts to satisfy our needs. When our needs are satisfied, there is less of this blind attempt to learn a new way to satisfy a need (the E. coli doesn't need to tumble). All of our behavior (and knowledge) is the result of the interaction between our biological inheritance (which determine our basic needs) and our own particular experiences. Since we all share the same basic human needs, it is the latter (experience as stimuli) which is primarily responsible for the differences between individuals and between cultures."

I guess the only thing I would ask about this is "what is knowledge/ what is behavior"? How can I learn to satisfy a need when the enviromental obstacles to satisfying it are always changing. I'm comfortable with the overall drift of the paragraph, but it really doesn't say anything specific enough to generate much disagreement. If the above is all the behaviorists believe then they are so vague that there is really no basis for looking for alternative explanations. If you want to show a behaviorist why they are wrong you have to get them out of the clouds. Ask them how an organism can do anything consistently at all; how can a rat always manage to press a bar in order to satisfy its presumed need when the rat is always in a diffeent orientation relative to the bar. How does a person manage to consistently take a drink to satisfy thirst when the drink comes in containers of different sizes, or in no container at all. These consistent results must always be produced in different ways. How does the organism manage to always use the appropriate ways -- almost always ways that s/he

has NEVER USED BEFORE. I am typing these words (which I have typed before) using patterns of muscle tensions I have never used before because of the funny
way I am sitting. But I have no problem producing the results I want using actions I have never used before. How do I do that? Are the stimuli guiding me? If so, HOW!! Is it because I have been previously successful in producing these results? If so, how does previous success insure that I will know how to produce the previously successful result under completely different circumstances? It's the supposedly little, dry questions like this that lead to the monster conclusions.

So just ask a behaviorist what it means to know how to do a behavior. When you get into the details, it turns out that what you are doing when you are behaving is producing intended perceptions. But, if the behaviorist does not want to look at the details then there is not much you can do. If the behaviorist is satisfied with a statement like "what people do is a product of both stimuli and genes" then that's the end of the conversation. It's like saying "objects move because of cause and effect".

Izack (901217): If you know of any example of control by an open-loop system then I want to know about it!! And I would really like to know how it works. I claim that, if a variable is demonstrably under control then that variable is part of a closed-loop negative feedback loop. I think your examples
of open-loop control are just higher order variables that are under closed loop
control. For example, control is better in pursuit than in compensatory tracking. This is usually explained in terms of open-loop control -- the person learns to "predict" the position of the target in pursuit tracking and makes "open loop" movements in anticipation. To the extent that the prediction is correct then control appears to be better than in the compensatory case where the temporal course of the target is invisible. I claim that the "prediction" is just a higher-order controlled variable - like a transition or sequence - and, to the extent that prediction works, it is really a result of the subject's ability to detect regularity in that higher order variable. The subject is then controlling a transition, not just the instantaneous configuration of target-cursor discrepency. The fact that this is what is occuring can be tested. If the prediction is open loop then failure of the prediction should result in no adjustment -- after all, open-loop means not monitoring the consequence of the output. There are experiments, using regular,
predictable patterns of transitions as targets which show clearly that, when the pattern is changed (from a circle to another pattern) there is a clear adjustment to this change. The fact that the subject was controlling the sequence of transition rather than just the target/cursor configuration is evidenced by the rather long time it takes to makes the switch -- about 400 msec.

Still, I would be very interested in an example of what you consider to be open loop control.

Regards

Rick M.

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213 336-6214 (day)
213 474-0313 (evening)
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Date: Tue, 18 Dec 90 18:47:10 CST
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: Bill Powers <FREE0536@UIUCVMD.BITNET>
Subject: Good behaviorists, Conscious Universe, Control

Dennis Delprato (901217)

On the contrary, Dennis -- it's great to see that Notterman and Mintz were capable of breaking out of the SR concept. I thought that the quoted paragraphs contain a brilliant analysis of the influence of an experimental method on the way the behaving organism is understood. From the dim mists of memory there comes a recollection of a paper by Notterman, Filiota, and Andri-something on obesity in rats, showing that they defend their body weights like control systems. That is probably totally garbled and I can't remember where or if I used that reference. I might even have written to Notterman back in the sixties, but nothing is left from that period of my life. If he's alive, he might be a person worth contacting, together with his colleagues. That kind of behaviorist I can take.

Mary points out that when we do experiments that measure control parameters, behavior "tends to take the form" of control systems. We, too, are applying a model. But I think we can argue that our model has been more thoroughly tested and given a chance to fail.

Notterman et. al. were (are?) certainly prototypical control system theorists. They probably had (have) problems with their colleagues, as control-system theorists usually do.

By the way, their experiment with strain-gauges and rats is exactly the one I proposed to do for my master's degree in psychology, in 1960. I was told that it was too unorthodox, and anyway there wouldn't be anyone who could sit on my committee for that sort of work. That's when I gave up the idea of becoming a psychologist and went to work for the astronomy department at Northwestern.

Chung-Chih Chen (901217) --

Gary Cziko can send you the archives for the missing week, or tell you how to access them.

I'll try to get hold of that book, but if you could see my face you would know that I am prejudiced against titles like that. At the moment when a prize-fighter recovers consciousness after being knocked out, would you say he is intelligent? But go ahead and try to convince me. I've been known to change my mind (no I haven't -- yes I have -- no I haven't ...).

Izhak Bar-Kana (901217) --

Paradise seems to have been good for you. Welcome back to Winter.

Yes, it's hard to convince some people that a simple control system can accomplish more than many extremely complicated approaches to the same task. One of the great difficulties is getting people to think in terms of continuous variables, isn't it? The digital revolution really brainwashed everyone. Even in electronics, technicians are happy to learn digital circuits because they're so easy to understand -- but they "don't do analogue." They barely understand Ohm's Law, and most of them don't even know what "impedance" means. So much for the great leap forward.

I completely agree that the best attitude in dealing with control system design is that of the realist. You can't play the piano, either, if you're wondering if the keyboard is really there.

I don't think I agree with you about the gradual progression from closed-loop ("stiff" skating) to open loop. But we can save a lot of detailed arguments if you will just get my book, "Behavior: The Control of Perception", and go through it. It should be in your University library, or obtainable on interlibrary loan. Published by Aldine, 1973.

Concerning temperature control: I disagree with your statement that a thermostat must "control" room temperature if it is to control the measured temperature, the input. It must be able to increase and decrease the heat content in the room (that is what I mean by "affect" the room temperature), which in turn affects the temperature at the sensor. But the temperature at the sensor is affected by other things, too, not just by the furnace. If something cools air near the sensor, the average room temperature must be raised higher than the set-point in order to bring the sensor back to the set point. If you think in just a little more detail about physical processes in the room, I think you will see that there are temperature differences in various parts of a room, and that there are many variable sources of heat and losses of heat that alter temperature in various places in the room. Only the temperature of the sensor itself is controlled -- that is, kept near the set point.

Did you get my post with the diagram in it (901208)? The furnace provides the "output quantity" which is measured in BTU per hour. That heat output enters the "environmmental link" along with heat from other heat sources and negative heat from causes that drain heat from the air (through walls, doors, windows, etc). The temperature at the sensor (input quantity) is the net result of furnace output added to other sources and losses of heat, integrated over time. The realization that only the input is controlled by a control system (and not just the idea of feedback) was my "great idea." That's the meaning behind the title of my first book. You already understand control theory -- just think about it, you'll get the idea. It's so simple that it took me three years or so to understand it. If you understand it in less time, that will show that you are smarter than I was.

Best to all -- Bill

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Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Comments: Please Acknowledge Reception,Delivered Rcpt Requested
From: RLPSYU08 <TBOURBON@SFAUSTIN.BITNET>
Subject: 1991 Meeting

From Tom Bourbon.

The 1991 meeting of the Control System Group will be at Ft. Lewis College, Durango, Colorado, 14-18 August 1991. This is a change of date and location from those discussed in the fall. We were unable to locate adequate facilities in Eugene, Oregon. Attempts to locate an alternate site that would be available in October also failed.

There are many positive reasons for meeting at Ft. Lewis College. The cost for room and board is remarkably low and anyone who wishes to do so will be able to bring their spouse, friend or family at an economical rate. Details of the costs will be sent to all CSG members in the February newsletter, but a postcard with a preliminary announcement will be mailed next week.

If you are not a member of CSG and want to join, send $25.00 (US) to Control System Group, 1138 Whitfield Road, Northbrook, Illinois 60062. (Our corporate headquarters are in the home of Bill and Mary Powers.)

If you are outside the United States and would like to attend the meeting, I will try to help, but you must take the first step. Let me know if your government, or any other body, has funds that might support your travel. Also, give me any names and addresses, phone numbers etc. that I would need to contact on your behalf. For my part, I will tell the appropriate person or persons that we are a real organization, that we are scholarly and scientific (some will debate that claim), that we plan to meet, and that we want you to attend.

There will be ample meeting room at Ft. Lewis, with a separate, secured, room for computers and other equipment. I will share other details when they are available. Make your plans, and be ready to register by the deadline of 1 July 1991.

Best wishes to all,

Tom Bourbon <TBourbon@SFAustin.BitNet>
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Date: Wed, 19 Dec 90 12:39:54 EST
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: BARKANA@DUPR.OCS.DREXEL.EDU
Subject: Open-loop, Closed-loop control.

Rick Marken:
You are so enthusiastic about what you believe in, and work so hard on it! This is the way to results, even if at this stage or other one is wrong, and you are not wrong.
Did you miss the start of my letter? I AM a feedback control guy and I do not agree to give up the closed-loop, not even for the sake of this discussion. I only claim that closed-loop does not explain everything, and I would dare to say, in particular when brain, learning and intelligence are involved.
A curiosity: only recently I had an argument with a distinguished colleague, whose argument was: I know the desired output of my plant, I know the plant perfectly (the transfer function) therefore I can use this desired output as an input to something that performs exactly the inverse of the plant. (By the way, for everybody, the "plant" or the "process" is what we want to control, but is already there, given. For example, the motor, the ship, the tank. We can only add sensors and the "controller.") This way, the output of the plant will do exactly what I want.
And then, I say: My friend, this is too idealistic, one can never know exactly the plant, the disturbances, etc. Furthermore, a transfer function followed by its inverse is not the equivalent of 1. (There is a problem of non-controllability, I don't want to elaborate). One cannot control without the closed-loop.

As you see, it is peculiar that I am now in the position to convince you that one can control without feedback.
But now, let us go on from his idea: Assume, for a moment, that we know EXACTLY the plant. We can then design a controller such that the plant will perform exactly what we want (Not necessarily "exactly" but in a satisfactory way). If we do not know, let us try to learn about it, or to "identify." Once we identify, we can design our controller.
Now I ask, how can one be sure that the identification works correctly? And how can I be sure that the plant will not be destroyed before the identification is finished? And what if the identification process is corrupted by noise?
All these thoughts occurred when I wanted to design some controller for a manipulator. It is a nonlinear system, and because of that, the load varies a great deal. If one uses only the high gains needed in difficult situations, one only amplifies noise in other situations. If one uses low gains, then performance cannot be obtained. Then, based on very

little prior knowledge on the manipulator, I build a simple adaptive controller (CLOSED-LOOP) such that the gains move up and down as a function of the tracking error, and the performance is quite good. However, since I do not use knowledge, the adaptive gains "work" very hard. Therefore, in parallel with the controlled plant, I use an identifier. The SAC (simple adaptive control) guarantees that no disaster will happen, even if the identification does not work properly. However, when the identification is correct, the controller based on it takes over and my adaptive gains (of SAC) decrease, and may vanish if they are not needed. The closed loop is there, and if the tracking error tries to increase, it will push it back. YET, there is a signal, directly from the input reference to the plant, and for most situation, is now the only signal that controls the position of the manipulator. Is it not enough? Is it not capable of accounting for disturbances? Is it a bad control system? A bad control system is just that, it does not become a noncontrol system. And now, because my closed-loop gains are adaptive, even when they are called to correct for uncertainties, they do it at much lower values, usually, than before the addition of the open-loop control.

I wish I knew how the brain does its modeling and learning. I think that the identifier must use low gains for identification ("slow identification" or "long-term memory") so that it is not much affected by nonrelevant transient, and only stores relevant knowledge. The SAC must be fast, to get the gain needed when it is needed.

Bill Powers:
Now the argument is very close. What you call the input we should both call the "controlled variable." If I measure the temperature in the neighborhood of the sensor, then this is the controlled variable. However, this is not the end of the story. A good control system would be careful with its sensors and (as organisms do) would use some redundancy, measuring the temperature in various points, thus maintaining some relevance in the measurements, either by averaging the various measurements, or even eliminating the unusual input. Furthermore, a lot of noise might have been added to this measurement, and a close look would reveal a lot of filtering used to "ignore" this part of the input, and pass only the signal that is relevant to the controlled variable. Only a very primitive control system would just respond to any signal (or sensation) received as input, and respond to any spike of noise that comes from who knows where. There is indeed the danger that the control system might respond to any input signal, even if it is not related to the designed control variable, but a good control system takes care of it, sometimes using prior knowledge, sometimes identifying the disturbance and compensating for it, sometimes filtering undesired signals.
May be that different backgrounds lead us to put emphasis on various aspects of the same phenomenon. You claim that I control the input, because my output was disturbed by some bias (mainly constant disturbance), and I claim that I do not control the input just like that, because in most cases it is mixed with noise and must be processed before I can be confident (always only to some extent) that it represents the designed controlled variable.
About open-loop control, the manipulator example above is also relevant. About open-loop in organisms, I don't have other example than myself, and it is only a one-sample-statistics. I know that before I know my car or my trajectory, I use a very stiff closed-loop system. If I get off the high-way, my control loop takes me back immediately, and I may reach the other side or crash into the cars moving in the opposite direction. Experience, or the teacher, teaches me to ignore this signal, keep the same direction, and come back slowly. I see here a combination of closed-loop and open-loop control, but this is only one opinion.

I will make a scheme of a control system, as I see it, but it will not be much different from yours. The manipulator scheme above may be relevant. I would add a direct block from the input to the effector (passing an open-loop controller) and some filter on the measurements.

Gary Cziko:
I surprized Petar Kokotovic, but he guessed immediately what my source was. About your last example, I would translate your message to Rick Marken and Bill Powers as: "If I did not KNOW it was closed-loop, I would swear it was open loop." Am I wrong?

With best regards, Izhak.

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Date: Wed, 19 Dec 90 18:21:47 CST
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Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: Bill Powers <FREE0536@UIUCVMD.BITNET>
Subject: Gary "quote"

Gary Cziko (901217) --

The problem with being general enough to satisfy behaviorists is that the result is ambiguous enough to let them say, "We already know that -- so what's new about control theory?" They've been doing that all along. Any statement that's going to be useful must be specific about the underlying assumptions that differ from those that behaviorists make. Here are some suggestions about how to make the statement more specific, if less diplomatic:

"We have inherited certain needs."

Add: These needs are not for external things but for internal states.

"We find ourselves in an environment in which these needs can usually be met, but we must figure out how to get them."

Substitute: We learn to perceive the external world and to control selected aspects of it. The aspects we learn to perceive and control are those that alter our inner states to accomplish and maintain satisfaction of our basic needs.

"Attempts which are successful in meeting these needs are somehow remembered, while those unsuccessful are eliminated. The resulting accumulation of knowledge is based on initially blind attempts to satisfy our needs. When our needs are satisfied, there is less of this blind attempt to learn a new way to satisfy a need (the E. coli doesn't need to tumble)."

Substitute: As long as basic needs are not satisfied, we reorganize. Random reorganization creates new perceptions, finds modes of action which take advantage of physical and social properties of the world to control those perceptions, and selects specific states of those perceptions to serve as secondary goals. Reorganization does not create specific acts as responses to specific events. Instead it creates negative feedback subsystems that respond to the difference between actual and desired perceptions. This relationship is such that the action automatically varies to counteract changes in the world that tend to create a discrepancy. Subsystems that do not satisfy basic needs are reorganized again and again until they do. We learn processes, not behaviors.

"All of our behavior (and knowledge) is the result of the interaction between our biological inheritance (which determine our basic needs) and our own particular experiences. Since we all share the same basic human needs, it is the latter (experience as stimuli) which is primarily responsible for the differences between individuals and between cultures."

Substitute: We do not learn how to act but how to create desired outcomes. All of our behavior and all of our knowledge arise out of a process that builds ever more complex perceptions of outcomes and ever more complex means of controlling them. The result is that our actions adapt to the physical and social properties of the environment, while their effects in the world are brought into alignment with our basic needs. Because each of us finds an individual solution to controlling perceptions and thus indirectly satisfying needs, we differ from each other in our actions and our acquired skills. Because the basic needs are genetically specified and similar in all of us, we all tend to seek similar internal states of being.

* * * * *

I think it's a fine idea to come up with a general statement of the control-theoretic view of human nature. But in making it general and avoiding jargon, I think we must also be sure that it communicates the basic structural differences between control theory and S-O-R behaviorism.

Best -- Bill

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Date: Wed, 19 Dec 90 20:19:26 EST
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: BARKANA@DUPR.OCS.DREXEL.EDU
Subject: Rick Marken

Rick Marken:
I think the argument becomes les and less important, and I don't want to get caught between behaviorists and control-theorists, because I may not understand either too well.
The content is important, however, and reading again your and my letter, I think that we say very similar things. You are right that the evidence on prediction does not prove anything about the existence of open-loop control. Actually, from an input-output point of view, open-loop and closed-loop schemes are equivalent. They have different properties wrt sensitivity to uncertainties, stability (including oscillatory or non-oscillatory responses), etc. But the argument is not which concept is better, only if open-loop control is there. In my case, of the manipulator, I know that the open-loop controller is there, because I put it there, and I see that the closed-loop "disappears" when open-loop is sufficient, because the gains vanish. I have not intention to claim that an open-loop system can deal with drift, disturbances, uncertainties, etc. I only claim that forms of open-loop control may exist along with closed-loop. The relative gains, or weightings, may vary, and in some situations, each one can be zero. If you have evidence that in biological systems there is no open-loop control, then I cannot argue, because I simply don't know. But one can explain some behavior either way, again, because from the reference-controlled variable point of view, they are equivalent.
Oherwise, I can add to your argument: a good closed-loop control system does use prediction IN THE CLOSED-LOOP, if it is needed. Any phase lead in the forward (I am afraid to call it feedforward) path, or velocity (tachometer) feedback ALONG with position feedback supply information on the future development, in other words, prediction. And this is only an elementary

example of prediction.

With best regards, Izhak.

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Date: Wed, 19 Dec 90 19:29:00 CST
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: TJ0WAH1@NIU.BITNET
Subject: Gary, Greg, Bill

Gary Cziko:

RE: The challenge of behaviorism as expressed in your "quote" of 901217 (repeated 901218), which reads in part, " Attempts which are successful in meeting these needs are somehow remembered, while those unsuccessful are eliminated....if there is a way even at this level to make an argument for CT, I'd like to be able to do it.

I believe there is a way. The sentence quoted above encompasses the crux of the matter. Consider the meaning of "attempts." What sort of behavior constitutes an attempt? Ask a behavioristic psychologist to give an example of an "attempt," and he or she will surely describe the sensible consequences of some unspecified muscular activity (e.g., "press the bar"). Such an example from a behaviorist is at once empirically sound and rationally illegitimate (a contradiction), because behavior, according to behaviorism, comprises only elicited and emitted outputs. The behaviorist does not recognize the contradiction probably because he or she supposes that the sensory consequences of muscular activates are the EFFECTS of muscular activity. (If the sensory consequences of muscular activity were the effects of the muscular activity (alone) it would, of course, be legitimate to identify a particular muscular activity in terms of its consequences.) However, except for the movements of the eyes--which are not disturbed by variable loads, this is virtually never the case, as Rick Marken (901218) has already amply noted. The sensory consequences of muscular activity always depend upon environmental circumstances, and environmental circumstances vary endlessly. This environmental variety, which implies that we are control mechanism, is part and parcel of the environmental variety largely "responsible for the differences between individuals and between cultures" (Cziko, 901218).

Ask your friend how an Alice would manage to get around in the world behind the looking glass--where one of the spatial dimensions is reversed. Does she immediately moon walk? When we are approaching a visible goal are we controlling our muscular contractions or the sensory consequences of our muscular contractions, via a negative feedback loop. If, and only if, the latter is true, a newborn Alice should evince the runaway behavior characteristic of positive feedback, rather than moonwalk. When I tested 4-day old chickens in such an environment they exhibited such runaway behavior: Hershberger, W. A. (1986). An approach through the looking glass. Animal Learning & Behavior, 14, 443-451.

Your idea of an observer controlling an illusory configuration is interesting, and I think worth exploring further.

Greg Williams:
I support your publishing an edited version of select network postings, hard or soft version.

Bill Powers:
I don't understand why your dynamic displays are not generating motion aftereffects. It is incredible. Maybe you are on to something.
Regarding the significance of illusions. A REALIST might regard illusions as embarrassing phenomena needing to be explained away. But, not being a REALIST, I am, at best, amused by such a view. It is obvious to me that replicable illusions are particularly significant phenomena precisely because they depend upon the nature of the observation. That is they provide information ABOUT the observational process, as opposed to information about the thing observed. Real (replicable) illusions provide a unique window to the perceptual processes. Notice that the word real (defined in terms of phenomenal coherence) is just as applicable to illusions as it is to electrons or electron holes (901219). However, it is difficult for me to imagine how reifying illusions, the way you are wont to reify electron holes, would prove helpful.
I found Gary's idea of an observer's controlling an illusory configuration intriguing--worth exploring further. Even if it proves to be a dead end, it might help get you and Rick out of the "rut" (901219).

Warm regards, Wayne

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Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: Bill Powers <FREE0536@UIUCVMD.BITNET>
Subject: Open loop, closed loop, control of perception

Izhak Bar-Kana (901219) --

I believe that one of the important steps one must take in understanding human control systems is to recognize the role of perception. If we were building an artificial device, then as its designers we would be aware of the true state of the controlled variable, and the state of the signal produced by the system's sensors as it represents that variable. We would be able to see what is signal and what is noise; we would know whether the signal was properly calibrated and linearly related to the external variable.

But in the human organism there is no third party who knows all this. I think that as a designer of systems, you can appreciate how the world must look to the system itself. It exists only in the form of the sensor signals, and whatever other signals are derived from sensor signals by computation. How can we discover the world as it is represented by the outputs of human sensors and sensory computers? The answer is so simple that nobody seems to have thought of it: just look around. Feel, taste, hear, and see. There it is. You are already experiencing the sensors'

output signals and you do not experience the world that gives rise to them. I do not think that this insight would come easily to anyone who has not worked with artificial sensors. But most people, in the end, understand it if they persist.

If you look at your experiences as signals in this way, you will realize that there is very little noise in them -- they are almost perfectly noise-free. Only in unusual circumstances -- near-perfect silence, the threshold of darkness -- do we experience our perceptions as behaving in a way that seems at all "noisy." Also you will realize that linearity and calibration mean next to nothing, because you are looking at the output, not the input, of the perceptual functions. You have nothing to calibrate them against but the outputs of other perceptual functions. You even pick reference signals from previously experienced sensory signals, so the scale of reference settings contains the same nonlinearities. You can say that THIS perception is nonlinear with respect to THAT perception or with respect to a meter reading (another perception), but you cannot say whether all perceptions are nonlinear in a different way with respect to their causes.

I think that this orientation makes a great difference in the way we build models of human behavior. We must realize that however the brain manages to bring external variables under control, it must manage this completely on the basis of information available to it through its senses -- its uncalibrated senses. It cannot look at the plant (the universe outside) and see what compensations are necessary in order to represent its variables properly. It knows only the variables, and even then only after they have already been represented as internal signals. The only way it can identify properties of the plant is through experience with sensory representations of the plant's behavioral variables in relation to sensory representations of the organism's own output efforts. It knows something of the inputs to the plant, and something of the outputs from it, but it knows nothing directly about the plant. The world outside is a black box. We who have seen artificial control systems both from the outside and from the inside have some advantage in understanding this situation, because we can appreciate what is lost when you lose that disembodied vantage point from which you can see what is happening on both sides of the sensory barrier and on both sides of the output boundary.

This means that when we try to guess how organisms learn to do things like adjusting their internal part of the loop gain appropriately, as in your Simple Adaptive Control, we must try to see how they can do it on the basis of information available INSIDE the controlling system. In your case you have accomplished that: just use the information in the error signal, which is inside the system. If the average error signal gets too large (which takes in many possible problems such as oscillations), reduce the gain of the output function, the effector part of the system. I don't know how your identifier works -- does it, too, work only on the basis of sensory signals available to the system as a whole? Or does it need external intelligence to tell it what to identify?

When we build models of human control systems, we naturally have to play the part of the "external intelligence" just to set up a plausible system. But our goal must be to learn how the system itself can come to acquire those design features that we find necessary, without knowing what we know about our own created system designs. I have felt for a long time that the people trying to reproduce human "pattern recognition" have been on the wrong track, because a "teacher" is an essential part of their approach. Some external intelligence must tell the recognizer if it is right or wrong. Real organisms do not have such a teacher, not when it comes to learning the basic perceptual and control processes themselves. Recent work on neural networks and perceptrons is, I think, a little closer to the right approach, because the system in part creates its own organization. But there is still a teacher who know the right answer. Real organisms never know if their answers are right, except in terms of how well they

serve to control what happens to the organisms.

My reason for total rejection of open-loop control is based on thoughts like these. How can the organism find the feed-forward output signal that will create "almost the right behavior" of the plant, without monitoring the behavior of the plant? You have to imagine setting the direct output signal to just the right value that will keep the car centered on the road without EVER seeing the relationship of the car to the road. It is impossible without the aid of some third party who knows what the plant is really doing. You may, of course, have sampled control, so that corrections are applied only now and then. But that is still control and it cannot work without feedback. All of what is commonly called "feedforward" is really explainable only in terms of a hierarchy of feedback control systems (in many cases, including those in an engineer silently standing by in the background, screwdriver in hand). Only through feedback can the so-called feedforward be properly adjusted. There is no friendly omniscient engineer in the background adjusting our own "feedforwards" for us.

Your example of overcontrolling a car is, I think, only a description of how we learn the right dynamics of response and get the control system stabilized. In the end we have very fine high-gain control with proper temporal filtering so that oscillations are eliminated. But we do not noticed this control because it takes place in our midbrains, brainstems, cerebellums, and spinal cords, where we seldom pay much conscious attention to what is going on. It doesn't seem that we are exerting much control effort when we drive down a straight and level road.

But just watch the steering wheel! It moves with every little bump in the road, every slight change in the crosswind, every little tilt of the roadbed. This control system is extremely sensitive to error -- but it keeps the error very small, so it does not have to make big efforts. Unless, of course, there's a big disturbance. We habitually observe from a higher level of abstraction, and we don't notice the errors or the corrections because they are happening at lower levels. The car just seems to go straight by itself. But just try holding the wheel absolutely still, and you will see that significant disturbances are always present. Their effects are cumulative. If their effects aren't precisely corrected the car will quickly go off the road.

Best -- Bill

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Date: Thu, 20 Dec 90 12:06:15 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: Joel Bennett Judd <jbjg7967@UXA.CSO.UIUC.EDU>
Subject: Season's Greetings

I wish I could get some graphics on here, but imagination will have to suffice. I was thinking about ways of commercializing CT, and I thought greeting cards might be a way to start. So here's a marketing proposal for CSG corporate headquarters:

On the front of the card there's Santa's sleigh all loaded down with toys and hitched up to the reindeer. But one of the runners is broken in half, and the sleigh is sitting half sunk in the snow. Beside it is a very dejected looking Santa Claus.

When you open it up, the inside says:

WISHING YOU AND YOURS

A DISTURBANCE-FREE CHRISTMAS

What do you think?

MERRY CHRISTMAS AND HAPPY NEW YEAR TO ALL - Joel Judd
==========
Date: Thu, 20 Dec 90 12:06:21 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: Joel Bennett Judd <jbjg7967@UXA.CSO.UIUC.EDU>
Subject: Accommodation & Assimilation

Hugh Petrie and Bill Powers,

I sent the following to Gary Cziko for a response; I'd be interested in how both of you (and anyone else) current think of the following (this follows after getting through "Dilemma..." once and rereading parts of "Behavior...".

It seems as though Hugh Petrie wants to limit reorganization to those cases requiring "radical change" of conceptual scheme(s) [accomodation], but not within scheme(s) [assimilation], while Powers uses it for any change in the "forms of the functions" of the Control hierarchy (Petrie also implies that CT really
models assimilation, while variation and retention models accomodation).Are they agreeing on this, or is there really a discrepancy, as I perceive it?
>
>Yes, I see this as a problem as well and it is a good question. My own
>bias is that there is no such thing as 100% assimilation, there is always
>some accommodation involved, but sometimes there is a lot and sometimes
>just a little. I think Powers might agree with this, I'm not sure if
>Petrie would.
>
>This would be a good question to send to Powers and Petrie via CSG-L.--Gary
>
Joel Judd
>
==========
Date: Thu, 20 Dec 90 10:22:21 -0800
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: marken@AEROSPACE.AERO.ORG
Subject: Open/Closed Loops

These are some comments on the thread related to Open-closed loop control. The relevent articles are from Izack Bar Kana (901217 and 901219) and Bill Powers (901220). In his excellent post (901220) Bill Powers has looked at the open-closed loop issue "from the inside". Let me try it again "from the outside" -- looking at control as the observer of a controlling system, rather than as an example of one of these systems. When we look at a living system we see that it produces many consequences. These consequences are potentially variable -- the temperature at the skin, the position of a limb, etc. The value of the variable at any time depends on many factors -- the "causes" of the variable. Thus, y = f(a,b,c,o) where y is the variable of interest and a,b,c and o are the variables that "cause" y; f

is the function that determines how y varies (over time) as a function of variation in the causal variables. If a,b,c and o vary over time then y should vary over time in a manner determined by f. If, however, y remains approximately constant over time then we might imagine that something fishy is going on. Stability of y could happen by chance -- the variations in a, b, c and o could just happen to produce a constant y. But the longer this goes on, the less likely it becomes that stability is occurring by chance. Moreover, if we can trace the stability of y to variations in o, which happens to be the causal influence on y exerted by the living system, then there is strong evidence that o is systematically counteracting the effect of a,b and c on y. I take these two pieces of evidence -- the stability of y and the fact that system outputs are the sole cause of this stability -- as evidence that y is a controlled variable. This is evidence of control "from outside the living system". It says nothing about how this control is achieved. What I am claiming is that the only organization that we currently know of that can provide an explanation for control is control theory -- that is, the theory that y (the controlled variable) is part of a closed negative feedback control loop. One other part of that loop must be a reference signal that specifies the particular value at which y is stabilized. If y is stabilized at different values then this reference signal must be variable. Observation of the environment of the control system reveals no variables "out there" that could possibly function as the refence signal(although people have been fooled into thinking that "targets" in tracking work this way. The simple way to show that they don't function as references is to show that people can reliably keep y stabilized at values that differ from the target value "when they want to"). Thus, the reference signal must be inside the system itself.

The controlled variable, y, need not be a simple aspect of the system's environment. We see living systems controlling very complex variables, such as their relationships with other people. I have seen people keep a variable called "in love" at nearly the same level for periods of years. The ability to control such variables implies an ability of these systems to perceive such variables. Perception, from a control theory point of view, is not some arcane discipline of only peripheral interest to psychologists. Perception becomes central to understanding human nature. What people do depends on what they perceive and where they want those perceptions to be.

This model of what we see as the behavior of organisms is radically different than any other model of behavior that is currently embraced by most life scientists. It is a model that works, that satisfies the requirements of scientific method and that provides a comfortably humanistic view of human nature. That is the reason for my enthusiasm, Itzak. I guess I "believe" in control theory; but not in the usual sense of belief. I am not reverential towards it. If it proves to be wrong I will happily abandon it for the improved point of view. You should go through the earlier posts about modeling and the importance of models to understanding my "belief" system. I believe, based on experimental evidence and matching the behavior of models to that of living systems, that control theory currently gives us the best (and only) model of how people (and other living systems) work. I think this is not only scientifically important but socially important. I think a case can be made for the notion that people have been screwing around with each other and making life more difficult for each other because thay have been looking at each other as a particular kind of object--one that can be controlled from the outside. The control model shows that this is not only false but also a sure recipe for conflict. And I think most people would agree that conflict between people (classes, religions, nationalities, whatever) has been the continuing obstacle to the possibilitiy of every individual (beside the current winners of each conflict) leading a graceful, dignified and
satisfying existence.

I probably will not be able to read or post stuff until january 3, 1991 so

happy new year (and new decade) to all.

Rick M.

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Internet:marken@aerospace.aero.org
213 336-6214 (day)
213 474-0313 (evening)

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Date: Thu, 20 Dec 90 14:42:00 EST
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Comments: Warning -- RSCS tag indicates an origin of SMTPUSER@UBVMSC
From: Hugh Petrie <PROHUGH@UBVMS.BITNET>
Subject: Re: Accommodation & Assimilation

Joel Judd (901220)--
I think in Dilemma I would have been more adamant that there was little if any, reorganization in the ordinary operations of established control systems. I reserved reorganization for more radical changes. I am sure that part of the reason was that I was also heavily influenced at that time by the fairly recently emerging conceptual change literature. Change of concepts truly required revolutionarily new ways of looking at things. At this time I probably agree with Gary that there may always be some reorganization going on during learning, sometimes more, sometimes less. I think that the educationally interesting questions then become "What is being reorganized? Reference signals, i.e., better ideas of what we want? Perceptual functions?, i.e., better ability to see the world in certain ways. Better comparators? and so on."

It might be of some interest to take well-known educational phenomena and ask ourselves what could be going on from a CSG perspective. This is one reason for my continuing interest in metaphors. We know that in varying fields students simply come naively to certain subjects with "wrong" conceptions, but ones which do pretty well for the gross controlling tasks in which they are engaged. One recent one I heard has to do with students naively believing that the reason you put on a sweater when it is cold is that wool is just always "warmer" than other materials. They have no idea about heat loss, insulation, etc. The question then becomes, "How do we understand the students' naive beliefs? How do we introduce disturbances to get them to acquire more adequate beliefs?--all from a control system perspective. "Hands on" science, I think, implicitly recognizes that one must provide experiences which will "mean" something to the students (fit into existing control systems, but cause enough disturbance to require reorganization?)

In short, I have no problem with saying that all learning requires more or less reorganization. My concern would be to take the next step and try to identify different kinds of learning requiring different kinds of reorganization of different parts of the control system(s) or the hierarchical arrangements among them.

Happy Holidays--Hugh Petrie

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Date: Thu, 20 Dec 90 14:23:59 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: g-cziko@UIUC.EDU
Subject: Balloon Thought Experiment

It's not control theory, but it's a good "light" question to get us in the right mood for the holidays. There must be some real physicists out there:

You board a jet aircraft with a helium filled balloon on a string. You tie the string to your armrest so that it floats freely above. As the jet accelerates down the runway, you are pushed back into your seat. What does the balloon do?

Predictions and reasoning appreciated.--Gary

Gary A. Cziko Telephone:
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Champaign, Illinois 61820-6990
USA

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Date: Thu, 20 Dec 90 15:23:56 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: "Norman H. Packard" <n@COMPLEX.CCSR.UIUC.EDU>
Subject: positive and negative feedback in large networks

I am Norman Packard, and I just recently added myself to the CSG mailing list, though I may not last because of the heavy traffic. My background is in physics, particularly study of chaos and other complex dynamics. I also work with evolutionary models, an autocatalytic soup model for the origin of life, a network model for the immune system, a model with organismic-level evolution of information processing, and most recently a "Turing gas" model with a population of interacting Lisp programs.

My "non-passive" interaction with CSG started with a dialog with Bill Powers about feedback loops in large networks. He suggested we post it; it is attached.

Norman
n@complex.ccsr.uiuc.edu

From n Mon Dec 10 15:10:13 1990
To: FREE0536@uiucvmd.bitnet g-cziko@uiuc.edu
Subject: Positive feedback
Status: R

Gary,

Thanks for your latest Powers blurb. I think forms of positive feedback do exist in biology, however... the main place I've come across something one might want to call positive feedback is in the immune system. There, the network picture of the immune system has a given antibody coupled to many others (and antigens, potentially), and the coupling includes a lot of both positive and negative terms, typically.

N

From FREE0536@uiucvmd.bitnet
g-cziko@uiuc.edu) Tue Dec 11 16:57:29 1990
Date: Tue, 11 Dec 90 13:55:17 -0600
X-Ph: V3.2@ux1.cso.uiuc.edu
To: (PACKARD_Norman)n-packard@uiuc.edu
From: Bill Powers <FREE0536@uiucvmd.bitnet> (by way of (Gary A. Cziko)
g-cziko@uiuc.edu)
Subject: Positive Feedback

Norman Packard (via Gary Cziko):

Positive and negative CONNECTIONS don't imply positive or negative FEEDBACK. To find the sense of feedback, you have to trace all the connections ALL THE WAY AROUND a closed loop. The product of all the signs is the sign of the feedback. So a negative feedback loop has to contain an odd number of negative connections.

Best -- Bill

Bill Powers 1138 Whitfield Rd. Northbrook, IL 60062 708-272-2731
(BITNET) FREE0536@UIUCVMD (INTERNET) FREE0536@VMD.CSO.UIUC.EDU

Date: Tue, 11 Dec 90 17:14:28 EST
From: n
To: FREE0536@uiucvmd.bitnet
CC: g-cziko@uiuc.edu
Subject: Positive Feedback

Bill,

Typical dynamical networks used to model biological systems (or pieces thereof), such as auto catalytic networks or immune networks, have many coupling loops of different sizes affecting the dynamics of any given element. According to your definition of product of signs around each closed loop, such systems might be seen as combinations of many positive and negative feedback loops, I suppose. I'm not sure how useful the terminology is in this context; I know of no results relating this kind of information about coupling to the global dynamics, the kind of results that are much easier to obtain for very low dimensional systems.

I understand that Gary Cziko is trying to set up a visit. I look forward to meeting you when you get here.

I think Gary may have sent you a preprint of one of my papers ("Measurement of Evolutionary Activity ..."); I will send the final version and some other papers that you might be interested in.

Cheers,
Norman

Date: Tue, 11 Dec 90 21:20:48 CST
From: Bill Powers <FREE0536@vmd.cso.uiuc.edu>
Subject: positive feedback etc.
To: n@complex.ccsr.uiuc.edu

Norman --

I'm trying to send this to you direct; hope it works.

You're right that there are many loops. They may be beyond any kind of modeling that we know how to do in the CSG. On the other hand, suppose that we could start identifying levels of control, with the outputs of one level being reference signals for lower levels. This kind of arrangement would look like just a big random collection of loops, positive and negative, if it weren't sorted out -- especially if the role of reference signals weren't recognized.

>From my standpoint this is hopeless. The problem needs someone who is thoroughly acquainted with the biochemistry involved AND who understands the principles behind the model we use in the CSG. It's not going to be easy to find such a person among biologists -- they seem to have an allergic reaction to the concept of a reference signal. They complain that this is just homeostasis, or else they shy off from the idea that the system has some magical knowledge of the state that's good for it. Neither complaint is relevant, but I've never been able to get past that point with a biologist or biochemist. Maybe it will take a physicist to break the ice (hint).

I'm sending you a letter that was circulated to a few CSG people who might or might not want to try the biochemical angle. Don't be misled by my "expertise" -- it's all a bluff. I'm just trying to make as much sense as I can out of what I have read, hoping to interest someone who can make more sense of it.

Bill Powers 1138 Whitfield Rd. Northbrook, IL 60062 708-272-2731
(BITNET) FREE0536@UIUCVMD (INTERNET) FREE0536@VMD.CSO.UIUC.EDU

Date: Wed, 12 Dec 90 12:54:22 EST
From: n
To: FREE0536@vmd.cso.uiuc.edu
CC: g-cziko@uiuc.edu
Subject: positive feedback etc.

Bill,

Got your direct message.

From my point of view, the central problem for deciphering the dynamics of large systems of coupled subsystems is contained in the hope you exress: "... suppose that we could start identifying levels of control, with the outputs of one level being reference signals for lower levels."

The problem is, how the hell do you identify the emergence of hierarchical control? I am increasingly convinced that it is impossible to obtain the answer by looking simply at the equations of motion (counting loops, weighting counts by signs, whatever), because the global dynamical behavior is, in general, uncomputable from the equations of motion. Moreover, besides this hollow-sounding formal result, practical experience from recent network simulations (of immune networks) show that a fixed topological network configuration (couplings in the equations of motion) can yield wildly different "effective" topologies when the dynamics are actually implemented,

depending on parameter values in the equations.

This leads to the idea that the identification of levels of control is essentially an a posteriori recognition problem. The major initial question is how characterize what one is trying to recognize. Maybe the CSG picture of control offers a framework to guide the recognition process.

I look forward to the letter you mentioned.

N

Date: Wed, 12 Dec 90 17:53:47 CST
From: Bill Powers <FREE0536@vmd.cso.uiuc.edu>
Subject: Biochemical loops
To: Biochemical loops <n@complex.ccsr.uiuc.edu>

Norman --

I'm glad to see that somebody else has problems with proliferations fo closed loops, too. We might as well go public with this, as some biochemists might be listening in. If you think so you can send this on to CSGnet.

There's no guarantee that the kind of hierarchy I propose is the one that will prove useful in biochemistry. On the other hand, it seems to me that the same principles are probably repeated at many levels of organization, so that what you find in behavioral organization might well be an example of principles that are employed at lower levels, too. Anyway, what do we have to lose but a year or two down a blind alley?

While I was still developing ideas about the behavioral hierarchy, I didn't start and the bottom and work up, or at the top and work down. I just took whatever control phenomena I could find and worked in whichever direction the least resistance seemed to occur. So maybe the biochemical systems have to be approached the same way: take the most obvious systems and try to work both down and up levels from there. The immune system may be a very difficult place to start the a posteriori recognition process (I completely agree with you about that being what the real task is).

There are some hormone systems that are pretty well known, although they haven't really been studied as control systems as far as I know. The thyroxin control system, for example. The pituitary detects circulating thyroxin concentration, compares it against a reference signal in the pituitary, and generates TSH (thyroid stimulating hormone) which is pretty clearly the error signal. The TSH drives the thyroid gland to produce thyroxin. The signs are exactly those I use in my model: an increase in circulating thyroxin (the controlled input to the pituitary) DECREASES production of TSH. So the perceptual signal is negative, the reference signal is positive. I don't know what the reference signal is -- it probably is set by signals from the hypothalamus. The whole pituitary seems to consist of a large collection of comparators in control systems for various circulating substances, receiving many reference signals via the neurohypophysis from the hypothalamus.

So that gives us control of thyroxin. Now, suppose we postulate that thyroxin becomes a reference signal for some set of lower-level control systems. In other words, we ought to find that the thyroxin

stimulates the production of some other substance that feeds back to cancel the stimulating effect of the thyroxin. The feedback substance (or an enzyme whose concentration depends on it) is thus a controlled variable that is made to track changes in the thyroxin level. There could be several or many such lower-level control systems, all having their reference signals determined by the thyroxin concentration, in parallel. Presumably, these would all be systems that control more detailed aspects of metabolism.

I'm afraid that I don't know enough biochemistry to carry this any farther. And it doesn't get us much closer to the immune system. But I think that if even a couple of levels could be found ANYWHERE in the system, we could learn enough from seeing how they work to take us another step. The idea that hormones are reference signals that control the concentrations of other substances might be new to biochemistry. It might give some bright young thing just the gestalt-breaking insight that's needed. That would please me no end.

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(BITNET) FREE0536@UIUCVMD (INTERNET) FREE0536@VMD.CSO.UIUC.EDU

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Date: Thu, 20 Dec 90 19:08:58 CST
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: Bill Powers <FREE0536@UIUCVMD.BITNET>
Subject: Up, Up, and away

Gary Cziko (901220) --

A balloon rises because of the tidal difference in air pressure on opposite surfaces (minus the smaller difference due to the lighter gas inside). This difference is maximum in the direction of the effective gravity gradient. As the airplane accelerates, the direction of apparent gravity moves from straight down to some angle tilted toward the rear of the plane. The density gradient in the cabin air tilts to conform (relative to the true direction of gravity -- I am assuming that the airplane is moving horizontally on the runway). The balloon will therefore move forward relative to the true direction of gravity (general relativity theorists, mind your own business, you know what I mean) and remain there until acceleration ceases and uniform velocity is reached.

If you are standing in the aisle holding the balloon's string, you will lean forward because your intention is to keep your balance. The balloon will lean forward because it can't do anything else. Same behavior, different explanations. Just goes to show that behavior without a system model doesn't tell you much about what's going on.

A detail: At the beginning of acceleration, the balloon will lean BACKWARD. This is because the air in the cabin must flow rearward to establish the new pressure gradient and will carry the balloon with it. When the gradient comes to a new steady state, the balloon will lean forward until the acceleration is removed. At that point, the cabin air will flow forward, carry the balloon even more forward with it, and finally the balloon will return to the straight-up position when the cabin air settles to a vertical pressure gradient.

You will do the same thing at the beginning and end of acceleration but for entirely different reasons that control theorists will readily understand. See currently antepenultimate paragraph, last sentence.

Anyone willing to buy an airline ticket and a balloon?

Gary: the 8th would be OK if that would fit Petar's schedule better. Roger on the test disk.

Best to all -- Bill

Bill Powers 1138 Whitfield Rd. Northbrook, IL 60062 708-272-2731
(BITNET) FREE0536@UIUCVMD (INTERNET) FREE0536@VMD.CSO.UIUC.EDU

Date: Tue, 18 Dec 90 19:29:36 GMT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: Chung-Chih Chen <chen%arti1@VUB.VUB.AC.BE>
Subject: levels

Bill Powers:
There is a big difference between us: You are a psychologist, but I am not. You like to use other people as subjects to do your experiment. But I just use machines (hardware and software) to do my experiments. You always try to see if your theory corresponds to how you and other people perceive. But I just try to produce some intelligence without caring about its psychological plausibility. That's why it's difficult for me to talk about my perception.

Since your hierarchy has its physiological basis, I don't know why it should have anything to do with language/culture difference. It should be the same for every human being.
I can find a Chinese term for each of your levels. I have never tried to examine my perception as carefully as you have done. Your levels match my experience (or reasoning) naturally. I don't see other alternatives.

I am thinking your suggestion on the image recognition problem. Only by designing a working model consistent with the control theory can we say that the theory is right.

Chung-Chih Chen
Artificial Intelligence Laboratory
(Building K, 4th Floor)
Free University of Brussels
Pleinlaan 2
1050 Brussels, BELGIUM
(email: chen@arti.vub.ac.be)

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Date: Fri, 21 Dec 90 09:09:20 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: "Stevan Harnad by way of Gary A. Cziko g-cziko@uiuc.edu" <harnad@CLARITY.PRINCETON.EDU>
Subject: Language, Tools and Brain: BBS Call for Commentators

CSGers:

Appended below is an invitation for commentary on an article that some CSGers might be interested in. Perhaps a copy of this could be sent to the Plooijs by someone who has their address.

I am told that Brain and Behavioral Sciences has the widest circulation of all psychology journals. It might be nice to get some control theory commentary into this journal. Of course, what would really help to shake up the behavioral sciences would be to have a target article by Bill Powers in the journal. Perhaps some good CT commentary might help pave the way for such an invitation.--Gary Cziko

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Below is the abstract of a forthcoming target article to appear in Behavioral and Brain Sciences (BBS), an international, interdisciplinary journal that provides Open Peer Commentary on important and

controversial current research in the biobehavioral and cognitive sciences. Commentators must be current BBS Associates or nominated by a current BBS Associate. To be considered as a commentator on this article, to suggest other appropriate commentators, or for information about how to become a BBS Associate, please send email to:

harnad@clarity.princeton.edu or harnad@pucc.bitnet or write to:
BBS, 20 Nassau Street, #240, Princeton NJ 08542 [tel: 609-921-7771]

To help us put together a balanced list of commentators, please give some indication of the aspects of the topic on which you would bring your areas of expertise to bear if you are selected as a commentator.

Language, Tools, and Brain: The development and evolution of hierarchically organized sequential behavior

Patricia Marks Greenfield
Department of Psychology
University of California, UCLA
Los Angeles, CA 90024-1563
electronic mail: rygreen@uclasscf.bitnet

Abstract: During the first two years of life a common neural substrate (roughly, Broca's area) underlies the hierarchically organized combination of elements in the development of both speech and manual action, including tool use. The neural evidence implicates relatively specific cortical circuitry underlying a grammatical "module." Behavioral and neurodevelopmental data suggest that the modular capacities for language and manipulation are not present at birth but come into being gradually during the third and fourth years of life. An evolutionary homologue of the common neural substrate for language production and manual action during the first two years of human life is hypothesized to have provided a foundation for the evolution of language before the divergence of hominids and the great apes. Support comes from the discovery of a Broca's area analogue in contemporary primates. In addition, chimpanzees have an identical constraint on hierarchical complexity in both tool use and symbol combination. Their performance matches that of the two-year-old child who has not yet developed the differentiated neural circuits for the relatively modularized production of complex grammar and complex manual construction activity.

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Date: Fri, 21 Dec 90 08:42:38 -0800
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: marken@AEROSPACE.AERO.ORG
Subject: B&BS

Gary:
(901221)
Thanks for the info about the target article in Brain & Behavioral Sciences. CSGers have delt with B&BS before. Bill has published at least two, probably four responses to target articles. I believe Tom Bourbon has also put in a couple and I published a reply to a target article on operant conditioning about a year or two ago. Tom Bourbon arranged for Bill to submit a target article several years ago. Bill Powers can tell you the story of that dismal interaction if he wants. I submitted a paper for a target article myself --it was of course rejected. B&BS is interesting because it does have a large readership and, for reasons I can't understand based on what I've read in there, something of a good reputation. I think it is really just one of the flagships of "trendy science" -- and lots of people like to go with the current trend, hence the large readership. The target article/commentary format is nice, though. I think that might be a nice way to organize some of

the CSGnet postings -- especially if they are to eventually appear in print.

Anyway, this will indeed be my last day to post and receive mail. My home modem didn't work for reasons that I am not sure of still -- I think it turns on the fact that my Mac is soooo old. The modem connector at home is nothing like the connector sent with the modem. But that just means I will have to concentrate on doing some work while I have the chance.

I will mention that I am still working on the hierarchy stuff. I am looking at the perceptual aspect of the hierarchy and using my "rate of presentation" approach to see at what rate different types of perceptions emerge. I discovered an extremely interesting reference on a type of relationship perception -- the perception of "causality". I'm sending for the book (by Michotte, 1963) but the gist of the work is this; certain temporal changes in events produce the perception that one event is the cause of the other. I have written a very simple program using four dots arranged as so: x xx x
call them 1 23 4
Dots 1 and 3 come on simultaneously first. After a variable delay (d) dot 1 goes off and dot 2 comes on. After another delay 3 goes off and 4 comes on. When d is very short all you see is a transition; but as d is lengthened you begin to see the transition of 1 to 2 as the cause of the transition from 3 to 4. I am trying to invent a way such that, by gradually adjusting d (from 0 on up) using the mouse, each level of perception gradually becomes visible. I think I can go from configuration to transition to relationship (in the above
example the subject can infer the sequence from the relationship -- but I think
I don't really see a sequence until the dots are moving more slowly than they are when I see causality-- implying that sequence is higher order than relationship).

Anyway, any suggestions on this (before I leave for vacation) would be most appreciated.

Hasta Huego
Merry Christmas

Rick M.

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Richard S. Marken USMail: 10459 Holman Ave
The Aerospace Corporation Los Angeles, CA 90024
Internet:marken@aerospace.aero.org
213 336-6214 (day)
213 474-0313 (evening)

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Date: Fri, 21 Dec 90 13:23:33 GMT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: Chung-Chih Chen <chen%arti1@VUB.VUB.AC.BE>
Subject: Bill

Bill Powers:

I think I have got all emails I should receive. They are just delayed. I have the email problem nearly once every week.

Maybe the prize-fighter won't be knocked out. He may get the prize !!!

Chen

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Date: Sat, 22 Dec 90 00:46:02 CDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Comments: Please Acknowledge Reception,Delivered Rcpt Requested
From: RLPSYU08 <TBOURBON@SFAUSTIN.BITNET>
Subject: B&BS

Gary Cziko: re your 901221 posting about B&BS, the history of CSGers and B&BS is even more disappointing than described by Rick Marken (901221) in his post, in reply. He, Bill Powers and had several commentaries published there, as far back as about 1978 or 1979. Hence, we three are "associates of the BBS project," whatever that is supposed to mean. I thought it meant we could suggest topics and authors that might make real contributions to the literature.

With that naive assumption, I urged Harnad to solicit a manuscript from Bill Powers. Bill can give you the details, if he wants to dredge up all of the accompanying emotions. The bottom line was that, after all of the effort by Bill and after reviews by some of us favorable to CST -- and by others, obviously more "important," who were opposed --Harnad concluded that CST is, "just another way to look at things." As though much of the high-church scholasticism in B&BS were anything other than "just a bunch of ways to look at things!"

So Bill tried again, in collaboration with Michael Hyland (one of the silent members of GSG-L -- perhaps he will want to comment on the B&BS days). After two tries, their manuscript ended up in the, "CST is just another way of ...," you know the rest.

So Rick tried. You guessed it!

During that period, we had other commentaries published, sometimes by invitation in the first round, more often as "continuing commentary" we submitted after the fact. But B&BS goes its way unaware of the issues raised by CST. Article after article, and the many hundreds of commentaries, have appeared on topics to which CST is clearly relevant, but there are no invitations to comment. In the beginning, I was an ardent supporter of the concept of B&BS, but I have grown tired of seeing so much creativity expended in endless rounds of viewing a topic from "this perspective," then from "that one," then from "yet another orientation," knowing all the while that CST is dismissed as ... . Often the entire assemblage of authors, for the target article and all of the commentaries, will dance around a topic, never realizing that all of their putatively alternate explanations are one, becuse all invoke the same linear model of causality, dressed up in different jargon.

Come to think of it, Harnad was right -- every time. CST IS a different way of looking at things.

And now that I think back over the B&BS cycle, I am amazed that Bill Powers still counts me among his associates! I wonder if I could forgive someone who did me a favor as big as the one I did him. Thanks, Bill.

Happy holidays to all.

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Date: Sat, 22 Dec 90 07:27:44 CST
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: Bill Powers <FREE0536@UIUCVMD.BITNET>
Subject: Intelligence, perceptrons

Chung-Chih Chen (901220)

It's encouraging that you find my terminology to have meaning in another language so unrelated to English.

I'm not a psychologist either, Chen. I have an undergraduate degree in physics (1950) and a lot of practical experience in designing electronic systems and in computer programming. I don't have any advanced degrees; one year of psychology graduate school (1960-61) was all I could stand. Whatever more I know about psychology I have had to learn on my own and with the help of friends in that field. All my initial work on control theory and human behavior was done while I was a medical physicist in the 1950s. What I learned in graduate school was that psychologists use a lot of terms without any clear definitions.

There's a phenomenon that someone has called the "down-the-hall effect." Scientists in various specialties often borrow terms from other specialties, but when asked to be more specific about the borrowed terms, they say "Oh, that's not my field, but there's a fellow down the hall who can tell you all about it." So we end up chasing a lot of words down the hall, but in fact nobody understands them. They're just words that sound as if there ought to be an idea behind them. Everyone assumes that there's somebody else who understands them.

"Intelligence" is one of those words that everybody uses and nobody really understands. One of the reasons I got out of psychology is that it relies very heavily on words that are never defined -- you're just supposed to know what they mean. Whatever meanings we attach to them are the ones we learned as children, by reading or just from context -- trying to imagine meanings that make sense of the words that other people use. This makes a lot of psychological ideas sound rather childish but even worse, there's no way to tell whether two psychologists who agree with each other are actually agreeing about the same thing. All you know is that they're using the same words. If you say that a certain way of behaving is intelligent, and I agree with you, we still don't know if you and I are seeing the same characteristic of the behavior. It's even worse when a psychologist and a physicist talk to each other using terms like "work" or "energy" or "field" or "integration."

Even if you're not a psychologist, saying that you want to make an "intelligent" machine forces you to be one, because there aren't any psychologists who can give you a formal meaning of intelligence. The closest they can come is to say that intelligence is a score on a certain kind of test, adjusted for the population mean. Off the record, of course, they will talk about capacities for doing this or that. But what those capacities are, such that they enable us to do this or that, remains unknown. Defining those capacities is exactly what you want to do, isn't it?

My private conclusion is that "intelligence" is a word that points vaguely in the general direction of something, but isn't worth using as a technical description of human characteristics. It's much better just to explore the characteristics, and name them after you are sure they exist. Maybe, when you finally figure out how we construct configurations in our perceptions, you will discover that the method is pretty simple-minded and not intelligent at all. In fact, it's probably pretty stupid, because we can see configurations that aren't even physically or biologically meaningful, like "horse-and-rider."

My levels do bear on what you're trying to do. If the nervous system began as a completely unformed random network, I can't see how it would tend to form levels at all, much less hierarchical levels of perception. I think that there must be some predisposition in specific anatomical levels to

carry out computations of specific kinds -- different kinds at each level. The perceptron approach, as I understand it, assumes that the basic predisposition is to compute weighted sums, with the weights being adjustable. In my model, that would seem more appropriate to the sensation level, not the configuration level. It could be that what SEEMS to be configuration-recognition in perceptrons is really just the ability to respond to specific sets of sensation-signals. Perhaps some other kind of computation is needed before sets of sensations-signals generated by one level of perceptron could be combined through a new kind of operation in another perceptron layer to yield a new KIND of invariance. You can still have random reorganization at each level providing the ability to learn new perceptions -- but the basic structure of the level determines what reorganization can accomplish. Just setting up a perceptron in its initial state implies some pretty strong assumptions about the kind of operation that will be the basis for computing all perceptual outputs.

It might help to consider other sensory modalities. In sound-perception, we can hear a major triad as remaining the same even though all the tones change in proportion. So maybe at the configuration level we should have ratio-computations, not just weighted summation. This might be accomplished by changing to logarithmic variables -- then weighted sums would take on the significance of products and ratios.

It may be that this pre-organization should include the components and general arrangements necessary for construction of control systems, meaning that outputs from higher levels become reference inputs for lower levels. This sort of feedback connection is already used by some neural network modelers, but they don't see the down going signals as reference signals. They call them "backward propagation," don't they? Maybe there's more to the idea of backward propagation than they have yet seen. I just don't know enough about this subject to offer more than a vague idea here.

I have a personal interest in this sort of work. If you could show that there is a type of computation that could support the extraction of configuration-like invariants from signals that represent sensation-like invariants, that would lend credibility to my definitions of these lower levels of perception.

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Date: Sat, 22 Dec 90 15:31:39 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: g-cziko@UIUC.EDU
Subject: Perceptrons

Bill Powers (901222) & Chen (901220):

Your discussion about perceptrons and machine pattern recognition remind me of the work done by Gerard Edelman.

Edelman won a Nobel prize in medicine for his work on immunology and now directs the Rockefeller neuro-something institute in New York. Recent books include _Topobiology_, _Neural Darwinism (1987), and _The Remembered Present: A Biological Theory of Consciousness (1990).

In _Neural Darwinism_, Edelman discuss a program (I believed he called it Darwin II) which learns to recognize patterns on its own without instruction by using a form of cross-modal correlation and a Darwinian variation and selection of synaptic connections. I never understood enough of the details to pass

judgment on it, but the overall approach did make sense to me (although I'm probably biased being a fanatical fan of Darwin's hammer). This cross-modal correlation technique coupled with the total abstinence of using providential information from outside the system (see Powers 901220a) plus a Darwinian random variation of connections with subsequent selection appears to me consistent with much of what Bill has been saying.

Of course, I realize that you may already know of Edelman's approach and have dismissed it for various reasons. But you may want to take a look at it if you are unfamiliar with it.--Gary

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Date: Sun, 23 Dec 90 17:24:45 CST
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: Bill Powers <FREE0536@UIUCVMD.BITNET>
Subject: Notterman and Mintz

Dennis Delprato (901217) --

I got hold of Notterman and Mintz (dynamics of response). Unfortunately, your quote was from the last page, while I started reading it from the first page. On Page 1 we find:

> Defining a "response," like many other fundamental matters, touches upon basic assumptions that may differ from scientist to scientist. We accept the deterministic view that a response is an identifiable change in behavior, one that is always the consequence of a prior event called the "stimulus." If a response is observed, a stimulus must have preceded it, whether or not the scientist can identify this initiating event."

You didn't warn me that this was going to be a religious tract.

The authors say (p. 2):

> Consider the examination of bar-pressing behavior: by means of appropriate recording equipment, we determine that a response has been made; this information is obtained with no apparent lack of decisiveness or precision. But when has the condition of the organism passed from that of "no change" to that of "change" and then back again to "no change?" Obviously, we are dependent on the elastic constant of a spring within a microswitch to resolve whatever doubt may exist. Whenever the spring has been compressed sufficiently for a pair of contacts to pass from open to closed and subsequently from closed to open, the cycle of no change to change and back again has been completed, and a response emitted.
>
> By changing the tensile quality of the spring, we may increase or decrease the level of force that, by these mechanical operations, comes to define a "response."
>
> (Skip to p. 4): The "operant", as we have seen, is defined in terms of the effect of motor behavior upon some specific object in the environment. ...But as just noted, the state of rest of an object cannot be changed unless some force has been exerted. Hence, it is possible to redefine operant behavior as behavior that is tantamount to the organismic emission of forces. (emphasic omitted)

This is tantamount, as well, to postulating that the effect on the external object corresponds to the emitted force, although we have just seen, two pages previously, that the spring in the microswitch assures that this cannot be true. N & M started out bravely enough trying to develop a physical analysis of behavior that recognizes the intervention of environmental properties between muscle forces and their effects. It's fascinating to see how their argument then veers away from the conclusion they are approaching, namely, that forces emitted by the organism cannot

simply be equated with their external effects. Such a conclusion would be unthinkable within the tenets of behaviorism: therefore, they avoided thinking it. If they had followed their logic to the conclusion that is inevitable instead of to the one they wanted, the last sentence cited above would have read

> Hence, it is IMPOSSIBLE to redefine operant behavior as behavior that is tantamount to the organismic emission of forces.

In the remainder of the book, the authors try experiment after experiment (there must have been thousands of runs) to show that emitted force can be conditioned. If it can, it is apparently not a very big effect. Only through statistical analysis, in most cases, can they show that "significance" has been reached. There is, of course, no model. And although the experimental setup would have been suitable for measuring force-control by rats, no data were taken that would enable us to salvage anything of that nature from all this huge amount of wasted effort.

So, Dennis, the quotation from the back of the book sounded promising indeed, but there was nothing in the preceding 263 pages to make this book relevant to control theory. I probably did see this book when it came out. But I doubt that I got very far beyond page 1.

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Date: Sun, 23 Dec 90 12:28:31 GMT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: Chung-Chih Chen <chen%arti1@VUB.VUB.AC.BE>
Subject: neural Darwinism, back propagation

Gary Cziko:
I find that there may be confusion in quoting other people's posts by using the dates you receive. Because you receive a post today, maybe I receive it tomorrow or even later. So I find it difficult to follow the dates quoted by other people.

About Neural Darwinism, I think it's plausible. I always believe that macrostructure reflects microstructure. If Darwinism is the mechanism of forming macrostructure in the universe, it's possible that Darwinism works also in our brain.

Bill Powers:
I knew that you were a physics major and you learned psychology and worked at Northwestern etc. I read the introduction about you in your book. But since most of your works are in psychology, I think you are more a (revolutionary?) psychologist than a physicist or an engineer.

I still have no clear idea how to use your theory to design the network. But the back propagation neural networks which are popularly used, in some way, may be related to feedback control. I will think about this. Maybe this is the right direction.

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Date: Mon, 24 Dec 90 13:55:11 EST

Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: "CHARLES W. TUCKER" <N050024@UNIVSCVM.BITNET>
Subject: HAPPY HOLIDAYS

901224

Dear CSG'ers,

I have tried to get this done and off for several days but there seems to be some holiday that everyone is referring to around here that calls for decorating a tree, buying gifts, getting ready to receive guests, preparing to a gathering of guests and the like. I hope that someone will tell me what all this fuss is about someday. I did read something that was quite interesting about this holiday season that y'all might enjoy: James S. Henry "Why I hate Christmas" <<New Republic>> 901231, pp. 21-24.

Something else y'all might enjoy are some quotes I copied from a 1988 calender which I thought were quite perceptive regarding living control systems. Here are just a few:

> Our most dangerous delusion is that we can control others. The hard-won truth is that we can control only ourselves.
>
> We can't control other people or events; all we're responsible for is our own behavior.
>
> Being human is difficult; we perform it imperfectly. And when we combine our effort with others -- building a building, performing a play -- we multiply our imperfections as well as our skills.
>
> Every problem teaches us how to resolve it. If we can't see the solution, then we're not ready for it, and instead of sputtering vainly, we should set that question aside and address ourselves to our appropriate tasks.
>
> Humans raise humans; if it were possible to do it perfectly, surely we would all be angels by now.
>
> If we try hard to force others to live in our world, because we think it is the real world, we are doomed to disappointment.
>
> We make the world we find --- at home, at work, and at play.
>
> Relationships are an area where many of us fail to recognize that a question can have many right answers.
>
> We are in control of our own thoughts and attitudes, and in this respect we are all powerful.
>
> Ideas that aren't applied or tested tend to become anemic; and work that isn't examined and evaluated tends to become stale.
>
> Human beings aren't error-free. We're human.
>
> Above all, let's forgive ourselves.
>
> Real freedom to be who we are can only be found among a circle of friends who have committed themselves to us, just as we've committed ourselves likewise.

Written by Karen Casey and Martha Vanceburg, Copyright 1983, 1987 Hazelden Foundation, Workman Publishing Company, 1 West 39 Street, New York, NY 10018 ISBN: 0-89480-419-7

Now on to some comments on the postings from 901213 thru 901219 (you see that I have followed the instruction of Gary and am using his dating procedure although I do find it awkward):

On Memory (Olson 901213, 901214; Marken 901213, 901214, 901214; Powers 901213,901214)

I don't think that we can re-mind ourselves too often that to take "memory" out of activity and treat it as a thing, a state, something static is to make it more difficult to understand the process and explain it. I find to talk about "remembering" as an activity makes much more sense to me and when I deal with the TOTT problem I try several procedures: if it is a name of an object be it a person, physical object, play, song, place - I go through the alphabet trying to remember what letter the name begins with; I talk with a person who had the experience with me (usually my wife- in fact many times the TOTT problem arises within our conversations); I try remembering other "facts" like who, what, when, where of the event, person, play or what ever. The point is that "memory" comes through activity and "remembering" is how we "find" it. At least I think that what happens; it is so difficult to remember these days !?!

On Instructions (Powers 901214)

I would say that most self-instructions are given in symbols (not alwys words) but I have recently observed my Grandson (who at 14 months can not speak so anyone but his parents can understand but who engages in nonverbal gestures like pointing) engaging in an activity that made me wonder about how he was telling himself what to do (I have this event on video and just observed it for the third time last night). A partial description of the event is: his mother was tell us about a book that was his favorite at the time by telling us the title, author, and some of the phrases in the book like -heads full of brains and shoes full of feet - and towards the end of her telling he walked to the cupboard that containd his toys and book an took several toys out of it and looked toward the back of the cupboard where his book are located - he reached in the cupbpard but could not get what was there - he walked to his mother and grasped her hand and pointed to the cupbpard and pulled on her hand - she got up and when to the cupbpard and he pointed to the book and she got the book sat down and began to read it to him. What symbols did he use to interpret what she was saying and tell himself to go to the cupbpard and all the other acts that he preformed I don't know but it makes me suspect the words are not the only means to provide self-instructions. I don't find an S-O-R explanation of this very satisfactory either. His mother claims he understand her but don't all mother say that about their children at that age it is only when they are older that they suddenly use another language to process the words ot others.

An instruction- a verbal or nonverbal gesture- get transformed by the hearer into instructions for an act - these are called "significant symbols" by Mead (we used this in our ABS paper) - they are symbols that are answered in a similar action by those implicited in a act together. This calls for a notion of semiotics which says that these gestures are transformed electrochemically by the hearing organism as well as the speaker. When we don't note an action similar to the one we

would perfrom given the same instruction we say the other did not understand or simply refused to persom the act. But whatever the case the hearer is still responsible for the action. My demo and all of yours use this basic idea - either you telling yourself what to do and another and through observation seeing if there is an error.

The hearer is still responsible for his/her own action since s/he gives the last instruction and thus does not have to tell him/herself the same instruction s/he has hear. We say that the speaker instruction is only an "occasion" for the hear's self-instruction so it is not a cause of it - all organisms are self-regulating.

I find many illustrations of what I am writing about on the NET: (1) in the experiment you described by Rick (901214) you used the phrase "you have to describe the point of the exercise and the subject has to agree to it" - examine those instructions and you may find words that specify a reference condition as "do what I'm doing now" is also an instruction to watch you and "imitate" or "reproduce your action" - also a reference condition may be found there - but usually I have found in some of the experiments that Rick has done at the CSG meeting that he points out "I haven't worked it out so anyone else can do it" which means to me that he can't give me very good instructions yet so I can do what the experiment calls for in action; (2) Bourbon (901217) tells us about sevral experiments where he is able to get people to do things together - he has to have some instructions to the subjects for that to happen - what are they? (3) your own example with the date of posts [Powers (901217)] is an illustration of using someone's else instruction for your own conduct. You were correct in my view - Gary did not make you do it and you are not forced to do it even today - you could still use 12/17/90 instead of 901217 and still be understood.

Another feature of making symbols for instructions is to make them redundant - present the same instruction in different modalities - the example that I like best is traffic signs (this driving stuff seems to come up all the time in our discussions) the modes used for a stop sign is: color, shape, height from ground, location and a word. I have been places where some of these modes are different and noticed that people altered their actions. Allen Funk did a trick once that put a stop sign on a sidewalk and people stopped behind his confederates; he probably did some other trick with signs but I don't watch his show that frequently to know - his show illustrates what happens when you alter instructions or "breech" instructions - a technique used by Harold Garfinkel, an ethnomethodologist, to show "taken-for-granted" instructions. More on this later.

I have been called away to plan for Santa's arrival. I wish everone a Happy Holiday and plan to be back on the NET soon.

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Date: Mon, 24 Dec 90 16:27:04 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: g-cziko@UIUC.EDU
Subject: Reference Dates

Chen (901223)

>Gary Cziko:
>I find that there may be confusion in quoting other people's posts by
>using the dates you receive. Because you receive a post today, maybe I

>receive it tomorrow or even later. So I find it difficult to follow the
>dates quoted by other people.

I'm sorry I wasn't clearer about this. By date, I mean the date that the message was SENT, not received. On messages I receive, this is always the first line of the header, and it should be the same for all recipients of the message, regardless of when they received it.--Gary

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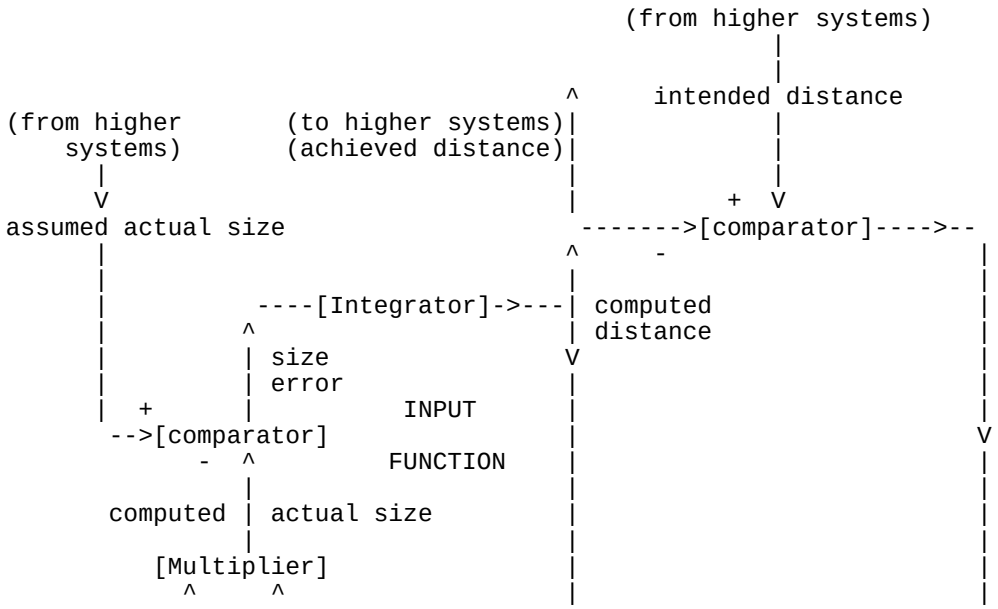
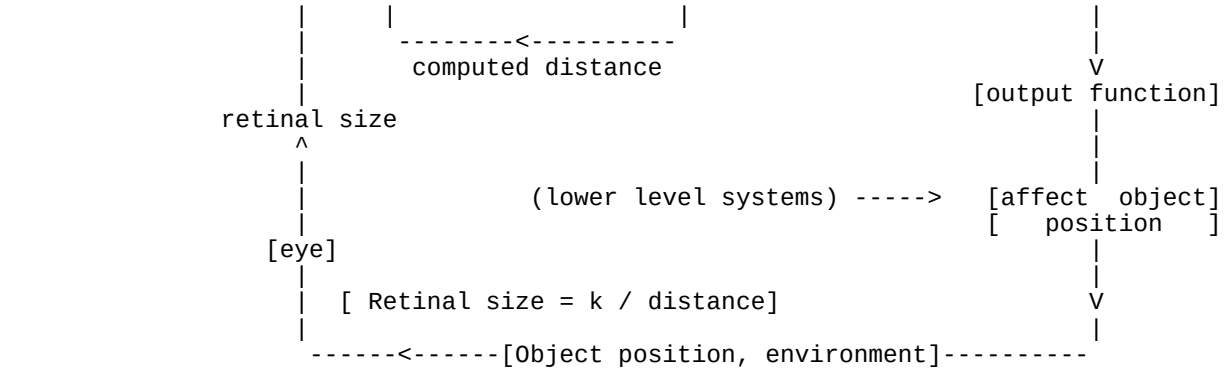
Date: Mon, 24 Dec 90 17:06:33 CST
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: Bill Powers <FREE0536@UIUCVMD.BITNET>
Subject: distance perception

Chung-Chih Chen (901223)

Probably the best date to use is the one after "Date:", which looks like the date the entry was created. I agree that sorting these posts is a real pain, and so is going through back posts to find something, at least the way I have them disorganized. Perhaps we could think up some SIMPLE header that we put in ourselves on the first line. I would love to be able to write a little program that gets rid of all the garbage that precedes every post and substitute something that I could sort on. But you never know what's going to be in that garbage, unless there's some rule I haven't been able to discern. I don't want to have to use AI here. Suggestions, anyone?

Chen, here's the diagram of that distance-sensing system I mentioned in a previous post (my 901216). This is an analog-computing setup but can easily be implemented on a digital computer. I haven't tried it but I'm sure it will work. Just don't let either input to the multiplier get too small or go negative.





This device will adjust the physical distance of the object until its monocular perceived distance matches the intended distance, given the hypothesized actual size of the object in signal terms. Can you devise a network that will produce a linear-separation signal from any two selected points on a retina? An average-linear-size signal for a whole object? This would be a little less ambitious than trying to produce a "this is a plate of scrambled eggs" signal, or even "this is an A", but it might be a step in the right direction.

Best -- bill

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Date: Wed, 26 Dec 90 06:55:40 CST
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: Bill Powers <FREE0536@UIUCVMD.BITNET>
Subject: Christmas, memory, self-instruction

Chuck Tucker (901224) --

I don't hate Christmas, although I have to take some pains to keep the myth and the commerce, on the one hand, separated from, on the other hand, happy gatherings with family and friends that the season has, in most years, meant for me. I do hate Buying-Christmas-Presents Anxiety, Phoney-Religious-Mawkishness Embarrassment, and other such psychosocial phenomena that arise mainly from my own failings. I'm not against the general principle behind religious observances (of at least some sorts): they provide a formal occasion for reviewing one's higher-order reference signals and attempting to reconcile them with commonly-held notions of the Good. Not many people have a network on which they can attempt such reconciliation's, nor have they the leisure or the support of their contemporaries that would allow time for such inner reorganizations. One might complain that religion has been the cause of more evil than good in the affairs of the human race, but we can also look on it as evidence that people are still trying to address the most important issues of human existence. I would be even more concerned about the future if they weren't even trying.

The sentiments you found on that calendar show (I assume) that common sense and experience can lead to conclusions in accord with the general concepts of control theory. How much more compelling they would be if behind them there were a shared model of how people work!

Memory --

As you say, memory is a process. But we know of so many mechanisms through

which recording and retrieval of sensory information can take place that it doesn't seem reasonable to me to dismiss "a memory" as having some form of existence even when it isn't in active use. I've speculated that memory, imagination, dreaming, and planning are all aspects of the same process. I can't think of any way to explain this general process but to say that sensory signals are physically recorded, then retrieved later for replay in the same channels they came from. They may be retrieved in novel combinations that never actually occurred before, but the basic elements retrieved, I am guessing, did occur. This simplistic picture is complicated by the fact that we can retrieve lower-order recordings and play them back in such a way that they reach higher-level perceptual functions that didn't exist at the time the original recordings were made. This puts an overlay of adult interpretations on memories that were recorded long ago, and provides a lot of room for the invention and editing of memories. Also, the fact that we can create new experiences out of the raw material in memory blurs the boundary between remembering and creating.

Instruction --

I agree with your suspicion that "the words are not the only means to provide self-instructions." I also agree that an S-O-R interpretation of instruction-following glosses over all the details that give the instructee a influence on what the instruction is to mean and how it is to be carried out. At some point the symbols in which the instruction is cast have to be translated into specific reference signals that are not symbolic. You can hear the instruction "Stand beside John," but that has to become a specific image of what the term "beside" means -- how the world is supposed to look when the instruction has been satisfied. I'm sure that there is a word for "beside" in Chinese, but it wouldn't do you any good to translate it from English to Chinese. You have to translate it into a reference signal against which a visual perception can be compared. You can't compare a visual perception against a word (unless the instruction is "write the word 'beside' "). Symbolic processes exist, in my model, at the (uh...) ninth level. At lower levels words are just perceptions like any other perceptions. So I would say that self-instruction is a ninth-level process, and that for other people to give you instructions, you have to comprehend them and work them into your own ninth-level processes. Don't forget that there are still principles and system concepts at higher levels, guiding the use of symbols but not through the use of more symbols. And maybe a "working-model" level that is still floating around in there somewhere with no number. I would argue against making the concept of "instruction" substitute for the more general concept of reference signal.

Gary Cziko (901224) --

The posts I receive start with a "Received: " header entry. Sometimes, next, there is a lot of information I don't want about what systems got the message before it was relayed to me. Then, finally, there's a "Date:" which I assume means the date the message was originally sent. I've also seen "X-to:" and "Really-from:". There doesn't seem to be any single consistent format that I can find. If there were a finite universe of possible foreplay messages, I could write a simple filter program that would condense all this stuff to a record of what I need to know about the message. This would significantly reduce the size of my files. I assume that there's nothing you can do about any of this.

Mainframe computers are big and powerful, which is nice for their owners and programmers. But their software always seems to be old-fashioned, because the basic user interfaces were devised long ago and can't be changed because there's such an investment in the data base. Only Darwin's Hammer is going to get rid of these dinosaurs.

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Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: g-cziko@UIUC.EDU
Subject: Organizing Messages

Bill Powers (901226)

Concerning messages received through the network, you write:

>There doesn't seem to be any single consistent
>format that I can find. If there were a finite universe of possible
>foreplay messages, I could write a simple filter program that would
>condense all this stuff to a record of what I need to know about the
>message. This would significantly reduce the size of my files. I assume
>that there's nothing you can do about any of this.

Right, there's nothing I can do about the message headers. But I don't see why you can't automatically strip off what you don't want. All messages should have date, from, and subject headers. Why don't you just strip off any header lines that don't start with one of these?

I know that this can be done since my telecommunications program, Eudora for the Macintosh, doesn't even display all the other stuff unless I want to see the full headers. I can sort my messages into as many mailboxes as I want. When I want to look in a mailbox, I see a list including the from, subject, and date fields. I can sort messages in a mailbox by any of these fields, and search for specific text in any message (ain't I wonderful?). If all this can be done on the Mac, it's probably possible on the IBM as well.

Hopefully, Greg Williams will take care of all of this for us when he puts out is CSGnet Digest or equivalent. That is, if enough people encourage him to do so.--Gary

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Date: Wed, 26 Dec 90 13:54:39 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: Joel Bennett Judd <jbjg7967@UXA.CSO.UIUC.EDU>

NOTE (12/26): I was having computer problems a couple weeks back and I think this got lost in the shuffle. The Memory discussion has kind of died down, but I'd still be interested in thoughts concerning Critical Periods.

The question about the T-O-T-T phenomenon and reply reminded me of the psychological and psycholinguistic work done with memory, and I want to mention a couple of specific examples just to see what thoughts those with a CT perspective might have regarding them. The statement which reminded me of these experiments was:

"The higher order system sending the reference signal is probably the one that wants to experience a particular perception..."

The work I'm thinking of is that done with witness memory that changes over time; with closure and the finding that a memory of open geometric shapes slowly closes those shapes after a while; and sentence memory tasks where even immediate recall doesn't affect the fact that we will replace uncommon or infrequent words or expressions with more common ones, or even complete incomplete actions or results described in the original sentences.

So it seems that, lying aside, we purposely change memories to conform to an internal reference perception, is that right? And in general, it seems that these references become more rigid as we get older (conservatism, narrow-mindedness; you can't teach an old dog...)?? And then what would CT say about Critical Periods of development? Are these lower levels of the hierarchy which are essential for higher order control? That would seem to address why some things are more "critical" (inflexible) than others.

Joel.

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Date: Wed, 26 Dec 90 19:49:25 CST
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: Bill Powers <FREE0536@UIUCVMD.BITNET>
Subject: Memory editing

Joel Judd (901226) --

"So it seems that, lying aside, we purposely change memories to conform to an internal reference perception, is that right? And in general, it seems that these references become more rigid as we get older (conservatism, narrow-mindedness; you can't teach an old dog...)?? And then what would CT say about Critical Periods of development? Are these lower levels of the hierarchy which are essential for higher order control? That would seem to address why some things are more "critical" (inflexible) than others."

I take this personally, of course. I don't think of it as conservatism so much as just getting righter and righter. I think we tidy up our world-models in our spare time, until all the loose ends that bother us (not necessarily those that bother other people) are taken care of. I've tried to resist that temptation, just letting the loose ends dangle even though the model would be nicer if they were clipped off or otherwise concealed from view. I can still say "I don't know" without getting red in the face. But of course if the traits you're talking about are really just a natural consequence of getting old, I guess I'm doomed to turn into an opinionated bore. If I haven't already managed to do that.

There's another angle to this, though. Suppose someone explains to you how an internal combustion engine works. And suppose you start playing with what you remember of the explanation, changing this and adding that. Eventually you might come up with a way a different kind of engine works, one that hasn't been built yet. Obviously this isn't "misremembering." It's creativity and invention. But it looks like misremembering, particularly if you end up believing that the new design is exactly what the original explanation described. This can happen when you get only part of the explanation (or see only an ambiguous example). Then you go off and think about it until what you heard or saw makes sense. When you later find out more about what you were told or what was happening, you realize that you've understood and remembered something quite valid, but different.

Is it possible to edit memory so thoroughly that you lose the original form and recall instead only the revised form? I think it is. This isn't a happy idea for anyone who values memories. Maybe, though, by recognizing this as a likelihood, we can learn that the past, once it's no longer the present, is truly gone forever. You can't go home again because sooner or later you revise your idea of what home was. If you still bear a grudge against your parents, they gradually turn into cardboard monsters in your memory. If

they protected you against all of life's little problems, you remember them as cardboard saints. In the first case you want the past to come back so you can get even. In the second, you want it back so you can feel safe. So maybe editing memories is some kind of attempt to deal with present problems that you're having trouble with.

Maybe the question to ask about editing memories is "what does it get you?" instead of "does it happen?" And the next question is, "How does it work?" You'd know better than I, but I think I've read that events are remembered well if they are remembered often. This doesn't necessarily mean that they're remembered accurately -- only that they don't fade away leaving nothing there. Maybe memories get changed if you deliberately alter them (in imagination) every time you recall them. The edited version gradually takes the place of the original. Can you think of an experiment that might demonstrate that effect? I can see why a witness might do such a thing. It's harder to see why a person would prefer a closed figure to an open one, but maybe if people often alter their memories of figures to make them more closed, that act reveals a reference signal.

Going back to an earlier paragraph, maybe it's not just the onset of senility that makes older people make their memories conform to standard patterns more than younger ones do (a thesis, by the way, that I would argue in just the opposite direction). Maybe it's a skill that, if learned too well at a young age, can only improve. As you can see, I am pleased to say that I have a firm answer for all your questions: maybe. Apologies to Mark Twain.

I don't know much about Critical Periods of development. See the work by Frans X. Plooij ("The behavioral development of free-living chimpanzee babies and infants", Norwood, NJ: Ablex (1984).

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Date: Thu, 27 Dec 90 17:09:04 +1100
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: mark@WACSVAX.CS.UWA.OZ.AU

Chung-Chih Chen (901216):

Sorry for the delay but I am having email problems here. I am typing this message using SMTP on a telnet connection to the sendmail port of a machine on the other side of OZ. Hope this works.

> It seems that each agent has several skills.

Correct!

> I still don't understand
> why you need agent and skill as two different things?

An agent is a collection of things which together are responsible for solving a particular aspect of the control problem that is being addressed. A society of these agents will cooperate according to the defined social structure (i.e. the control system's organization) to produce a net behaviour which solves that problem. A skill is only one type of component in an agent; others are: short term and long term memories, and activation rules.

> Why don't you use
> one agent as one skill? Isn't that simpler?

It may be possible to implement a skill as an agent and then have a society of these skills forming the original agent. I had thought of this long ago

and may eventually do this. However, there is one problem that (for the time being) forces me to distinguish between agent and skill. The problem is that arbitration must be performed upon a set of skills which are designed to solve the same aspect of control using different criteria.

For example, consider a robot path that has been computed so that it is free of stationary obstacles. In my system the next step is to compute the linear` velocity along this path so as to evade any moving objects. Now, given a perception of a moving object and the desired robot path, only one skill is then chosen from the set of relevant skills to compute a safe velocity. To choose that skill requires knowledge of all those skills in that set. The easiest way to do this is to have a central arbitrator for this particular set of skills, hence the activation rules. Every agent has such a central arbitrator which specialises for the associated aspect of control and so conflict resolution is also distributed throughout the control system.

Later I may take another look at conflict resolution and try to come up with something that will lead to a simpler agent definition; perhaps one that will not distinguish between agent and skill.

Mark

  -
  o   Mark Nelson
 < -   Computer Science Department
 / >   The University of Western Australia
' ~

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Date: Thu, 27 Dec 90 08:55:01 CST
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: Bill Powers <FREE0536@UIUCVMD.BITNET>
Subject: Agents and skills: crowd program

Mark Nelson (901227) --

In the "Crowd" program that I wrote in 1989 (and am still developing) for Chuck Tucker and Clark McPhail (sociologists in the CSG), each actor is composed of several simple control systems. There are two basic "skills" -- moving at a controllable speed, and turning at a controllable rate. These skills are used by as many as seven control systems, often in a "conflicting" manner. Usually, however, only four or six of the control systems are made active for a given "person." They work in pairs, each pair controlling a speed and a direction.

There can be up to 255 individuals on a screen. Active individuals move according to their own control activities. Inactive individuals are stationary and are just obstacles. Groups of individuals can be defined so that they share characteristics.

Proximity control:

Proximity is defined as k/distance-squared (from a pre-calculated table). Each control system concerned with proximity compares sensed proximity (from the table-lookup) of a given object, compares it with a reference-proximity, and generates an error signal that affects speed of movement (regardless of direction). There are two proximity tables: a steeply-declining function used for avoidance, and a more slowly-declining function used for goal-seeking.

Destination-seeking: The reference level for a destination-seeking system is normally set to the maximum possible proximity to a specified destination position on the field of play (255 units). The proximity error is near maximum until the person gets close to the goal (because of the

inverse-square definition of proximity). Thus the speed due to destination-proximity error is maximum until the goal is nearly reached.

Person-seeking: The reference level for proximity to a single designated person is usually set to less than maximum, corresponding to a separation of a few feet. The proximity error contributes to the speed of movement, the contribution being zero at the reference-proximity. A negative error subtracts from the total speed signal.

Group-seeking: This is just like person-seeking, but the position sought is the centroid of the positions of all other members of the group to which the active person belongs. Proximity is defined as the proximity to the centroid. In the version under development there can be 16 groups.

Person-avoidance (always used): The proximity avoidance control systems sense only negative error, meaning that the reference proximity is less than the sensed proximity. Error is zero if sensed proximity is less than the reference proximity. The reference level for proximity is usually set to zero, so only negative error can occur. As proximity error rises, the speed of movement is slowed.

Direction control:

Whatever the speed of travel, the direction of movement is (on one iteration) the sum of the current azimuth angle and all the error signals that affect direction. So direction is the integrated sum of angle error signals.

Destination-seeking: The directional difference between the momentary velocity vector and a spatially fixed goal is sensed: the reference-direction is always set to zero (non-adjustable) to indicate that the person is to move directly toward the goal. The error signal contributes to the rate of change of direction.

Person-seeking: Just like destination-seeking, but the angles are sensed as the difference between current direction of travel and the direction to a specific other person.

Group-seeking: same as person-seeking, but the direction sought is that of the centroid of all other persons in the same group.

Person-avoidance (always used): The sum of all proximities of other people to the left of the direction of travel is found to yield a "proximity-left" signal, and a similar sum is used to derive "proximity-right". The sum of these two integrated proximity signals is used to control proximity as described above. The difference is used to contribute to the rate of change of direction of travel.

There's a complication needed to avoid positive feedback. If the left proximity is greater than the right proximity, the difference signal is multiplied by 1 to create the error signal. If the opposite is true, the difference signal is multiplied by -1. Thus negative feedback is preserved whether passing a single nearby obstacle on the left or on the right. To prevent impasses, the error signal is dithered by a small random amount on each iteration.

General:

Avoidance of obstacles and seeking of a goal position or another person's position can generate conflict. The outcome depends on the various loop gains and reference levels set for the various control systems. If the destination-seeking loop gain is much higher than the avoidance gain, there are collisions. The normal case is for the avoidance gains to be much higher than the seeking gains. The result is very efficient progress toward

a goal in a sparsely-populatated field, and a very tortuous approach when there are many people on the field. The program is set up so that all gains and reference signals can be adjusted. Some very intelligent-looking behavior results, even though there is no path-planning at all, and no intelligent choice of routes between obstacles. Perhaps Tucker and McPhail might want to add some comments on the behavior of this model.

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Date: Thu, 27 Dec 90 15:42:16 EST
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: "CHARLES W. TUCKER" <N050024@UNIVSCVM.BITNET>
Subject: SOME MORE COMMENTS FOR THE NEW YEAR

901226

Dear CSG'ers,

I was drawn away from the NET and did not finish my last note. We all had a great time beginning Chritmas Eve with a gathering for 27 people, followed by breakfast at another home, dinner at another and returning here to finally do "Santa Claus" about 8PM. As usually there was so much "stuff" for the children that there was no room for it in the car and we still have it here at the house. Mother will be arriving tomorrow and we will be heading for the beach on Saturday or Sunday to spend New Year's Eve there with my son Curtis and his friends. I may not send another message until next year so all have happy and safe celebration for the eve and try to find a way those that make such decisions not to start a war [one of those times that we wished we could control another's conduct !]. This leads to my topic - positive feedback.

Postive Feedback (Powers 901214)

I know this is an old one but I thought that the "obsessive-complusive" characterization of positive feedback was very important and I don't think that I really appreciated its role in living systems until the comment. As I take it the correction of error is not positive feedback but simply the correction of error but when the error fails to be corrected (for whatever reason) and the error continues to increase then we have a process of positive feedback. I take it that positive feedback will eventually destroy the living system, e.g., cancer would be a positive feedback process in the living body that will destroy it. Or any behavior that we might call "addictions" (negative ones not positive ones as Glasser would have it) beyond the ordinary amount of food, drink, and the like. Now if this is the case then "chronic mental illness" as characterized by Scheff (and modelled by Walter Buckley) is a process of positive feedback until the medical organizations take over and then it becomes an error to act sane because the reference conditions for behavior are specified by the role of "being mentally ill" (you can then only be a former mental patient after that but always a mental patient or certainly one who is "crazy"). This is one of the major reasons the Glasser and a few others like those who do clinical work in CSG avoid the medical vocabulary and do not talk of psychiatric illnesses or mental illness but rather irresponsible or untoward or difficult or bothersome conduct. Does this make sense or does it sound crazy?

901227

Another aspect of so-called "mental illness" is that not all of these "conditions" can be accounted for by using positive feedback some such conduct is purposive, i.e., a person acts "crazy" with the purpose to offend others. Erving Goffman did a wonderful job describing such action in his "The Insanity of Place" and in various places in his book and collections fo essays; I get the sense that Goffman had a up close and personal experience with these actions.

This discussion of "positive feedback" bring up at least two questions: How is this related to reorganization of a living system? and How can such conduct be modelled so as to be better understood? I think that its relevance for reorganization is quite direct - if it does not occur the system will terminate existence or dramatic change its conduct but I am not ready to describe these events in detail. I think that Bourbon's experiments and perhaps my demo can be altered so that one subject will continue to make errors that can not be corrected so a "positive feedback" process will be observed. Of course these studies have to be carefully done since if such actions lead to reorganization as we would speculate then we might have a "Milgram" type situation on our hands. Thought and planning are essential for such studies. More on this later I hope.

A thought about the "Gulf Crisis" or (How could we be so stupid as to get into another family feud in someone else's yard?)

I listened to the former Prime Minister of GB Heath when he testified before the House Committee a week or so ago when he was accounting for Saddam's "miscalulation" of the situation, i.e., Saddam did not believe that we would bother with his invasion of Kwait. (Heath's entire testimony was excellent and quite beyond many House members) My interpretation of the entire account was that Saddam made the mistake most people do by looking at actions and ignoring what the actor is controlling for. I must admit this is particularly difficult with this administration, especially George Bush (apparently), since he seems to keep shifting what he is controlling for - one day it is economics, another it is aggression and still another morality. But I thought that Heath's account was readily and reasonably interpreted in a sociocybernetic frame. I would be useful to have a number of such interpretations to indicate the utility of our interpretation.

Finally, I found some statements in Bill Glasser's recent book <<The Quality School>> which I like and thought you might like:

A boss drives. A leader leads
A boss relies on authority. A leader relies on cooperation.
A boss says "I." A leader says "We."
A boss creates fear. A leader creates confidence.
A boss know how. A leader shows how.
A boss creates resentment. A leader breeds enthusiasm.
A boss fixes blame. A leader fixes mistakes.
A boss makes work drudgery. A leader makes work interesting.

A question is: Where are our leaders?

Happy New Year

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Date: Fri, 28 Dec 90 08:20:57 CST
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: Bill Powers <FREE0536@UIUCVMD.BITNET>

Subject: Correction, description of crowd program

Mark Nelson -- correction of my 901227 --

I described the directional control for avoidance incorrectly. The SUM of proximity-left and proximity-right signals is also used for directional avoidance -- not the difference. The error signal that contributes to turn rate depends on the error between reference proximity and TOTAL proximity. The SIGN of the error is multiplied by the sign of (left-proximity minus right-proximity) -- that is how negative feedback is maintained. Thus speed and direction (for avoidance) are both based on the TOTAL proximity error signal.

Also I forgot to mention that the proximities derived from the distance to all other persons are weighted by the cosine of half the angle between the direction of travel and the direction of each person. So proximity directly to the rear is given a weight of zero (cosine of pi/2), and so on. Integer arithmetic is used throughout, with trigonometric functions taken from precalculated tables where the least angle unit is 1/4096 of a circle.

Best -- Bill

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Date: Fri, 28 Dec 90 13:01:15 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: Joel Bennett Judd <jbjg7967@UXA.CSO.UIUC.EDU>
Subject: Re: Memory editing

Bill Powers (901227),

>I take this personally, of course. I don't think of it as conservatism so
>much as just getting righter and righter. I think we tidy up our world-
>models in our spare time, until all the loose ends that bother us (not
>necessarily those that bother other people) are taken care of.

No offense...I'm just reorganizing to accommodate a CT frame of reference. As I do so, I think of what's currently going through my mind and try to understand how it would be viewed from a CT perspective. The "old dog..." metaphor is just a piece of societal philosophy, but one of many trite phrases we use that could show a sort of implicit awareness of CT (kind of like Tucker's calendar). In fact, "frame of reference" has a sort of CT ring to it too, doesn't it? The whole memory discussion struck a chord because I was reading an account of some western pioneers, which included some of my forefathers, and I was surprised at how often the author described how difficult or trying a period of time would be, and then would compare the 'factual' account with someone's recollection of it. The recollection would invariably be described by the author with the phrase "the afterglow of memory", and would rarely reflect the hunger or danger or trials or whatever of the episode remembered. Having just entered that "thirtysomething" period of life this year I'm taking memories a little more seriously and appreciate your thoughts on the matter.

>But it looks like misremembering, particularly if
>you end up believing that the new design is exactly what the original
>explanation described. This can happen when you get only part of the
>explanation (or see only an ambiguous example). Then you go off and think
>about it until what you heard or saw makes sense. When you later find out
>more about what you were told or what was happening, you realize that
>you've understood and remembered something quite valid, but different.

>And the next question is, "How does it work?"

>You'd know better than I, but I think I've read that events are remembered
>well if they are remembered often. This doesn't necessarily mean that
>they're remembered accurately -- only that they don't fade away leaving
>nothing there. Maybe memories get changed if you deliberately alter them
>(in imagination) every time you recall them. The edited version gradually
>takes the place of the original. Can you think of an experiment that might
>demonstrate that effect? I can see why a witness might do such a thing.
>It's harder to see why a person would prefer a closed figure to an open
>one, but maybe if people often alter their memories of figures to make them
>more closed, that act reveals a reference signal.
>
My point in bringing up my memories from a Psycholinguistics class (and I should go look up some of the references to what I was alluding to), is that some memory changes are virtually immediate - you can read a sheet of 12 sentences, then try to write them down right after, and they will be different (William Brewer was the instructor and author of some of the papers we read). Now, if you know you are participating in a memory experiment, and deliberately memorize exactly what you read, you will recall the exact S's, up to the limits of immediate recall. But it seems as though we are PREPARED to fashion certain memories; we almost WANT (?) to remember things certain ways. What the sentence work led into in the class was schema theory. When asked to recall things like professor's offices, subjects will fill in missing things which "should" be in a Prof's office, eg. books. And there was also some effect when anomalous things like skulls were placed in the scene.

But I'm getting to the limit of my knowledge in this area. I guess these questions get at HOW it might work, I would say that the question of WHAT does it get you could simplistically be answered: less error. All in all it seems to me that the phenomena are pretty powerful, or rather the notion that control systems are sort of prepared to [directly] affect memories is powerful. And then there's the related issue of forgetting...

Joel

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Date: Fri, 28 Dec 90 19:30:39 CST
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: Bill Powers <FREE0536@UIUCVMD.BITNET>
Subject: Memory

Joel Judd (901227 and previous) --

Editing of memories could be demonstrated only if you could show that a person remembered correctly at one time and then incorrectly at a later time. If that can't be demonstrated, we have to suspect not memory but perception. Your mention of strong <immediate> effects makes me lean toward the perception hypothesis: what's recorded is played back faithfully, but the original perception was modified or embellished before it got to the recorder.

If you looked at a professor's office for a couple of seconds, what would you have perceived of it? I doubt that you would have noticed the position, color, size, and angle of every book on every shelf. You just noted "books" -- a category -- on "shelves" -- another category. If that was what you paid attention to, that is what you remember, the names of categories. Later, if you're asked to say what books were in Professor Cziko's office, you would probably say "Books on education and other stuff," and perhaps a few titles you happened to notice ("Debbie teaches discipline") which you would interpret (during recall) as a book on education. You would imagine details that satisfy the memory of the categories -- you weren't attending at a low enough level to record them. This amount to a proposal that attention has something to do with remembering. Any data on that?

When we look at any scene, we are imagining large parts of it. The fertility of perceptual imagination was illustrated to a class I was in, by Oliver J. Lee, the general semanticist. He said "Suppose I tell you that there is a picture on the wall here, and it's a picture of a boy walking away from you down a road." He then asked a series of questions, more or less as follows -- try your own answers:

Where is the boy going?
What is the boy wearing?
Does he have shoes on?
Is there anything on his face?
What color is his hair?
What's in his hip pocket?
Is there anything around his neck? What color?
What is he carrying?
How is he carrying it?
What is it made of?
What is attached to it? And to that?
Is he alone?
Is the road paved?
What kind of day is it?
What are the sky conditions?
Where is this scene taking place?
How long ago did it take place?
What is the picture part of?

As you answer each question, more questions could be asked that would result in even more detail. And everything is completely imaginary. If your age or cultural background doesn't resonate to this example, I'm sure you can make up one that does work.

When you look at a rug in your living room, do you imagine that it extends under the furniture? When you look out a window, do you imagine that the scene extends beyond the windowframe? When you get out a new sheet of paper, do you imagine that it is uniformly white over its whole surface?

I think that when we perceive a scene, we attend to it from some fairly high-level point of view. We supply continuity, resolve ambiguities, fill in expected details, by using imagination at lower levels. The result gets recorded. When we remember the scene, we get back what was recorded, including the imagined parts just as if they had really been there. So memories are only as accurate as the perceptions on which they are based.

There would seem to be a lot of opportunity here to reinterpret some existing experiments and to think up some new ones.

Best -- Bill

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Date: Sat, 29 Dec 90 16:47:28 EST
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: Dennis Delprato <USERXEAK@UMICHUM.BITNET>
Subject: Notterman & Mintz & History

REALLY FROM: Dennis <DELPRATO@UM.CC.UMICH.EDU>

Bill Powers (901223)--

Let me clarify my opinion of Notterman & Mintz ("Dynamics of Response," 1965--my message of 901217). I was impressed with two

features of this work. First, the use of a strain-gauge to transduce rats' operations on their environment: as you noted, this instrumentation has much to offer by way of nonhuman control systems research. Furthermore, think of its potential for use with infants and other humans with deficient communicative mechanisms, as well as for control system work with problems that come up in rehabilitative medicine. I found the report of your experience at Northwestern U. most disheartening--but too believable, given the power of tradition in psychology and in other areas, as well. (I am here referring to Powers's message of 901218 in which he related the refusal of faculty to approve master's research in which a strain-gauge would have been used).

The other area in which Notterman and Mintz impressed me was at the interpretative level. I do not maintain that the quotations were faithful to the researchers' procedures and data, at least as I could determine. Note my crack about their amusing attempt to concoct feedback loops. However, this point brings up a much larger issue--that of interpretation of data. Data never speak. (Need I remind CSG-NET participants of this?) Rather humans must "intervene" and put forth accounts of what data mean. It is for this reason that I find historical-critical analyses indispensable in science. Here is the power of Mach's contribution (1883) to mechanics. Mach's position was that one cannot understand mechanics in the absence of "a critical and historical account of its development." It is my understanding that Mach made a great contribution to the science of mechanics.

How does the above bear on Notterman and Mintz and the control theoretical approach to psychological behavior? Briefly put, all the data in the world will not be sufficient for large-scale acceptance of control theory. Tom Bourbon's posting (901222) re. BBS episodesBBS episo

nicely (unfortunately) illustrates my point. Mainstream biobehavioral scientists are virtually completely ignorant of the evolution of thinking about psychological events. They, for example, have no idea of the sources of the lineal mechanisms they espouse and from which they conjure up one version after another of "trendy" INTERPRETATIONS. I am by no means "brilliant," but I will say that given what I know about the scientific evolution of psychology, there is no way I can promote and pass on the sort of refurbished lineal mechanisms and dualisms that BBS participants take as "state of the art." I have UNDERGRADUATES who will not buy these hoary "truths." This is not to say that detailed study of the scientific evolution of thinking about psychological events is necessary for everyone to come around to control system and related interpretations, for science can only refer to the behavior of scientists who develop and live in a complex scientific matrix that, in turn, is situated in an even more complex socio-cultural matrix. It is likely that I and my students are about the only collection of individuals who came to control theoretical views by way of studying the scientific evolution of psychology. Others have come by way of a variety of alternative routes, for history is not hidden and available only by way of mysterious extractive methods. People live, observe, think, and so on. I submit that most who are sympathetic to control systems interpretations have contacted some important developments that are illuminated by formal study of the scientific evolution of psychology.

As for Notterman and Mintz's interpretations at the end of their book, I am impressed that they must have made some contacts that are crucial to a control theoretic perspective. The fact that the heart of their book is so mainstream, to me, makes their INTERPRETATIONS all the more telling. Simply put, it indicates that feedback control ideas were around (of course they were) and that some combination of factors led Notterman and Mintz to speak the way they did to end up their book.

Thus, they are only protobehavioral control systems theorists in the sense of "early form of," as Kant was a protoscientist of psychology. To the psychological scientist of today, Kant is (or should be) irrelevant. To today's control system researcher, Notterman and Mintz's work (apart from a basic technological innovation) _is_ irrelevant. As transitional figures, they transmitted a little of the new, and a whole lot of the old.

As should be clear from the above, I am interested in how ideas become accepted in psychological science and in the macrohistory of psychology. With but one exception, extant histories are mostly misguided microhistories that function to perpetuate traditional thinking about behavior by forcing tons of trivia onto readers. Theeader closest we have to Mach's "The Science of Mechanics: A Critical and Historical Account of Its Development" is J. R. Kantor's "The Scientific Evolution of Psychology" (2 vol.) which unfortunately could come across to readers as a promotion of yet another version of behaviorism in the form of "interbehaviorism." However, unbiased study reveals that Kantor's conclusion is that the sweep of history points the way to nothing but a naturalistic science of psychological events.

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