

Misunderstandings and distortions. See Dispute_PCT.pdf

Unedited posts from archives of CSG-L (see INTROCSG.NET):

This thread deals with myths, misunderstandings, and distortions of feedback, cybernetics and PCT. The term Devil's Bibliography was suggested and stuck. Thus this file is called DevilsBibliography.

See file ToughSell.pdf for the following post by Tom Bourbon as a suitable starting point:

Date: Thu Oct 22, 1992 6:21 pm PST
Subject: PCT popularity; Why 99%?

Date: Sat Oct 24, 1992 6:19 pm PST
Subject: Objections to PCT

[from Gary Cziko 921025.0200 GMT]

To Bill Powers, Greg Williams & Tom Bourbon and other interested parties:

When I try to discuss PCT with "mainstream" psychologists, two objections often come up: (a) feedback is too slow for many behaviors; (b) deafferentated animals can behave with no sensory input.

I know that both of these subjects have been discussed on CSGnet in the past, but I wonder if there exists published or unpublished papers which address these issues with more rigor. If these don't exist, perhaps they should.

In the meantime, I remember Bill having made comments about the "speed" objection (and perhaps the deafferentation objection too) and Tom having made comments about the deafferentation studies and I would appreciate if they could summarize their arguments. From Greg I would expect some arguments that the lowest levels may not be fast enough for feedback control with the control effectuated by higher levels sending output commands and perceiving the results of the commands (although I don't want to start Bill vs. Greg on yet another topic before they've settled the current one).

If these objections against PCT are so common, then perhaps PCTers should have objections ready against the objections. --Gary

Date: Sun Oct 25, 1992 7:31 am PST
Subject: Objections to PCT

[From Bill Powers (921025.0800)] Gary Cziko (921025.0145) --

> When I try to discuss PCT with "mainstream" psychologists, two objections often come up: (a) feedback is too slow for many behaviors; (b) deafferentated animals can behave with no sensory input.

The first objection is a myth tracing back to cybernetics; the second is a straw man argument.

First objection:

If you think of feedback as something that follows after the end of a behavior, then of course feedback is too slow. When you realize that feedback actually starts at the instant that action begins and continues throughout the action, however, that lag disappears.

Another "slowness" commonly cited is reaction time. The reaction time people usually think of is 200 milliseconds, the reaction time of a saccade or of a motor response to a sudden visual stimulus. This is far longer than actual delays in kinesthetic control systems. The delay in the lowest spinal-cord

control loops is 9 or 10 milliseconds, and in brainstem loops only about 50 milliseconds.

What laymen don't realize is that even WITH lags, a control system can be stabilized by the use of proper temporal filtering in the loop. William Ashby, who did much to start these myths, was a psychiatrist; he didn't know anything about stabilizing control systems. In fact, a properly stabilized control system with a lag can reach equilibrium in one reaction-time, given good filtering. See my Psych Rev article "spadework" [Quantitative Analysis of Purposive Systems: Some spadework at the Foundations of Scientific Psychology, reprinted in Living Control Systems, Vol I] where I lay out the requirements for achieving this by using a "slowing factor."

Another related myth is that a stimulus-response system must be faster than a control system. A properly-filtered control system is always at least as fast as, and usually much faster than, a straight-through SR system using the same components but without feedback. The reason is not hard to understand.

If you want a response proportional to a stimulus without feedback, you have to adjust the gain so that in the steady state, the output is of the required magnitude in relation to the input. The device responds by producing an output that rises exponentially to a final value given a step input (all real devices have at least this kind of lag). The maximum possible speed of response without feedback is thus set by the inherent slowness of the physical device.

If we use negative feedback from the output to the input, we can arrange the feedback ratio so that in the final steady state, when the feedback cancels the input, the output is again of the required magnitude. Now, however, we can greatly increase the amplification in the device itself, which is in the forward part of the loop. This does not speed the device up; it still takes as long as before to reach, say, 90% of the final output value. All it does is make that final output value much larger.

Thus when a step input occurs, the initial response of the device is such as to approach an output value that is many times as great as the desired amount of response. The output begins to rise very much faster than in the case without feedback. If there were no feedback, the result would be an enormous overshoot of the desired output value. But as the output increases, so does the negative feedback. When the final state is reached, the negative feedback is canceling most of the input, and the output becomes exactly the desired amount with no overshoot at all. But the time taken to reach the final state is only a fraction of what it would be without the feedback.

This is exactly what H. S. Black discovered in 1929. He found that vacuum tube amplifiers with a certain inherent gain and bandwidth could be used to achieve not only far more stable gain but a much wider bandwidth, through the use of negative feedback. This knowledge never got into cybernetics, and thus never got into psychology, and thus failed to inform the mythmakers in these fields. Control engineers didn't read the psychological literature, so they never set the record straight.

The fact is that a design with negative feedback is almost always faster in response than a design without negative feedback.

Deafferentation:

This objection is a straw man. There has been a great deal of grisly and clumsy research aimed at disproving a claim that nobody ever made: that without feedback, there can be no behavior. Taub and Bizzi and others, being ignorant of control theory, misinterpreted what control theorists have to say about feedback, and set out to disprove their own misinterpretation. A simple phone call to the right people could have saved decades of effort and a whole lot of misery.

Consider the following control diagram.

Date: Tue Dec 15, 1992 3:05 pm PST
Subject: Where are the Goals

[FROM: Dennis Delprato (921215)]

Only skimmed over reviewer's comments on Rick's most recent clash with tradition. Two quick points:

1. On goals: One impediment to people like the reviewers grasping PCT is the tendency to treat PCT as saying something about what they know as goals (fine). BUT the problem is that they do not get away from the conventional notion that the goals are "out there." I suggest that this one important factor in many not grasping what the likes of Rick are saying.
2. On feedback control: The crude reactions to feedback control especially frustrate me. Mainstreamers seem to take one or more of the following positions:
 - a. Feedback control is not important in psychological behavior.
 - b. It is important but we know all there is to know about what's going on here. Let's get on with the important stuff.
 - c. Feedback? Oh, you mean reinforcement. This has been beaten to death. Or-- I am quite up on reinforcement, am continuing the work of the great learning theorists.
 - d. Feedback control -- I agree it is important for motor skills, but we are not interested in this area.
 - e. Feedback? -- Too mechanical, OK for machines but not for....
 - f. Feedback? That's information on how well one is approximating a goal that is out there in one's external environment.
 - g. Feedback? Control? I don't know what your are talking about and that ain't all 'cause I don't give a damn either.

This is not a very sophisticated classification of reactions to feedback control, but it does begin to give some idea of how it is that K. U. Smith's and now Powers's work tends to be met with wide yawns.

Date: Wed Dec 16, 1992 5:42 pm PST
Subject: Point of View

[FROM: Dennis Delprato (921215)]

Rick Marken's recent replies to reviewers of his "Blindman" paper brought home what I suggest is another major roadblock to comprehension of PCT. Recall his major thesis:

"If organisms are in a negative feedback situation with respect to the environment, then their behavior will APPEAR to be SR, reinforcement and cognitive when it is actually NOT -- it is CONTROL OF PERCEPTION". (921213)

This is obvious to CSG-L participants, but a most remarkable position to just about everyone else who is involved in bio-behavioral science. What makes it so difficult is that it rules out the classical science idea of the independent observer. The above fundamental of PCT is based on a particular "point of view." The point of view is NOT one of the conventional independent observer/experimenter of the classic independent variable-dependent variable framework of mainstream psychological science. In fact, basically, Rick's point is that as long as researchers stick to the classic methodology, they will never detect the very different picture from the one tradition yields. From its point of view, behavior is best described as S-->R, motor program --> movement,

The issue seems to be somewhat related to what physics went through in moving from classic mechanics/physics to what the professional physics literature refers to as the new (or modern, or relativistic, or quantum) physics. As physicists more and more went into the microscopic world, they found, to their dismay, that the physical events could not be described and known independently of the behavior of the observer, including the particulars of the conditions of observation (e.g., "measuring instruments"). They didn't

realize it then, and still don't, but they discovered that the dichotomy between a physical world and a psychological world (so much a part of our cultural tradition) no longer made sense, except as fiction. Notions such as indeterminacy and complementarity evolved to help thinkers cope with the confusing conclusion that a physical world independent of the observer, and thus an observer independent of the physical world as well, no longer fit into formal scientific formulations. Only the vernacular, but not the formal scientific language, allowed preservation of the traditional dualisms.

As Dewey and Bentley (in *Knowing and the Known*, 1949) put it, physical science was forced to adopt transactional procedures of inquiry in which "transaction" refers to the "full ongoing process in a field where all aspects and phases of the field, including inquirers themselves, are in common process."

Although it is possible to find hints of recognition of the need for transactional procedures of inquiry in psychology, as we full well know, classic independent variable - dependent variable with independent observer is the only approach to inquiry even taught at the highest levels of psychological training. (And it is most unfortunate when one has to resort to "naturalistic observation" or to "correlational" methods. But at least one can work like the dickens to keep the observer *independent* in these cases.) Rick is asking quite a bit of his readers. I imagine few even recognize what they are being asked to consider. If we examined them in great depth, I'll bet that the most astute will get as far as something like, "This is your (Dr. Marken) point of view. I suppose you believe it sincerely and with good reason, but I Cannot buy it. I cannot get into your shoes/head/mind."

I fear that wider acceptance of PCT views will require more direct consideration of the inevitable role of the observer in psychological inquiry. When this has been addressed in the mainstream literature, the observer has been taken as a creator of data and knowledge, with the result of preserving material- spiritual dualism. I believe the PCT alternative is one in which knowledge is relative to the observer. My suggested emphasis may seem unnecessary, but the equations do not interpret themselves, as should be obvious by now.

Dennis Delprato

Date: Sun Dec 20, 1992 9:08 am PST

Subject: Misstatements & Other Basics

[FROM: Dennis Delprato (921220)] >(Bill Powers (921218.1500)

>RE: Feedback is too delayed.

> Dennis, would you be willing to become a repository for citations from the literature containing misstatements about feedback control, PCT, etc.?

Pleased to, especially given that I seem to have already begun this out of my own curiosity.

Note another major roadblock that you bring up in the following: "I am a professional control-system engineer / I have a close friend who is a real control-system engineer / I have a high IQ and studied control system engineering / ... and you simply are not getting it right at all. Too bad, too, since the idea of applying control system theory to humans is an excellent way of showing how psychology is nothing but physics."

>Bruce Nevin (921218.1324) --

> That Latin saying developed into a most interesting and relevant discussion. It is surely true that our most profound problems in introducing PCT come from those who think they already have a grasp of what feedback and control are about. You'll remember that a year or so ago we had a participant on the net who was a "real control-system engineer." He obviously understood control systems -- but he absolutely could not accept the statement that control systems control their inputs!

He eventually bade us farewell, saying in a perfectly friendly way that he just couldn't go along with this strange way of looking at control systems, but good luck to us.

Dennis Delprato

Date: Mon Dec 21, 1992 11:49 am PST
Subject: Re: Martin to Rick on Shannon

From: Tom Bourbon (921221 10:15) [Martin Taylor 921218 18:30]

Make a simple offer ...! Edited... Dag 930606

Tom to Martin (in the present):

Neither the model of the control system or the environmental phenomena with which it interacts need be linear. Bill has published and posted on introducing nonlinearity into the PCT model and into the environment. So has Rick. I haven't, but I have tested the effects of nonlinearities in the coordinated systems: The models continued to function at the same level of realism. I will try to put together a post on that topic, in the style of my post a few days ago on adding disturbances to various signals in the control system.

In the meanwhile, I wonder why so many people continue to assert that PCT models are necessarily linear and cannot explain and predict events when there are nonlinearities in the system or the environment. Where do these ideas come from? Why won't they go away? (Dennis Delprato: If you are starting a collection of false assumptions and assertions about PCT, this certainly is one. We should compare collections -- mine goes back a few years.) Everyone who clings to that assumption should read Bill's "spadework" paper in Psych. Review (1978 -- 14 years ago folks) [in LCS I] where he discussed various blunders in the history of cybernetics. That is also where he quantitatively demonstrated the ease with which a PCT model maintains control in the presence of nonlinearities.)

Until later, Tom Bourbon

Date: Mon Dec 21, 1992 8:44 pm PST
Subject: Prediction as Feedforward

[from Gary Cziko 921222.0430 GMT] Dennis Delprato and Bill Powers:

I suppose here's another misunderstanding concerning control systems that we might want to add to our library.

In discussion the application of control system models to understanding "motor control" with a physiological psychologist, he was arguing that the ability to predict meant that feedforward was taking place. The example he used was predicting the movement that an object would take and using that knowledge for tracking (like how it's easier to point to the end of a swinging pendulum than to a fly caught in the shower stall with you).

I don't think I did a very good job at trying to explain how tracking patterns can be seen as controlling a higher-level perceptual variable. Perhaps someone can help could help me out with this. It is also related, I believe, to the discussion between Taylor and Powers concerning what a control systems has to be able to "predict" in order to maintain good control.

--Gary

Date: Tue Jan 05, 1993 5:17 pm PST
Subject: Devil's Advocate

[From Bill Powers (930105.1530)] Greg Williams (920105) --

> For the tracker to "respond" to the "discriminative stimuli," all that is necessary is for him/her to be able to see the cursor movement, NOT to "tell... WHAT THE DISTURBANCE IS." If the cursor is seen to be moving away from the target position -- due to the net COMBINATION of handle position and net disturbance, of course -- then the tracker responds by moving the handle in the direction (determined previously in practice, via "reinforced" learning) which moves the cursor in the direction toward the target position.

How about "the tracker sees an error between the cursor position and its intended position, and responds by moving the handle at a velocity proportional to the amount of the difference and a direction corresponding to the direction of the difference?" This is a verbal description of the organization of the control system.

Your way of putting this assumes that the intended position of the cursor relative to the target is AT the target. It is perfectly possible to move the cursor so it remains a fixed distance to either side of the target. This makes the definition of a discriminative stimulus somewhat difficult, because at that specified distance from the target, most of the time, one can see -- nothing. The stimulus now has to be defined as the distance between the cursor and an arbitrarily-located empty place in space, or alternatively as the distance of the target from that empty place minus the distance of the cursor from that empty place. No matter how you put it, the discriminative stimulus has to be imaginary.

This mistake has been made many times in the past -- the view assumes that some "salient" (meaning obvious-to-me) aspect of the situation is the reference condition, forgetting that this condition is just one point on a scale, and therefore not realizing that control could take place relative to any position on that scale. This is how people have concluded that reference signals come from the environment. That's another myth that got launched in the '50s.

The cursor position and velocity always reflect the ongoing behavior of the disturbance PLUS the ongoing behavior of the handle. If the cursor begins moving slowly to the right, this could indicate that the disturbance has started pushing it to the right a little faster than the handle is pushing it to the left, or that the handle has started pushing it to the left a little slower than the disturbance is pushing it to the right. The information required to make even this qualitative judgment is not contained in the cursor position or velocity. You must perceive your own handle movements directly and estimate how the cursor would be moving and where it would be positioned if your handle were the only influence.

In Demo1 there is a phase in which the difference between compensatory and control behavior is illustrated. In compensatory behavior the "cursor" on the screen shows the disturbance magnitude, not the actual cursor position. The task is to estimate where to put the handle at each instant so that the effect on the now-invisible cursor would keep it from being disturbed. This is impossible on the face of it, so the demonstration shows a trace of what happened to the real cursor during the run (afterward), and you can also alternate with controlling the real cursor so you can pay attention to how your hand moves and learn how much it needs to move and where the center of movement is. By using all this (higher-level feedback) information over many trials, you can actually improve your performance in the compensatory phase quite a lot. You can, with a lot of practice, get the RMS error in the invisible cursor position down to only about 10 times what it is when you can't see the disturbance but can see the cursor.

Even with all this practice, you can't estimate your handle's effect on the cursor well enough to achieve the kind of control you get without having to pay attention to the handle at all and without any direct information about the disturbance magnitude. In the compensatory case you don't have to estimate the amount of disturbance by comparing felt handle position with seen position of the cursor. You are given an exact quantitative picture of the disturbance magnitude. And you still can't achieve the performance of a control system within less than a factor of 10 worse.

Furthermore, when the disturbance is shown on the screen as a pointer AT THE SAME TIME YOU ARE CONTROLLING A VISIBLE CURSOR, your tracking performance is not measurably different in most cases when the disturbance information is eliminated (this is not in Demo1 but I have done the experiment). The only case in which some measurable difference can be seen is when the participant pays attention to the disturbance information and tries to use it to improve control. In that case, the quality of control deteriorates sharply until the person ceases to pay attention to the disturbance information.

All these demonstrations, which I have actually done and which are easily reproducible, show that the person is not making any use of information about the disturbance, either directly when it is available on the screen, or indirectly by estimation of expected handle effects on the cursor.

These facts about control are easily demonstrated, but to understand them you have to think quantitatively. As long as you are allowed to talk about "cursor movements" and "handle movements" and "target movements" you can gloss over these quantitative facts because the language doesn't specify HOW MUCH movement there is in relation to other movements. In a subtle way, you're using the known outcome of the experiment to provide just the meaning for the general terms that is needed to make them fit the observations. It would not be possible to go the other way: if you didn't know how the experiment came out, describing the relationships in terms of "movements" and "positions" and other such qualitative notions wouldn't tell you anything about which way the cursor would move. The cursor movement is a small difference between two variables that are changing over a range of perhaps 20 times as much (RMS comparisons). Just saying that the disturbance moves the cursor to the right while the handle tends to move it to the left leaves the direction of the actual cursor movement undefined.

I tell you: the disturbance is changing to push the cursor to the left while the handle is moving to push it to the right. Which way, pray tell, do you predict that the cursor will be moving?

In your Devils's Advocacy, you have brought out exactly what is wrong with conventional objections to PCT. It is the qualitative nature of the arguments that makes it seem that alternative explanations fit the facts. Qualitative descriptions are crude enough to allow for any outcome at all -- or its opposite. Qualitative explanations, if cleverly enough constructed, are unfalsifiable. This is the attraction of generalization and qualitative description: you can't be wrong.

> One last time: don't take high correlations as THE sign of stimulus-response relationships.

Why not? That's what THEY do, isn't it? This brings out the main reason we can't talk to conventional behavioral scientists. If you show them a tracking experiment, the first thing they will do is look for high correlations: this behavior is a response to that stimulus. When we carefully set up the experiment to show what actually happens to the correlations, what do they do? Do they say "Oh, migosh, it looks as though I have the wrong explanation!"?

In a pig's eye. They immediately back off, and say that this situation is more complex than it appeared, and requires a different explanation. They abandon the simple analysis of simple experimental data and start talking about vague effects of discriminative stimuli and reinforcements, all of which somehow have exactly the effects required to account for the experiment -- for the cursor going up a little here, down a little there, and wiggling just so in between while the handle traces out an almost perfect mirror image of the invisible disturbance.

The real problem is that such people don't have any idea of what a real explanation amounts to. They have given up on science.

Best, Bill P.

Date: Tue Jan 12, 1993 12:12 am PST
Subject: feedback too slow

[Avery.Andrews 930111.1905]

Reading around in the Osherson, ed. 'foundations of cognitive science' (1989) I came across the claim that feedback is too slow to solve inverse kinematic & dynamic problems for fast movements. Where can I read about why this claim is false or irrelevant (e.g., true only for certain kinds of highly skilled movements that people practice enough to make it plausible that they have elaborate feedforward schemes for).

Avery.Andrews@anu.edu.au

Date: Tue Jan 12, 1993 2:48 am PST
Subject: slow feedback reference bungled

[Avery Andrews 9201112.2108]

The reference to the excessive slowness of feedback is actually:

Jordan and Rosenbaum (1989) 'Action', in Posner, ed. Foundations of Cognitive Science, pas. 731, 746. This paper also cites some very suspect-looking work by Kelso and others that is supposed to support a concept of 'coordinative structures' (what seems to be going on is low level configuration/relationship control between articulators).

I guess the obvious question to ask about any movements that do look too fast for feedback is how they go under novel or unpredictable dynamic conditions. E.g. how does the concert pianist make out with lead weights attached to her fingers.

Avery.Andrews@anu.edu.au

Date: Mon Jan 11, 1993 1:10 pm PST
Subject: Who's got the generative model?

[FROM: Dennis Delprato (930111)] Greg (930109) Bill (930108.0800)

I believe the history of science shows that worthwhile new ideas advance when they are contrasted to previous ones that workers find of value. For this reason, I am enthusiastic about Greg Williams's recent attempts to push some basic operant theoretical accounts to the limit vis-a-vis PCT.

- >> But he [Skinner] was fixated on environmental control of behavior, and was forced to conclude that behavior is controlled by its consequences, even though the only CLEAR relationship he could see was that of consequences being controlled by behavior. I have always considered this to be his most intellectually dishonest ploy.
- > You have to remember that he construed current control of consequences by behavior (which he freely admitted) as itself having "come under the control" of the organisms' history -- of consequences in the past.

And through which mechanisms does this history operate? The operant theorist is forced to stay descriptive. Fear of mysterious nonspatiotemporal inner processes leads them to posit no underlying process. Admirable perhaps when all that was available was mysterious. Wide open territory for PCT.

- >> From another standpoint, the behaviorist COULDN'T characterize the experimental setup correctly. To do so would be to see that the stimulus is not an independent variable. The assumption is that the stimulus varies, and as a consequence of that the response varies. To measure the response, one arbitrarily varies the stimulus, so the stimulus has a known value or pattern that is independent of the behavior. If the stimulus is defined so it depends on the response, it's impossible to

perform this manipulation (without breaking any actual feedback loop that's present).

- > I think you are putting words into the mouths of at least some behaviorists (including Skinner), but if you can produce some documentation to support your claim about what they say, I'm ready to be corrected. A while back, I posted some quotes from Skinner which contradict the notion that he thought a stimulus could not be affected by the responding organism. Note that even in a feedback situation, the stimulus can be manipulated to a degree, because control isn't perfect.

The trouble may be that they stop with a description of the experimental set up (i.e., procedure). They take the procedure (e.g., S-dee-Response-Reinforcer, coupled with deprivation or the like) sufficient for explanation. Incorporation of a control system account requires that one go beyond the obvious details of the procedure, including history of reinforcement and verbal statements that "feedback is involved."

- >> Skinner saw the reinforcer as a consequence of behavior. But being unable to give up the idea that the environment controls, he then treated this consequence as an independent variable, and said that it controls the behavior. To be sure it controls only FUTURE behavior, but with his blind spot he never saw the obvious implication: that the BEHAVIOR which produces this consequence controls ("controls" meaning influences) the future behavior via the apparatus. To see this loop whole would have meant giving up the concept that the environment determines behavior, and that, above all, he was unwilling to do.
- > Seeing the whole loop, with BOTH environmental and organismic influences on output, is EXACTLY the middle way between environmentalism and organismism which I was arguing for some time back.

Bill is right. Skinner always came back to the environment as the ultimate independent variable. Although he and many followers have verbally stated that environment determines response and response determines environment (stimulus), in practice, this has amounted to nothing. True, some followers (e.g., Baum) have gone a bit farther than did Skinner, but Bill can tell us about these.

- >> No, the behaviorist will yell "Foul!" when I point out that the so-called stimulus is not an independent variable.
- > That's what I'm not so sure about. I wish we had a real behaviorist on the net. Maybe Dennis could speak to this issue?

Some behavior analysts are getting away from stimulus as true independent variable and response as true dependent variable. BUT, as with above comment, they do not go anywhere with this thinking. Close but so far.

In my opinion, the major stumbling block for behavior analysts as far as PCT is the requirement to take S-R and R-S as SIMULTANEOUS. This is very difficult for two basic reasons: (1) our culture teaches lineal thinking and (2) the damn procedure (itself a product of lineal biases) is lineal (ess D then R then reinforcer). The only way to get S-R and R-S simultaneous is via theory, then this poses the added difficulty of changing the status of the response consequence.

Hope I've said something to help Greg. My perspective suggests there is a great opportunity for one to prepare an interesting paper entitled something like "From Feedback Functions to Perceptual Control Systems."

Dennis Delprato

Date: Tue Jan 12, 1993 9:13 am PST
Subject: slow feedback

[From Rick Marken (930112.0800)] Avery.Andrews (930111.1905) --

- > I came across the claim that feedback is too slow to solve inverse kinematic & dynamic problems for fast movements. Where can I read about why this claim is false or irrelevant

I think Bill Powers posted a wonderful exposition on the "feedback is too slow" nonsense. I don't have it available but maybe someone could re-post it; it was really excellent. The answer to the above claim is simply "how do you know that people solve inverse kinematics to make movements of ANY kind -- slow or fast; maybe they just control their perception"?

- > This paper also cites some very suspect-looking work by Kelso and others that is supposed to support a concept of 'coordinative structures'

Welcome to the wonderful world of high powered motor control nonsense. Two papers in "Mind readings" are attempts to show, experimentally, that the leap into 'coordinative structure' models may have been a bit precipitous.

- > I guess the obvious question to ask about any movements that do look too fast for feedback is how they go under novel or unpredictable dynamic conditions. E.g. how does the concert pianist make out with lead weights attached to her fingers.

You've got it! The question has been asked, and answered, by PCT research and models, many times. But no one pays attention because they already KNOW that PCT can't work (because feedback is too slow). If you want to read more amusing statements about feedback being "too slow" made by authoritative leaders in the study of human movement control, try the article by Abbs and Winstein in M. Jeannerod (ED) Attention and Performance XIII, Hillsdale, Erlbaum, 1990 (I just pulled this off the shelf and found the reference by looking in the index under "feedback"). I find that the latest collection of papers on motor control is as good for a laugh (or better) than a Robert Benchley collection. The best thing about it is that these people (the motor controllers; not Benchley) are SERIOUS.

Best Rick

Date: Tue Jan 12, 1993 11:03 am PST
Subject: S-R & PCT

[From Bill Powers (930112.0900)] Greg Williams (930112) --

- > ... where does a model for tracking which uses C, H, and T as its variables fit? And where does a model of gravitational attraction which uses distance and mass (observationally proportional to weight) fit?

C, H, and T (and D) are the observable variables whose behavior we must explain, just as position, velocity, and acceleration are the observable variables which Newton had to explain. The explanation goes beyond the observable variables, in proposing entities like mass, or error sensitivity.

Mass, remember, is not an observable variable (weight is not the same thing as mass), and neither is the universal constant of gravitation, nor the inverse-square relationship of force (acceleration for a free body) and distance. A pound of feathers and a pound of lead do NOT fall at the same rate. The attraction between nonspherical objects does NOT go as the inverse square of the distance between their centers. A cannonball does NOT travel in an ellipse with one focus at the center of the Earth. The observations deny Newton's laws. Newton replies, "Yes, but the underlying relationships are as I suppose. If you calculate viscous friction, and integrate using my universal law over all the infinitesimal particles of irregular objects, however imaginary those particles may be, you will see that the laws predict exactly what we observe."

Suppose we have a simple control system with a loop gain of 1,000,000 and a slowing factor in the output function that is sufficient for stable operation. In this case, the true steady-state relationship between C and H is $H = 1,000,000 * (C^* - C)$. This is not, of course, what we observe. We observe that $H = -D$ and $C = C^*$, as near as we can measure, give or take random noise and measurement error. If there are variations in C^* we will see C varying in the

same way, but H will not vary a million times as much. H will vary only as much as needed to cancel the effect of D on $(C^* - C)$. We do NOT, in general, see H varying a million times as much as C. Yet a generative model in which the error sensitivity is one million explains the observations.

> Still, I think Skinner's "prematurity" warning still counts for something...

Skinner was denying the usefulness of models of the interior of the organism at exactly the same time the principles of control theory were being developed. He defended his views against cybernetics and cognitive models as against any other proposals. He took the deviations of others' views from his own as prima facie evidence that the other views were wrong: no proof needed. Skinner's main modes of argument were ridicule and assertion. He did not test hypotheses. He simply offered positive instances worded so as to support his position.

Suppose that Skinner had really believed, as he seemed to claim, that models of the inner working of organisms might some day provide explanatory principles not present in radical behaviorism. In that case, all his explanations of behavior in terms of external events and situations should have been appended with "... or some cause working from inside the organism." Obviously there was no such appendage: it would have made his bold assertions look foolish. What Skinner believed, as far as I can see, must have been what many cognitive scientists believe today: that if you followed all more abstract explanations down to their fundamental bases, the causes would eventually trace back to the environment. In other words, Skinner considered that he was only stating in an approximate way what would some day be shown to be the only accurate way. This was his lifelong faith.

> Actually, from what I've read, they actually claim that they DON'T WANT TO COME UP WITH AN EXPLANATION -- ONLY PREDICTION/CONTROL. But the upshot is as you say, of course, and they can't get as close to their professed goal as they could with PCT models (which, as noted yet again above, might be very difficult to generate for complex situations).

The trouble with qualitative language is that you don't get any idea of proportions. To say that they can't get as close to prediction as PCT can leave the image of a footrace with PCT winning in a final burst at the finish line. With respect to prediction, PCT is crossing the finish line while the operant approach still has one foot in the starting block.

Do you realize that there is no basis in the operant-conditioning model for predicting that there will be any behavior at all in a Skinner box? And that even if you admit as a prediction an extrapolation from previous experience, this model can't predict how much behavior there will be, if any? The reinforcement rate supposedly sustains the behavior rate. But the reinforcement rate depends on behavior, so unless you know in advance what the behavior rate is going to be, you can't say what the reinforcement rate will be. Not being able to predict the reinforcement rate, you have no basis for predicting any particular behavior rate. So there is NO PREDICTION AT ALL.

The best that the operant approach can do is to describe what has already happened, and predict that what has happened will happen again. All of the mathematical manipulations I have seen in the operant literature have been manipulations of algebraic identities; with only one equation to represent a situation requiring two or more equations for its complete description, that is all that can be done. It is not that the operant model predicts less well than PCT. It does not predict AT ALL.

> Skinner's extreme historical environmentalism and an extreme "moment-by-moment" mechanistic organismism need melding into a broader -- and I think truer -- picture.

As Dennis pointed out, "history" is not a causal mechanism. The past can't affect the present any more than the future can. Everything that operates on behavior is present now, or else it has no effect. The only way for the past to seem to operate in the present is through memory; and it's only the current

contents of memory, not what actually happened in the past, that has an effect NOW.

This is a fundamental principle of all the hard sciences. The history of a variable is irrelevant; the path by which it got to its present state (including derivatives) is of no consequence. Generative models work strictly in present time. I don't see any possibility for a merger here.

Avery.Andrews (930111.1905) --

> Reading around in the Osherson, ed. 'foundations of cognitive science' (1989) I came across the claim that feedback is too slow to solve inverse kinematic & dynamic problems for fast movements.

This is a good one for the collection of myths. In fact this is a double whammy: a myth based on a myth. In the first place, it's not necessary to solve for the inverse kinematics because a control system automatically does that using the environment as its own model. So it's true that control systems aren't fast enough to solve the inverse kinematics without feedback: no neural system is fast enough. But solving the inverse kinematic problem isn't necessary in any case, and control systems are certainly fast enough to do what is necessary to control a dynamical system.

> Where can I read about why this claim is false or irrelevant (e.g., true only for certain kinds of highly skilled movements that people practice enough to make it plausible that they have elaborate feedforward schemes for).

Look up analogue computer methods for solving second-order differential equations. Korn and Korn is the only reference in my head, and it's probably way out of date (Greg?). See also my arm model, which controls a dynamical system without solving the inverse kinematic or dynamic equations.

I know of no situation in which literally solving the inverse kinematic and dynamic equations is a plausible explanation for behavior.

Best to all, Bill P.

Date: Wed Jan 13, 1993 7:51 pm PST
Subject: Feedback is too slow

[FROM Dennis Delprato (930113)]

Allegations that movements occur too rapidly for feedback to play a role represent one of the major arguments against feedback control theories. The central-peripheral issue goes back to the 19th century, and peripheral control was tied to "response-produced feedback" -- usually as a stimulus (!) for the next response. Thus, control theory as we know it never really came up. Nonetheless, feedback control theory of the modern type (I acknowledge the major contributions of K. U. Smith and W. T. Powers for this) has been saddled with the near-ancient arguments used against its very distant relative.

From S. W. Keele (Attention and Human Performance, Pacific Palisades, CA: Goodyear Publishing Co., 1973), it appears that data interpreted in support of the allegation go as far back as Woodworth (1899, The accuracy of voluntary movement, Psychol. Rev.). Lashley (1917, The accuracy of movement in the absence of excitation from the moving organ, Amer. J. Physiol., v 43, 169-194) also cites Woodworth (1899). In this 1917 paper (well in advance of his much-cited 1951 "The Problem of Serial Order" chapter) Lashley brings up the musician.

I found it interesting to be reminded of just how influential the set construct was. This is especially the case insofar as the set construct is simply an earlier version of today's notions of cognitive control, motor programs, schema, et al. The latter tell us not a bit more than did the idea of set or determining tendency. Such is progress.

As far as feedback control theoretical arguments against the feedback is too slow position, I have been able to find this addressed by T. J. Smith and K. U. Smith. This is in a preprint of a chapter entitled "Feedback-Control Mechanisms of Human Behavior" prepared for the Handbook of Human Factors/Ergonomics. I received the preprint approx. 1987; thus, the book must be published by now. In the preprint, the authors address our topic in Section 8.6.3.3 (Control Theories of Fast Movement Coordinations).

They point out that it is virtually impossible to test the alternative to feedback control -- central brain programming-- and that feedback control has been much evaluated, including with musicians. They find that feedback delay has major debilitating effects on performance to the point that the highly - skilled musicians they used invariably refused to continue with the process after brief exposures fearing permanent impairments. One of their main reactions to the argument of feedback being too slow to play a role in skilled movements is that if this is the case, delay and other distortions of feedback (e.g., displaced visual feedback) should not impair performance. But data from a wide variety of performances (musical, drawing, writing, and many more) clearly show increased impairments with increases in experimentally-manipulated distortions and delays of feedback.

They also bring up considerations that, to me, require elaboration to be convincing: what they call predictive control and body movement tracking.

Date: Wed Jan 13, 1993 10:26 pm PST
Subject: feedback too slow

[Avery Andrews 930114.1652] (Dennis Delprato (930113))

Cool. Smith & Smith have a useful bad guy list, tho with nothing after 1981, so it would still be good to know what Schmidt 1982 has to say.

The Smith & Smith article ref. is:

Smith, T.J. and K.U. Smith (1987) 'Feedback and Control Mechanisms of Human Behavior', in Gabriel Salvendy (ed) Handbook of Human Factors, Wiley, pp. 251-293, esp. 266-268.

Looking it over quickly, a possible story is this: given the speed of piano-playing movements, it does seem plausible to me that a motor program is necessary for this. But necessary does not imply sufficient, & what the Smith & Smith discussion (of the effects of delayed feedback) shows is that motor programs are not sufficient. Maybe it's time for Sesame Street to include some some vs. all drills alongside of in vs. on, etc.

And these sorts of movements are really lousy guides to the nature of routine activity, since they are a form of behavior which nobody can achieve without an inordinate amount of practice, and many can perhaps not achieve with any amount of practice.

Avery.Andrews@anu.edu.au

Date: Tue Jan 12, 1993 5:08 pm PST
Subject: feedback to slow

[Avery Andrews 930112.1133] Bill Powers (930112.0900)

- >> Where can I read about why this claim [feedback too slow] is false or irrelevant (e.g., true only for certain kinds of highly skilled movements that people practice enough to make it plausible that they have elaborate feedforward schemes for).
- > Look up analogue computer methods for solving second-order differential equations. Korn and Korn is the only reference in my head, and it's probably way out of date (Greg?). See also my arm model, which controls a dynamical system without solving the inverse kinematic or dynamic equations.

This sort of reading isn't enough to make an impression on the establishment. Academics are usually too busy, & maybe in some cases, too stupid, to actively extract the relevant implications from the stuff they read--everything has to be spelled out in ghastly, explicit detail, not left to lie in math books, or buried in CSG-L archives or C code. Students have more time, but they also can't expected to have very broad backgrounds, so again, the needed bits and pieces have to be laid out and put together slowly and carefully.

It needs to be documented in full

- a) who first made the too slow claim, and what they were talking about when and why.
- b) wherein what was said is factually wrong.
- c) wherein it is irrelevant to the general case.

Could something like this be published in Biological Cybernetics?

Remember that one the most important aspects of academic streetfighting is to make the opposition look stupid (as Chomsky did to Skinner with his review of Verbal Behavior), but to do this effectively you have to lay out the details in a way to be clearly accessible to prospective students, and practitioners of neighboring disciplines.

Avery.Andrews@anu.edu.au

Date: Thu Jan 14, 1993 10:09 am PST
Subject: Re: feedback to slow

[Martin Taylor 930114 12:00] (Avery Andrews 930112.1133)

>It needs to be documented in full

- >a) who first made the too slow claim, and what they were talking about when and why.
- >b) wherein what was said is factually wrong.
- >c) wherein it is irrelevant to the general case.

> Could something like this be published in Biological Cybernetics?

I doubt it could be published. Who made the claim was presumably not an electronics person. Early in EE you get taught (or did a few decades ago) about a concept called Gain-Bandwidth Product. With positive feedback the apparent gain goes up and the bandwidth (and normally the delay) goes down. With negative feedback the reverse should be expected to happen. At least that's my very vague and distant memory, and what feeds my intuition about this issue.

Maybe my memory is all screwed up, but if not, there's nothing to publish except a reference to standard textbooks.

Martin

Date: Thu Jan 14, 1993 4:50 pm PST
Subject: RE: Feedback is too slow

[From Bill Powers (920114.1400)] [Martin Taylor 930114 12:00]

> Who made the claim was presumably not an electronics person.

Too right. Much of this confusion can be traced back to early cyberneticians, almost none of whom had engineering knowledge of control systems. Psychologists tended to treat anyone with the label cyberneticist as a technonerd, so when they decided to believe something a cyberneticist wrote

they invested it with full authority. That's how a lot of myths about control theory got into circulation.

Best, Bill P.

Date: Thu Jan 14, 1993 6:10 pm PST
Subject: Feedback too slow

[FROM: Dennis Delprato (930114)] >Avery Andrews 930114.1652

> Cool. Smith & Smith have a useful bad guy list, tho with nothing after 1981, so it would still be good to know what Schmidt 1982 has to say.

Does this mean you don't have access to R. A. Schmidt's Motor Control and Learning, referred to by Jordan & Rosenbaum (in Posner, Ed., Foundations of Cog. Sci.? I can probably find a copy in a few days and promise to try if this might help get you to write a paper on this issue. You mentioned Biological Cybernetics as an outlet. Given that this is one of the oldest (mis?) conceptions in psychology, I imagine that a thorough, well-documented and well-reasoned paper would be given some consideration by several editors. Hope you get a chance to see what Woodworth (1899) did and concluded -- and let us know.

> The Smith & Smith article ref. is:

> Smith, T.J. and K.U. Smith (1987) 'Feedback and Control Mechanisms of Human Behavior', in Gabriel Salvendy (ed) Handbook of Human Factors, Wiley, pp. 251-293, esp. 266-268.

> Looking it over quickly, a possible story is this: given the speed of piano-playing movements, it does seem plausible to me that a motor program is necessary for this. But necessary does not imply sufficient, & what the Smith & Smith discussion (of the effects of delayed feedback) shows is that motor programs are not sufficient. Maybe it's time for Sesame Street to include some some vs. all drills alongside of in vs. on, etc.

I am not an authority in the motor skills area, but it seems to me that theorists have overlooked the possibility that what's happened with the development of very rapid movements is that larger and larger physical/physiological units have become functional psychological response units or patterns. Thus, the psychologist need not explain control of the units themselves -- I am quite certain physiologist Tom Smith would hold that physiological data indicate nothing but feedback control processes at the physiological level. Level of description/explanation is a relevant consideration always. From a psychological level, what appears ballistic is not so when described from the physiological perspective. Up to the present, when researchers have observed response patterns, they have assumed that the patterns needed explanation from a psychological perspective, hence suggestions of mechanistic chaining (behaviorists) or mental organization and so on (cognitivists). Neither side considered that the patterns (organizations of physical/ physiological events) were simply givens, with the only needed explanation being their development (principles of how re-organization takes place). Note, for example, how Lashley (1951) argues for "motor patterns" that "require the postulation of some central nervous mechanism which fires with some predetermined..." (p. 123). So they might. But would this be equivalent to "cognitive control?" This is the common reasoning.

R. W. Pew (1974, chapter in B. H. Kantowitz's Human Information Processing: Tutorials in Performance and Cognition) seems to take a move in the direction I suggest. To a lesser extent, also, perhaps does Glencross (1977).

> And these sorts of movements are really lousy guides to the nature of routine activity, since they are a form of behavior which nobody can achieve without an inordinate amount of practice, and many can perhaps not achieve with any amount of practice.

I agree. What about all the awful musicians and typists? Nevertheless, these movements are found and must be explained.

Dennis Delprato psy_delprato@emunix.emich.edu

Date: Thu Jan 14, 1993 6:45 pm PST
Subject: feedback too slow; psychologists & cybernetics

[Avery Andrews 920104.1339] Dennis Delprato (930114)

- >> Cool. Smith & Smith have a useful bad guy list, tho with nothing after 1981, so it would still be good to know what Schmidt 1982 has to say.
- > Does this mean you don't have access to R. A. Schmidt's Motor Control and Learning, referred to by Jordan & Rosenbaum (in Posner, Ed., Foundations of Cog. Sci.? I can probably find a

Yes. This would be very helpful. I can't promise a good product real soon, since I'm feeling overextended & have to do some linguistics soon, but I do promise to try to try, as it were.

I agree entirely that levels is the key here. Presumably what there is in piano playing is systems that set a series of reference levels in sequence, without waiting for evidence that one has been achieved in order to set the next, with the details in charge of lower-level systems (are the spinal reflex loops fast enough to be useful in controlling piano playing, or are their effects actually disturbances that have to be pre-compensated for?). But many higher-level aspects of the performance then require perception of what has happened already (I'm a firm believer in the existence and importance of response-chaining in everyday life).

In K. VanLehn's 'Problem Solving and Cognitive Skill Acquisition' in the Posner volume, p. 555, there's an interesting statement about automatization of skills:

If exactly the same task is practiced for hundreds of trials, it can be automatized, that is, it will be very rapid, cease to interfere with concurrent tasks, and run to completion once started even if the subject tries to stop it. If the task varies beyond certain limits during training, however, even hundreds of practice trials to not suffice for automatization (Schneider and Shiffrin 1977, Schiffrrin and Sneider (1977)).

the citations are:

- 1) Controlled and automatic human information processing:I. Detection, search and attention. Psychological Review 84:1-66.
- 2) Controlled and automatic human information processing:II. Perceptual learning, automatic attending, and a general theory. Psych. Rev. 84:127-190

(Lest people get false ideas about my ability to cover literature, the book literally just fell open at that page, so I read the paragraph at the top. Pennies from Heaven.)

So my story would be that there are automatic routines, which are presumably normally very short, but with extensive practice can be made larger. And may be the skill acquisition literature can provide some actual evidence about what kinds of disturbances these lower level units can handle (assuming that that's what 'varying the task' involves).

Bill Powers (930114), psychologists & cyberneticians

I found some likely-looking references to feedback bungles in Quantitative Analysis of Purposive Behavior, but who actually commits the Input Error?

Avery.Andrews@anu.edu.au

Date: Fri Jan 15, 1993 9:31 am PST
Subject: Re: feedback too slow

[Chris Malcolm] Avery Andrews writes:

> I came across the claim that feedback is too slow to solve inverse kinematic & dynamic problems for fast movements. Where can I read about why this claim is false or irrelevant?

It's true for assembly (arm-hand type) robots. See any intro robotics textbook. One of the reasons why assembly robots can't compete with human assembly speeds is that solving the inverse kinematics at the speed of fast human arm movements is just on the brink of being too expensive computationally. Inverse dynamics is orders of magnitude worse. But of course we are talking here about algorithmic solutions based on mathematical models, and that is not the only way of solving these things (e.g. Churchland's crab). In the end you come up against imagination failure, i.e., "we can't think of a way of doing this, so we think it is probably impossible."

Date: Mon Jan 18, 1993 9:03 am PST
Subject: Devil's Bibliography

[From Rick Marken (930118.0800)]

Here are some entries for the "Devil's Bibliography"

1. Sheridan, T. B. and Ferrell, W.R. (1974) Man-machine systems, Cambridge, MIT Press

Figure 9.1 Shows "reference input variable" as a variable outside the human operator where it is combined with an output variable to produce the input to the subject, $e(t)$, the "error variable" which causes (via the operator function $Y(H)$) the "control variable" that influences the output of the "plant".

Figure 11.10 (on quasi-linear model of a car driver) shows the "desired path" (of the car) as an INPUT to the driver.

There are probably more but I'm going to look for a newer source. Ah, here's one:

2. Huchingson, R. D. (1981) New horizons for human factors in design, New York: McGraw Hill

Figure 5.15 (p 181) shows a "command" signal being combined with system output to produce the display variable that is the input to the "man" component of the system. The "man" box converts the display input into an output that is the "input" to the machine that produces the output that ultimately becomes the display input.

Getting even more recent (and with no obvious signs of anyone catching on) we find:

3. Boff, K.R., Kaufman, L and Thomas, J.P (1986) Handbook of perception and human performance: vol.II, Cognitive processes and performance. New York: Wiley

Chapter 39 (by C. Wickens) is chock full of the "input blunder" and "man machine blunder". Particularly clear examples are:

Figure 39.1 which shows the error signal ($e(t)$) as the input to the human operator.

Figure 39.17 which shows the "optimal control model" as a sequence of transformations of input yielding a "control" output; the secular contribution of the operator is observation and motor noise -- quite an operator, though not completely unlike some people I've met.

Figure 39.39 shows an input being turned into a "displayed error" and presented to the operator who turns it into the output variable.

Finally, the latest source that was nearby:

4. Weiner, E. L. and Nagel, D. C (1988) Human factors in aviation. New York. Academic Press

In Chapter 11, "Pilot control" by Sheldon Baron we find Figure 11.4 which shows altitude and pitch signals as inputs to the pilot "box" which transforms these inputs, via the function $Y(P)$, into outputs that enter the vehicle's machinery.

I could probably find some articles from the 1990s by searching through recent issues of journals like "Human Factors" but I like the stuff in these "bibles" of "human performance" knowledge the best.

Best regards Rick

Date: Mon Jan 18, 1993 9:24 pm PST
Subject: Re: Devil's Bibliography

[From Bill Powers (930118.1600)] Rick Marken (930118.0800) --

You spur me to add a few more entries to the "Devil's Bibliography."

Here is Warren S. McCulloch helping to form the myth that feedback systems go unstable when their gain exceeds unity:

McCulloch, W. S.; Finality and form; in Embodiments of Mind, (Cambridge, MIT Press, 1965) pp. 256-275.

When we change the magnitude of the quantity measured, a reflex may return the system toward, but not quite to, the original state, or it may overshoot that state. The ratio of the return to the change that occasioned it is called the gain around the circuit. When the return is equal to the change that occasioned it, then the gain is one....if the gain is greater than one at the natural frequency of the reflex, fluctuations at that frequency begin and grow until the limitations of the structure composing the path reduce the gain to one; then, at the level for which the gain has become one, both the measured quantity and the reflex activity go on fluctuating." (p. 267)

Even earlier than this confident mangling of closed-loop properties, we have this:

McCulloch, W. S., Appendix I: Summary of the points of agreement reached in the previous nine conferences on cybernetics. Cybernetics: circular causal and feedback mechanisms in biological and social systems. Transactions of the Tenth Conference, April 22,23 and 24, 1953, Princeton, NJ. Josiah Macy, Jr. Foundation, 1955. LCN 51-33199.

The transmission of signals requires time, and gain depends on frequency; consequently, circuits inverse for some frequencies may be regenerative for others. All become regenerative when gain exceeds one. Regeneration leads to extreme deviation or to schizogenic oscillation..." (p71)

This rules out negative feedback systems with loop gains higher than unity -- in other words, all actual control systems that exist in organisms.

On explaining how control works:

Wiener drew a most illuminating comparison between the cerebellum and the control devices of gun turrets, modern winches, and cranes. The function of the cerebellum and the control of those machines is, in each case, to precompute the orders necessary for servomechanisms, and to bring to rest, at a preassigned position, a mass that has been put into motion

which otherwise, for inertial reasons, would fall short of, or overshoot, the mark." (p. 72)

Here we have the germ of the compute-then-execute approach.

There is a certain kind of intellect which can, on hearing the merest summary of an idea, immediately leap ahead to its most profound implications and applications, completely unaware, or unconcerned, that it has a superficial and mostly incorrect understanding of the idea.

Here's another gaggle of myths, this time from W. Ross Ashby, in An Introduction to Cybernetics (New York: Wiley, 1966 (third printing, copyright 1963)).

The basic formulation of s.11/4 assumed that the process of regulation went through its successive stages in the following order:

1. A particular disturbance threatens at D;
2. it acts on R, which transforms it to a response;
3. the two values, of D and R, act on T simultaneously to produce T's outcome;
4. the outcome is a state of E, or affects E." (p. 221)

E is an essential variable that is to be stabilized by the action of a regulator R, acting through an environmental function T which is fixed. Regulation is achieved when the effect of D on T is precisely cancelled by the response of the regulator R to D, also acting on T. It is assumed that E depends on T and T alone, so there are no disturbances acting directly on E that can't be sensed by the regulator.

If R and T are precisely calibrated and act with infinite precision, then perfect regulation is possible -- but not otherwise. Ashby tended to overlook the question of precision, largely because in examples he tended to use small integers or decimal fractions accurate to one decimal place to represent the variables. As a result he greatly overestimated the capacities of compensating systems, and therefore, by comparison, greatly underestimated the capacities of control systems.

Regulation by error. A well-known regulator that cannot react directly to the original disturbance D is the thermostat- controlled water bath, which is unable to say "I see someone coming with a cold flask that is to be immersed in me -- I must act now." On the contrary, the regulator gets no information about the disturbance until the temperature of the water (E) actually begins to drop. And the same limitation applies to the other possible disturbances, such as the approach of a patch of sunlight that will warm it, or the leaving open of a door that will bring a draught to cool it." (p. 222).

Note the implication that a compensating regulator might exist which, on seeing someone approach with a flask, could deduce that it contains cold water and is about to be immersed in the bath. Note also the unspoken assumption that merely from qualitative knowledge about a flask of cold water, a patch of sunlight, or a potential draught through an open door, the regulator could be prepared to act quantitatively: to add heat to the bath that would exactly compensate for the cooling from the water in the flask or the evaporation due to the draught, or cooling just sufficient to prevent any rise in the temperature of the bath. From qualitative knowledge of the disturbance, the regulator somehow achieves exact quantitative compensation for the quantitative effects of the disturbance. If, of course, such a thing were possible, the compensator would be much superior to any form of feedback controller. But such a thing is not remotely possible.

After doing through a series of diagrams, Ashby finally diagrams the true error-driven control system:

... we have the basic form of the simple 'error-controlled servomechanism' or 'closed-loop regulator,' with its well-known feedback from E to R." (p. 223)

The diagram is

$$\begin{array}{ccccc}
 D & \rightarrow & T & \rightarrow & E \\
 & & \hat{} & & | \\
 & & | & & | \\
 & & R & \leftarrow & -
 \end{array}$$

Now we get to a whole fountain of misinformation about control systems, a series of deductions that is just close enough to reality to be convincing, and just far enough from it to be utter nonsense.

A fundamental property of the error-controlled regulator is that it cannot be perfect in the sense of S.11/3" (p.223)

He then goes through a "formal proof" using the Law of Requisite Variety to conclude

It is easily shown that with these conditions E's variety will be as large as D's -- i.e., R can achieve no regulation, no matter how R is constructed (i.e., no matter what transformation is used to turn E's value into an R-value)."

If the formal proof is not required, a simpler line of reasoning can show why this must be so. As we saw, R gets its information through T and E. Suppose that R is regulating successfully, then this would imply that the variety of E is reduced below that of D -- perhaps even to zero. This very reduction makes the channel

$$D \rightarrow T \rightarrow E$$

to have a lessened capacity; if E should be held quite constant then the channel is quite blocked. So the more successful R is in keeping E constant, the more does R block the channel by which it is receiving its necessary information. Clearly, any success by R can at best be partial." (p. 223-224)

This argument has apparently convinced many cyberneticists and others that the Law of Requisite Variety is more general than the principles of control, and in fact shows that control systems are poor second cousins to compensators when it comes to the ability to maintain essential variables constant against disturbance.

In fact this argument shows how utterly useless the Law of Requisite Variety is for reaching any correct conclusion about control systems.

Having swept through this dizzying exercise in proving a falsehood, Ashby then grudgingly allows feedback control to creep humbly back into the picture:

Fortunately, in many cases complete regulation is not necessary. So far, we have rather assumed that the states of the essential variables E were sharply divided into "normal" ... and "lethal", so occurrence of the "undesirable" states was wholly incompatible with regulation. It often happens, however, that the system shows continuity, so that the states of the essential variables lie a long a scale of undesirability. Thus a land animal can pass through many degrees of dehydration before dying of thirst; and a suitable reverse from half way along the scale may justly be called "regulatory" if it saves the animal's life, though it may not have saved the animal from discomfort. "

Note the gratuitous "half way along the scale."

Thus the presence of continuity makes possible a regulation that, though not perfect, is of the greatest practical importance. Small errors are allowed to occur; then, by giving their information to R, they make possible a regulation against great errors. This is the basic theory, in terms of communication, of the simple feedback regulator." (p. 224)

The argument then veers off into "Markovian machines" and Markovian -- stochastic -- regulation. This is billed as the most important and far-reaching application of the error-controlled regulator.

Note how the argument relies on qualitative statements to reach quantitative conclusions. It is perfectly true that if a compensating regulator affects T equally and oppositely to the effect of D, E will not be affected at all. But by that same argument, to the extent that R does not have perfect information about D (and about the nature of the connection from D to T and from R to T), T will not be affected equally and oppositely, and thus to the extent of the imperfection, E will not be perfectly regulated. Furthermore, if there is any disturbance at all that is NOT detected by R (for example, a disturbance that acts directly on E), the effects of that disturbance will not be compensated at all. If R does not compensate for all nonlinearities and time-functions in the connection from D to T, compensation will not occur. When the processes involved are thought of as real physical processes in a real environment, the idealized assumptions behind the compensatory regulator are easily seen to be unrealistic -- they predict regulation that is far, far better than any that could actually be achieved in this way.

Note also how the qualitative concept that error-regulated control must be imperfect is used to imply that it must be more imperfect than compensatory regulation. This non sequitur has appeared in the literature over and over ever since Ashby. In his earlier book Ashby was still toying with true feedback control and continuous systems; the appendix is heaped with rather aimless mathematics that is oriented in that direction. But in this second book, Ashby shows that he never understood how an "error-controlled" regulator works. He didn't know that the "imperfection" inherent in such systems can be reduced to levels of error far smaller than the error-reductions that any real compensating system could achieve -- smaller by orders of magnitude, in many cases, particularly cases involving human behavioral systems.

Ashby's entire line of reasoning about feedback control in An introduction to cybernetics is spurious. Yet Ashby has been revered in cybernetics and associated fields for 40 years as a deep thinker and a pioneer. His Law of Requisite Variety has nothing at all useful to say about control systems -- and in fact led Ashby to a completely false conclusion about them -- yet it is still cited as a piece of fundamental thinking. Whether Ashby originated these misconceptions or simply picked them up from others I don't know. One thing is certain: he did not get them from an understanding of the principles of control.

Here's a little test.

I have 200 pounds of ice cubes, and you have 50 gallons of boiling water. Desired: a nice tub of water for a bath. I get to throw in the ice cubes (you can see exactly how many I throw in); you get to pour in the boiling water. As you see me disturbing the bath with ice-cubes, you estimate how much boiling water to pour in to arrive at a bath of the right temperature. When I have exhausted my ice cubes, you finish the process by adding more boiling water in the amount you think is necessary.

As an alternative, I will let you see a thermometer in the tub, but will not let you see how many ice cubes I am throwing in. You must base your additions of boiling water entirely on the thermometer reading.

Whichever method of filling the tub you elect, when the process is finished you must then step into the tub and immediately sit down in it.

Which method would you choose: compensating for known disturbances, or basing your action on perception of the state of the essential variable without knowing what the disturbances are?

Best, Bill P.

Date: Mon Jan 18, 1993 10:14 pm PST
Subject: Re: Devil's Bibliography

[Avery.Andrews 930119.1700]

Who shall be responsible for looking after it? I'll try to integrate, but all contributors should keep ahold of their own stuff.

My feeling is that there should be a bad guy list (McCulloch, Arbib, at least on the basis of his Hand. of Phys. diagrams (when I reread the accompanying discussion, it started looking even worse, since he was mangling concepts presented quite clearly and coherently by Houk and Rymer in their Handbook article), for real trash by famous people, and a good guy list, for stuff that is basically sensible (Camhi, Houk & Rymer from what I've seen so far), although arguably hindered by a bad heritage.

Hmm. I think I remember trying to make sense out of the Ashby book when I was in school ...

Avery.Andrews@anu.edu.au

Date: Tue Jan 19, 1993 12:24 am PST
Subject: Devils Bibliography

[Avery Andrews 930119.1919] Here's one from me:

Arbib, M. (1981) 'Perceptual Structures and Distributed Control', in Handbook of Physiology, Motor Control II.

Figure 11 on pg. 1446 is supposed to represent a 'feedforward' system operating alternately with a 'feedback' system, where the 'feedforward' system operates to correct large errors, and the 'feedback' to correct small ones. But it is clear that the 'feedback' system is just a 'gross' feedback system - one with low gain, but high efficacy to correct crude errors.

Figure 12 (pg. 1447) is supposed to represent 'coactivated' systems, where the 'feedforward' system injects signals concurrently with the 'feedback' one. But this is also wrong: the 'feedforward' system has only one input line (from 'desired output' = reference signal), and since it knows nothing about what is, it can't achieve any useful effect. It needs (at least) another input line, representing info environment from which undesirable influences on the output can be predicted.

The accompanying text does not improve things. On pg. 1445 feedforward is described by paraphrasing a (clear and cogent) exposition by Houk and Rymer (1981), who describe feedforward as monitoring the environment for factors that will create predicatable disturbances to the controlled quantity, and injecting appropriate signals to counter the deviations before they actually happen. Later we get:

"Here we extend the sense of feedforward to include a strategy that generates control signals to rapidly bridge large discrepancies in desired output at too great a velocity for long-latency feedback paths to have any effect." which seems confused. Then alpha-gamma efferent coactivation is presented as an example of the coactivation model of Fig 12, which seems pretty dubious (a non-dubious one, using the Houk & Rymer concept of feedforward, would be turning the steering-wheel in response to perceived curvature in the upcoming road, before there is any visible drift of the car from a desirable position on the road).

Date: Tue Jan 19, 1993 12:27 am PST
Subject: feedback too slow

[Avery Andrews 930119.1922]

Maybe another contributing factor to the 'feedback too slow' story is the work by Rack and others (who seem to know what they're doing) arguing that the spinal reflexes are too slow and have too little gain to be much use in compensating for short-term disturbances.

Hopefully this doesn't imply that they're not useful for dynamics - it is perhaps significant that amphibians, who lack the dual alpha-gamma efferent system, are not noted for their acrobatic abilities.

Avery.Andrews@anu.edu.au

Date: Tue Jan 19, 1993 4:58 am PST
Subject: Schmidt quotes

From Greg Williams (30119)

The following excerpts are from Richard A. Schmidt, MOTOR CONTROL AND LEARNING: A BEHAVIORAL EMPHASIS, Human Kinetics Publishers, Champaign, Illinois, 1982 (dedicated to "former mentors" Jack A. Adams and Franklin M. Henry).

[diagram on page 204 shows "closed-loop model for movement control" with (surprise!) the reference signal and comparator inside the person]

205 - "The idea is that the system [diagrammed on page 204] can 'compute' the expected nature of... sensations in the form of a reference and can compare the feedback it receives on a particular trial with the feedback it expects to receive.... Closed-loop models... are thought of in essentially two ways. First, they provide a basis for knowing if a movement produced is correct or not.... A second way... concerns the control of ongoing movements..."

206 - "The closed-loop model... has been very effective and useful for certain kinds of responses.... the model seems to have considerable appeal for movements in which something is regulated at some constant value... These movements are called TRACKING responses... The most important generalization from... [tracking] research is that if the models are used in computer and/or mechanical SIMULATIONS of the human in which the computer or mechanical device is controlled in ways analogous to those in... [the diagram on page 204], these nonliving devices 'come alive' to behave in ways nearly indistinguishable from their human counterparts. By proper adjustment of certain mathematical or electronic elements in the devices (called parameters), the system can be made to show many human characteristics, and they track with essentially the same levels of error.... The large body of experimental literature suggests that because the human can be mimicked so well by computers that use closed-loop mechanisms such as shown in... [the diagram on page 204], the human in these [tracking] situations can be regarded as a closed-loop control system, responding essentially by analyzing the feedback produced against the reference of correctness and issuing corrections. The evidence does not prove that humans actually track this way, but the agreement between theoretical predictions and data is very strong, and alternative theories cannot boast of similar success."

210 - "Engineers can design robots and other machines to behave... using what they call POINT-TO-POINT COMPUTATION methods. The position of the limb at each point in space and at each time in the movement is represented by a reference for correctness, and the system can be made to track this set of positions across time to produce an action with a particular form. But the system must be very 'smart,' and it must process information very rapidly, even for the simplest of movements. All of these references for correctness must be stored somewhere, and each of the points will be different if the movement begins from a slightly different place or if it is to take a slightly different pathway through space.

Engineers have generally found that these methods are very inefficient for machine (robot) control, which has led many motor behavior researchers (see Kelso, Holt, Kugler, & Turvey, [in G.E. Stelmach & J. Requin, eds., TUTORIALS IN MOTOR BEHAVIOR,] 1980; Kugler, Kelso, & Turvey, [in same,] 1980) away from these kinds of control processes to explain human skills....

A compromise position is that only certain positions in the movements are represented by references of correctness. One viewpoint is that feedback from the movement when it is at its endpoint is checked against a reference for correctness and that subsequent corrections are initiated to move the limb to the proper position. These views of motor control hold that the limb is more or less 'thrown' in the direction of the endpoint by some kind of open-loop control and that then the limb 'homes in on' the target by closed-loop control."

211 - "... the information-processing mechanisms, which lie at the very heart of the closed-loop system shown... [on page 204], require a great deal of TIME AND ATTENTION for stimuli to be processed to yield a response.... with rapid

actions sufficient time is not available for the system to (a) generate an error, (b) detect the error, (c) determine the correction, (d) initiate the correction, and (e) correct the movement before a rapid movement is completed. Muhammad Ali's left jab is a good example. The movement itself is about 40 msec; yet, according to our estimates [made earlier in the book on the basis of movement-correction experiments], detecting an aiming error and correcting it during the same response should require about 150 to 200 msec -- the time necessary to complete the activities of the stages of information processing.... Another problem is that the time between two responses that are not grouped (see... [M.C. Smith, in W.G. Koster, ed., ATTENTION AND PERFORMANCE II, 1969]) was found to be essentially 190 msec. Therefore, the time between successive corrections (because corrections are responses) should be similarly separated. This information has serious consequences for models of human limb control that demand a large number of attention-based corrections in a very short period of time. Point-to-point computation models have this basic problem when human performance is considered."

212 - "What about the possibility that the central nervous system contains closed-loop mechanisms that do not require any attention?"

215 - "... experiments like Dewhurst's ([IEEE TRANS. BIOMED. ENGR. 14, 167-171] 1967) show that corrections for suddenly presented changes in position can be initiated far more rapidly than the earlier 200-msec estimates, with correction latencies being from 30 to 80 msec in the various investigations that have been done. This kind of result suggests that the information-processing stages are not involved in these corrections, as the stages require too much time for processing."

243 - "On strict experimental grounds [deafferentation studies], the evidence does not really show that the [open-loop] response-chaining hypothesis is incorrect, although it strongly suggests it. A more reasonable hypothesis would be that feedback is not essential for the production of at least some movements, although it is likely that feedback provides increased flexibility and improved fine movement control. The possibility remains that under some conditions or for certain kinds of skills the response-chaining hypothesis might be correct, but it seems fair to say that it is not correct in general."

249 - "Henry and Harrison (PERCEPT. MOT. SKILLS 13, 351-354] 1961) asked subjects to begin with a finger on a key located by their hip and at a 'go' signal to move forward-upward to trip a string in front of their right shoulder. They were to do this as quickly as possible. The simple RT in this situation was 214 msec on the average, and the movement time was almost the same, at 199 msec. Sometimes a second light would come on indicating that the subject should avoid tripping the string or at least begin to slow the limb as quickly as possible. The 'stop' signal could come on at one of four points: 110, 190, 270, and 350 msec after the 'go' signal....

250 - Henry and Harrison measured the time to begin to decelerate the limb after a 'stop' signal.

Their results are simple. Only when the 'stop' signal was given at the 110-msec location was there a tendency for the subjects to start to slow the movement before it had been completed. But the more interesting feature of these data is the subject's response in the 190-msec condition. Notice here that the 'stop' signal came on 24 msec BEFORE the movement even started, and yet the movement was carried out without interruption....

If the information-processing stages are too slow to be involved in details of a particular action..., then the question is: What does produce these patterns of action? The best theory to have been proposed at this point is that these patterns are PREPROGRAMMED, structured in advance, and run off as a unit without much possibility of modification from events in the environment. To me, this is one of the strongest lines of evidence available that movements are controlled by motor programs."

There is more, but I'm tired of typing for now. If this is useful, let me know and I'll probably post another installment.

As ever, Greg

Date: Tue Jan 19, 1993 6:17 am PST
Subject: Re: Schmidt quotes

[From: Bruce Nevin (930119 08:44:23)] (Greg Williams (30119))

Controlling for the stop signal I assume is at a higher level of the hierarchy, and the delay corresponds to that in the portable demo described in BCP (extended arm is raised before lowering it in response to signal).

Isn't some of their time problem due to thinking of the loop sequentially (trial and then error and then correction in another trial) rather than concurrently and continuously?

Bruce bn@bbn.com

Date: Tue Jan 19, 1993 9:13 am PST
Subject: Devil's Biblio/Schmidt

[From Rick Marken (930119.0800)]

I'm glad to see the contributions to the "Devil's Bibliography". Avery -- keep posting candidate entries whenever you find them. They won't get lost. Greg (as you can probably tell by now) is an excellent archivist and publisher. Maybe someday the "Devil's Bibliography" entries will be compiled in an issue (or two) of Closed Loop.

Greg Williams (30119) --

Thanks for the excerpts from Schmidt. You ask:

> There is more, but I'm tired of typing for now. If this is useful, let me know and I'll probably post another installment.

Yes. It's EXTREMELY useful. Thank you for doing it and when your hand and arm return to normal please post some more. There are some real gems in what you posted already such as:

> 205 - "The idea is that the system [diagrammed on page 204] can 'compute' the expected nature of... sensations in the form of a reference and can compare the feedback it receives on a particular trial with the feedback it expects to receive

Sounds right to me; of course, the comparison is part of a continuous process that makes the feedback sensations match the "expected" (reference) sensations. But the statement is consistent with (at least part of) PCT.

After describing how well closed loop models mimic behavior, Schmidt makes the following, remarkable statement:

> The evidence does not prove that humans actually track this way, but the agreement between theoretical predictions and data is very strong, and alternative theories cannot boast of similar success."

So why the apparent eagerness to abandon closed loop models in favor of other schemes? The following paragraph seems to hold a clue -- it shows that there is a strong desire to maintain the "information processing" concept of the nervous system (stimulus input -->processing-->response output). I believe this is the result of an unconscious assumption that the "causal model" that underlies the information processing view MUST be correct -- closed loop feedback or not. This paragraph is a candidate for the frontpiece of the Devil's Bibliography:

> 211 - "... the information-processing mechanisms, which lie at the very heart of the closed-loop system shown... [on page 204], require a great deal of TIME AND ATTENTION for stimuli to be processed to yield a response.... with rapid actions sufficient time is not available for the system to (a) generate an error, (b) detect the error, (c) determine the

correction, (d) initiate the correction, and (e) correct the movement before a rapid movement is completed. Muhammad Ali's left jab is a good example. The movement itself is about 40 msec; yet, according to our estimates [made earlier in the book on the basis of movement-correction experiments], detecting an aiming error and correcting it during the same response should require about 150 to 200 msec -- the time necessary to complete the activities of the stages of information processing.

The idea that this is all part of a continuous LOOP is lost in the dust of the causal (input-output) view of behavior.

Thanks again for all the work on this Greg.

Regards Rick

Date: Tue Jan 19, 1993 9:36 am PST

Subject: Re: Devil's Bibliography

[From Bill Powers (930119.0900)]

Today is JAMES WATT'S BIRTHDAY.

Ft. Lewis' computer is down again; the Fine Arts building collapsed under the weight of a new heavy snow and this has probably upset everything, although the computer is not in that building. So I might as well go on with the Devil's Bibliography.

This is Myerson, J. and Miezin, F. M.; The kinetics of choice: an operant analysis. Psychological Review Vol. 87, No. 2, 160-174 (1980). Quotations are placed between pairs of dashed lines.

M&M:

The results of a large number of experiments on concurrent schedules are well described by the matching law (Herrnstein, 1961),

$$(1) \quad B_1/(B_1+B_2) = R_1/(R_1+R_2)$$

where B_n is the rate of response or amount of time allocated to some behavior $_n$ and R_n is the obtained rate of reinforcement for this behavior. (p. 161)

WTP:

This equation shows a relationship between behavior rate and reinforcement rate. Without altering the algebraic relationship, we can simplify it:

1 multiply by denominators:

$$\begin{aligned} B_1 \cdot R_1 + B_1 \cdot R_2 &= B_1 \cdot R_1 + B_2 \cdot R_1 \\ B_1 \cdot R_2 &= B_2 \cdot R_1 \end{aligned}$$

$$2. \quad B_1/R_1 = B_2/R_2, \text{ or } B_1/B_2 = R_1/R_2$$

The first form of 2. states that the ratio of behavior rate to reinforcement rate on each schedule is the same. This will be trivially true for any pair of identical fixed-ratio schedules, because B_n/R_n is merely the number of presses required to produce one reinforcement. Equation 2 states that this number is the same on both schedules. Note that the "rate" aspect drops out because both numerator and denominator contain inverse seconds as units.

However, the result will be wrong for any pair of fixed-ratio schedules in which the number of presses per reinforcement is not the same for both choices. Whether the prediction is judged wrong will depend on how far the prediction must be from the data while the data are still considered "well-

described." There seems to be no reason to believe that the results will be any better for variable ratio or for any interval schedules.

M&M:

In the two-alternative case, reinforcing a response for which preference is $P_1 = B_1/(B_1+B_2)$ decreases preference for the alternative, P_2 . The rate at which P_2 decreases will be proportional to the rate R_1 , at which the first response is reinforced. Thus

$$(2) \quad dP_2/dt = -kR_1P_1$$

where the constant k governs the proportion of change per reinforcement.

Because preference is defined as relative probability, $P_1 + P_2 = 1$, and decreasing the preference for one alternative must therefore increase the preference for the other. Therefore, the decrease in P_2 in Equation 2 implies an equal and opposite reaction, a compensatory increase in P_1 :

$$(3) \quad dP_1/dt = +kR_1P_2 \quad (\text{p. 162})$$

Probability has nothing to do with it. If $P_n = B_n/(B_1+B_2)$, then $P_1+P_2 = 1$ because $B_1/(B_1+B_2) + B_2/(B_1+B_2) = (B_1+B_2)/(B_1+B_2) = 1$.

M&M:

If, however, in addition to reinforcing one response at rate R_1 , we also reinforce the other response at rate R_2 , then equation 3 must be supplemented to include the proportional decrease in P_1 produced by reinforcement of the second response.

Note that R_1 and R_2 have suddenly become manipulated independent variables.

M&M:

Moreover, Equation 2 must be supplemented to include the compensatory increase in P_2 that comes from decreasing P_1 . Therefore the complete system is described by

$$(4a) \quad dP_1/dt = kR_1P_2 - kR_2P_1$$

and

$$(4b) \quad dP_2/dt = kR_2P_1 - kR_1P_2$$

This system is represented diagrammatically in Fig. 1a. At equilibrium,

$$dP_1/dt = dP_2/dt = 0,$$

Whereupon Equations 4a and 4b both lead to

$$R_1/R_2 = P_1/P_2 = \frac{B_1/(B_1+B_2)}{B_2/(B_1+B_2)} = B_1/B_2$$

which is algebraically equivalent to Equation 1 (Baum & Rachlin, 1969). (p. 162)

Note that Equations 4a and 4b are not a "system" of equations. The two equations are linearly dependent and are just two ways of writing the same relationship.

So M&M have managed to run this series of permutations of the original equation around to its starting point, which is a statement that the ratio of behaviors to responses is the same on both choices (even if the authors have failed to see that this is what it states).

M&M:

The general kinetic model just described is a hypothesis concerning the functional relation between the reinforcement input to the organism and the behavioral output. (p. 163)

It is no such thing. In all operant experiments, reinforcement is a function of behavioral output; that is what can be observed. Behavior is the input to the scheduling apparatus; reinforcement is the output of the apparatus. There may be a simultaneous dependency of behavioral output of the organism on reinforcement input to the organism, but to describe that would require a second basic equation to be solved simultaneously with the first.

By plodding through the rest of the paper one can show that every step of the development is just as far from real system analysis as the initial part. The mathematics gets more and more involved as the paper proceeds, so it becomes more and more tedious to show that each successive form is merely a transformation of the preceding forms, extending the tautology (and the self-contradictions) to ever greater complexity, and that all the "predictions" of dynamics are simply curve-fitting of arbitrary mathematical forms to the data. The entire paper is an attempt to make deductions about a system-environment relationship by taking into account only one of the two necessary functional relationships -- and attributing that one incorrectly to the organism.

M&M:

Powers (1973,1978) has contended that systems analysis represents a theoretical alternative to behaviorism. However, the present treatment demonstrates the compatibility of the two approaches and suggests that the techniques of systems analysis have much to offer to the mathematical behavior theorist. Powers' analysis differs from ours in two important ways. First, his is based on a control theory model. Control theory is a branch of systems analysis that assumes the existence of control [sic] variables or reference values [sic sic sic] with which inputs are compared (Milhorn, 1966). Although such hypothetical reference values have their uses, they are by no means necessary for the analysis of feedback systems (Milhorn, 1966; Rosen, 1970). Second, in opting for a "quasi-static" analysis (Powers, 1978), we believe that Powers has forsaken perhaps the most important attribute of systems analysis, its ability to describe both transient-state analysis and equilibrium behavior. (p. 172).

No comment required in the present company.

Best to all, Bill P.

Date: Tue Jan 19, 1993 1:27 pm PST
Subject: Devils Bibliography: A Special Corner

[Rick Marken (920119.1200)] Avery Andrews (930119.1919)

> Figure 11 on pg. 1446 is supposed to represent a 'feedforward' system operating alternately with a 'feedback' system

I think there should be a special corner in the hell of the Devil's Bibliography for pronouncements about the effectiveness of feedforward.

Feedforward is just Ashby's "compensating regulator" that Bill Powers (930118.1600) demolished rather uncomfortably with his newly coined "parable of the bathtub". The corner of hell for feedforward zealots should have Powers' bathtub attended by two devils, one with 200 pounds of ice cubes, the other with 50 gallons of boiling water -- AND NO THERMOMETER ANYWHERE.

> feedforward is described by paraphrasing a (clear and cogent) exposition by Houk and Rymer (1981)

Houk & Pymer's description of feedforward may be cogent -- but if they think feedforward actually works (produces control) then it's the Powers bathtub corner for them too.

Best Rick

Date: Tue Jan 19, 1993 2:04 pm PST
Subject: devils bathtub

[Avery Andrews 930119.730] Rick Marken (920119.1200)

Houk & Rymer (and even Arbib) don't suggest that feedforward can be effective on its own - in their diagram it acts in concert with a feedback system (like turning the steering wheel on the basis of the perceived curvature of the upcoming road, which can be done in accord with a formula posted by Bill during the steering discussion a few months ago. So I'd put H&R on the goodguy list (after all, Houk & Milhorn (1984) described the posture-control circuitry that Bill & Greg simulated a simplified version of).

(Greg Williams (30119))

Wow! The page 210-211 quotes are really off the wall. It seems to me that this stuff can be used in arm documentation. E.g. Jordan & Rosenbaum dismiss feedback as too slow citing only Schmidt, but judging from these quotes he's demonstrably so ignorant that they seriously discredit themselves by doing this.

'Event-based' analysis seems to be a common thread running through many of these DB entries, & it's interesting that that's where Beth Preston's Synthese paper falls apart too, in my judgement. Also assuming that reaction times for voluntary activity have much to do with the existence of lower level closed-loop mechanisms. I think that a lot of the movement-control schemes in Jordan & Rosenbaum are event-based too, but I'm not sure, since the prose is fairly vague (or my math too feeble).

But, it does seem like at the vicinity of 212 Schmidt might be getting around to making sense, but I don't get a clear conception of what overall conclusions he's coming to there.

Avery.Andrews@anu.edu.au

Date: Tue Jan 19, 1993 2:37 pm PST
Subject: Re: Devil's Bib comments

[From Bill Powers (930119.1430)] Greg Williams (930119) --

RE: Devil's Bibliography

The excerpts from Schmidt make him sound like an almost-good-guy. Actually, a good guy who is looking for whatever there is in control theory that seemed to him usable. What he's missing is the concept of a hierarchy of control. It's interesting that even in 1982 he is citing the engineers' "point-to-point"

method of controlling movements, and the fact that it is computationally expensive. This idea clearly influenced Kelso et. al. It would be interesting to see if we could find where this approach to movement started -- probably WAY back. The idea that a simple analogue signal variation could drive a controlled movement with hardly any computation at all seems to have been lost along with analogue computers.

Is there any way to check out something about his "former mentor," Jack A. Adams? Back in '57 or '58, Bob Clark, the late Bob MacFarland, and I went to the U of IL to give a seminar for O. Hobart Mowrer's graduate students. Among them, I think, was Jack Adams. Mowrer was very taken with our concepts of "feedback theory" as we called it then (and as the Adams in the literature refers to it). Mowrer devoted a good part of chapter 7 of Learning theory and the symbolic process (New York: Wiley (1960)) to our ideas. If this is the same Jack Adams, that would be interesting, because as far as I know he doesn't cite us or Mowrer. If it isn't the same Adams, it's not so interesting.

I just computed the minimum acceleration of Ali's fist, from the remembered data that his jab moved about 6 inches. If this took 40 milliseconds as cited by Schmidt, Ali's fist accelerated at 187.5 meters/sec², or 19 g's. Obviously no control is possible during such a brief movement, but it's easily explainable as resulting from a step-change in the position reference signal. The Little Man exhibits such speeds of movement when the kinesthetic reference signal is stepped abruptly to a new position (I set the parameters to give about 100 millisecond movements, but the model can go faster). If the reference position is set beyond the target, as fighters are supposed to do, the transition time to hit the target can be even less than 40 milliseconds even with the model set to go 95% of the way to the target in 100 milliseconds.

Bruce Nevin (930119.0844) --
RE: Schmidt

> Controlling for the stop signal I assume is at a higher level of the hierarchy, and the delay corresponds to that in the portable demo described in BCP (extended arm is raised before lowering it in response to signal).

Yes, that's what I would say, too.

> Isn't some of their time problem due to thinking of the loop sequentially (trial and then error and then correction in another trial) rather than concurrently and continuously?

I agree that it is.

> Poul Andersen did this sort of thing with a General Semantics premise in the early '50s (The World of Null-A f'rinstance).

Writing under the name of A. E. van Vogt? Actually I think World of Null-A came out a good deal earlier than the '50s -- it was my introduction to Korzybski. I remember the fizz of intellectual excitement when I realized that the fascinating quotes used for chapter headings came from a REAL BOOK! I spent my later high-school years trying to develop that "cortico-thalamic pause." It greatly improved my ping-pong game.

Best to all, Bill P.

Date: Tue Jan 19, 1993 2:47 pm PST
Subject: devils bathtub

[From Rick Marken (930119.1400)] Avery Andrews 930119.730

> Houk & Rymer (and even Arbib) don't suggest that feedforward can be effective on its own - in their diagram it acts in concert with a feedback system.

Oh, all right. They can have the tub in the PCT corner of heaven which has a thermometer that shows current temp, derivative of temp and a "predictive display" of up to 10 minutes of estimated future temp -- constantly revised based on changing outputs (ice cubes, boiling water and atmospheric conditions) -- the delux model. I agree that this kind of "feedforward" could be used to improve control (the predicted information is still part of a closed control loop -- the word "feedforward" may be what is misleading; predicted future information is just a present time perceptual input that can be a controlled variable -- or part of a controlled variable -- like any other controlled variable).

Best Rick

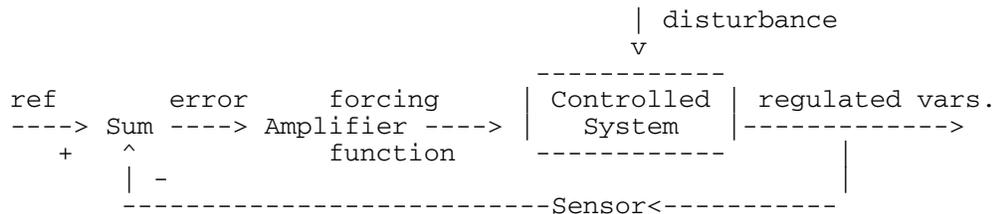
Date: Tue Jan 19, 1993 3:29 pm PST
 Subject: Schmidt

[Avery Andrews 930129.0952]

It seems to me that the critical thing w.r.t. Schmidt is to figure out what aspects of it Jordan and Rosenbaum were thinking of as evidence for feedback being too slow. I leaped to the conclusion that it was the 210-211 stuff, but we better be careful about getting this right. By the way, I don't notice what I would call the 'event-based blunder' on the list in QAPB. Does it have a standard name?

Presumably at the bottom of the pit in our inferno would be McCulloch, who seems to be deeply implicated in the event blunder & subverting understanding of continuous/analog computing in general.

Houk & Rymer on the other hand strike me as definitely being good guys. The main problem with their exposition is that, although it is very clear, their diagrams are not appropriate for psychology (they seem based on the requirements of chemical engineering): there;s a box labelled 'controlled system' with arrows coming out labelled 'regulated variables', with the effectors, comparators, etc. outside the 'controlled system'.



Looking at this, it's basically the same as Bill's diagrams, but inside out: the 'Controlled system' is the entire environment, the forcing function is the effectors, etc.

Avery.Andrews@anu.edu.au

Date: Tue Jan 19, 1993 9:07 pm PST
 Subject: Bathtubs; feedforward

[From Bill Powers (930119.2200)]

If I used all the ice and you used all the boiling water, and the ice was just freezing, I think the bath would come to 84 F or 29 c -- pretty tepid. That's 200 pounds of ice and 800 pounds of boiling water. Anybody check that out? Of course you don't know how far below freezing the ice cubes were. One assumes sea level and distilled water.

RE: feedforward in steering a car, and ignore this if I'm repeating myself.

If you adjust the place on the road where you look, you can anticipate the curves without feedforward. By looking at the road just far enough ahead of

the car, you will see a deviation in the road before the car gets to the curve. This distance has to be adjusted for the speed of the car, so higher-level control is involved. With just the right distance, reacting to the disturbance in the usual way will result in the commencement of a steering effort just when it's needed. Try it: it works. Of course there's no way to prove that anyone ever did it this way before.

Best, Bill P.

Date: Wed Jan 20, 1993 5:22 am PST
Subject: More Schmidt quotes

From Greg Williams (930120)

I think the burgeoning DB + critique thereof could be put to good (if mischievous?) use. Why not work up the quotes and challenges/reinterpretations and send copies to lots (ca. 50?) of "biggies" in the "harder" (???) end of establishment psychology? An epistle from CSGnet to YOU: we have some problems with the basic assumptions... might you be able to set us straight. Even if not one joins the net, I bet at least a few will have their eyes opened a bit to the fact that they have been glossing over some difficulties. Anybody else up for this? The dialogue needs to escape the confines of the net, in my opinion -- without the necessity of submitting to a journal. I think we need ongoing interactions with REAL "devils."

OK, Rick, here's some more. (You ARE a nice guy, despite what they say about you on the net.)

253 - "... Keele and Posner ([J. EXP. PSYCHOL. 77, 155-158] 1968).... trained their subjects to move a hand held stylus (like a pencil) to a small target a few centimeters away. Knowledge of results about MT [movement times] was provided, and subjects were trained to move in either 150, 250, 350, or 450 msec, in separate sessions. Then, when the subjects had perfected the MTs, on certain trials (unpredictable to the subjects) the experimenters would turn off the room lights when the subjects left the starting position, so that the remainder of the movement would have to be made in the dark. The logic was this. If, in the 250-msec condition, for example, the lights-on condition produced less movement error than the lights-off condition, then the lights being on must have contributed to accuracy, and the implication would be that vision was being used for movement control. Conversely, if no differences in accuracy were obtained between the lights-on and lights-off conditions, then the implication would be that vision was not being used in the response and that visual feedback processing time was greater than the corresponding MT in these responses.

Keele and Posner (1968) measured the probability of missing the target for the four MT categories for the lights-on and the lights-off conditions... The most important finding is that, in the 150 msec MT condition (the actual MT was 190 msec), about as many target misses were recorded when the lights were on (68 per cent) as when the lights were off (69 per cent). The authors argued that when the MT was 190 msec or less, the vision of the hand or target did not contribute to movement control; thus, they argued that using visual feedback required at least 190 msec (one RT [reaction-time]) to process. Also, as the MTs increased, an advantage began to develop for having the room lights on. When the MT was 260 msec, there were 47 per cent misses for the lights-on condition and 58 per cent misses for the lights-off condition. The lights-on advantage increased steadily as the MT increased, so that with a 450 msec MT, the lights-off condition had 47 per cent misses and the lights-on condition had only 15 per cent....

254 - Hawkins et al. [ms. in prep.] performed a series of experiments much like that of Keele and Posner, except that their subjects practiced for a block of trials with vision always presented, and then a block of trials with vision always withheld, thus eliminating the problem of having the vision unexpectedly removed. They plotted the total variability around the target... as a function of the MT... The lights-on condition was beneficial at all MTs, even for those as short as 100 msec! These data suggest that vision, when its presence can be expected, can be used in far less time than the 190 msec that

Keene and Posner suggested, perhaps with visual processing times as short as 100 msec.... Another experiment showed that the advantage of the lights-on condition vanished when the MTs were 80 msec..."

255 - "Taken together, these findings seem to provide a contradiction to the general notion that vision presented in the environment cannot influence a movement until all of the stages of information processing have been completed -- that is, until about 190 msec have elapsed. How can the advantage of vision occur when the MT is far shorter than this 190 msec? One answer is that vision in these situations is not processed like a suddenly presented stimulus to which a response must be made, because more or less continuous vision of the hand and target occurs during the movement...."

Another possibility is that vision 'tunes' the motor system for accuracy... It is possible that corrections in the movement do not occur at all, but that vision prepares the spinal apparatus for the ongoing movement... These ideas are speculative..."

299 - "... it does not make sense to claim that motor programs operate without feedback."

As ever, Greg

Date: Wed Jan 20, 1993 9:51 am PST
Subject: More Devil's Bibliography

[From Rick Marken (930120.0900)]

No collection of misconceptions about PCT (and general feedback control) would be complete without a quote or two from:

Fowler, C. and Turvey, M. T. (1978) Skill acquisition: An event approach with special reference to searching for the optimum of a function of several variables. In G. Stelmach (Ed.) Information processing in motor control and learning (pp. 1-39) New York: Academic Press

An entire section of this article is dedicated to Powers HPCT model of behavior. It starts on p. 26 and runs to p. 31. Their efforts to show the limitations of the HPCT model are detailed, quantitative and completely wrong. Despite numerous demonstrations of the fallacies and misconceptions in the Fowler/Turvey paper (as well as letters to Turvey -- the main "bad guy -- see below) explaining these problems) there has never been any reply from Fowler/Turvey and certainly never an apology for this terribly mischievous "trashing" of HPCT.

Here are a couple of quotes from the end of the section describing (incorrectly) HPCT.

" Quantitative knowledge of results must rarely be informative in a hierarchical closed-loop system because, typically, there is a one to many mapping between an error signal and the conditions that may have provoked it. We can conclude from that, perhaps, that the actor/perceiver is not appropriately characterized as a hierarchy of control systems at least when he is performing tasks in which he must exploit the abstract information putatively extracted by the superordinate levels of the system". p 31

This statement is the typical mixture of some correct statements and some incorrect assertions that is the hobgoblin of PCT reviews. Yes, there is a one to many mapping of error to outputs in a hierarchy of control systems, but this problem is solved on the input side of the hierarchy -- when there are several higher order systems.

"The closed-loop model of Powers characterizes the actor as an inflexible general-purpose device". p 31

No comment necessary -- Oh. All right. Can everyone say "reorganization".

An interesting followup to this catastrophic article (from an HPCT perspective -- maybe an accurate report would have allowed the PCT revolution to start in the early 1980s) is that Carol Fowler was the Editor at JEP who handled the review of my 1986 Hierarchical Control paper which was EXTREMELY critical of the Fowler/Turvey article. Yet, Ms. Fowler was not only fair in the review process -- she was largely responsible for arguing for it's acceptance (since there were some disagreements between the reviewers about its merit). So, although Carol Fowler happens to be the first author of one of the most notorious anti-PCT articles ever published, I would have to class her as one of the "good guys" -- if not in terms of understanding PCT, certainly in terms of scientific integrity. I think she must have been a student of Turvey's at the time the article was written -- and under his apparently strong "trendy science" spell (I've never met him but I hear he talks a pretty convincing line of BS).

And, yes, it's true -- I AM a nice guy; though not nearly as nice as Greg and Ray are for saying it.

Best Rick

Date: Wed Jan 20, 1993 12:25 pm PST
Subject: Re: feedback too slow

[Avery. Andrews 930121.700]

I don't think that sending out a list of embarrassing quotes to 50 big names is the right way to go. After all, it's not the big names who are the worthwhile targets, but their prospective students, and assorted dissatisfied mavericks. Priority one is to get a useful document available online for CSGNet, so that people who are intrigued by what we're up to but not sure that it isn't bull can get some easily verified hard evidence that many establishment figures are seriously confused. Then perhaps some kind of article somewhere.

Re 6 DoF, has anyone analyzed the DoF of a set of octopus tentacles, I wonder? (and they can learn to open jars with them ...)

Avery.Andrews@anu.edu.au

Date: Wed Jan 20, 1993 1:52 pm PST
Subject: Schmidt's Book

[FROM: Dennis Delprato (930120)]

Late last week I promised AVERY ANDREWS I would try to locate R. A. Schmidt's Motor Control and Learning: A Behavioral Emphasis. I found the 2nd ed. (1988, Champaign, IL: Human Kinetics Press). Given Greg's marvelous efforts with the 1st ed., you may not need anything further from Schmidt. However, if you, AVERY, supply me with your regular mail address, I could pass on hard copy of any pages I might find that update anything in the 1st ed. I want to live up to my promise, but don't have the flying fingers of Greg.

Date: Wed Jan 20, 1993 3:53 pm PST
Subject: schmidt not a goodguy

[Avery Andrews 920121.1030]

Here's some evidence that Schmidt and a number of other people don't understand feedback very well:

Cruse, H, J. Dean, H. Heuer and R.A. Schmidt (1990) 'Utilization of Sensory Information for Motor Control', in O. Neumann & W. Prinz, Springer-Verlag, pp 43-'79.

This article covers a lot of ground, and makes some proposals that may well be right, as far as I know, but the smoking guns of ignorance appear on p. 60,

where they are discussing a model in which commands are sent out to effectors on a 'feedforward' basis, with a feedback system supplying corrections (sort of like what I've speculated for driving). The bloopers:

1. "The advantage of an open loop system is that it responds quickly to an input signal, and cannot become unstable like a closed loop system subject to a delay in the feedback signal."

AA The first statement being wrong, since a reference level and a current perception can be compared monosynaptically, which is trivial in the overall time-budget. The second is true, but irrelevant, since you get the possibility of instability once a feedback system is included.

2. "Owing to the operation of the open-loop system, the closed-loop system only has to deal with small error signals, so the gain (S) can be relatively low."

AA Wrong because you need a high-gain system to compensate for small errors.

There's lots more here to mull over, but I can't do it today.

Avery.Andrews@anu.edu.au

Date: Thu Jan 21, 1993 2:19 pm PST
Subject: devils bibliography - bizzi

[Avery Andrews 930122.0906]

Bizzi's work seems soundly and closely reasoned, but there's a bit of evidence that at least in 1976 he had some strange ideas about how kinesthetic feedback would be expected to work. It appears in

Bizzi, E., A. Polit and P. Morasso (1976) 'Mechanisms underlying achievement of final head position', J. Neurophys. 39:435-444.

The major point of the paper is that when monkeys orient their heads towards a flash of light, the final head position seems to be determined whether feedback is available or cut off (by deafferentation), and that adding either inertial or constant force loads to the head does not affect the final position (once the constant force load is removed).

The strange idea about kinesthetic feedback arises in discussing the possibility of a mechanism comparing actual with desired vs. actual current head position: "the output of this hypothetical comparator might provide a signal leading to the cessation of the ongoing motor pattern." (rather than the cessation of a signal from the comparator causing a cessation of the motor effort). This idea is attributed to two earlier papers:

Gibbs, C.B. (1954) 'The Continuous Regulation of Skilled Response by Kinaesthetic Feedback', British Journal of Psychology 45:24-39. Eccles, Sabah, Schmidt + Toborikova (1971) 'Modes of Operation of the Cerebellum in the dynamic loop control of movement,' Brain Research 40:73-80.

Also somewhat suggestive is the conclusion drawn from one of the experiments. In this experiment intact monkeys orient their heads to a light flash against a constant force load, which is released soon after the movement commences. There is no pre-set position they are trained to achieve, but what happens is the head stops for while, and then, when the load is removed, moves a bit further, as if there were a spring-like force attracting it to its final position, so that the initial part of the turn stops when the load balances the spring force.

This is consistent with any number of possibilities (such as a kinesthetic reference level with a relatively low gain control system), but the conclusion drawn is:

"the program for final position was maintained during load application and was not readjusted by proprioceptive signals acting at segmental or suprasegmental mental levels" (438).

or, restated:

"proprioceptive signals originating from the moving neck fail to reset the central patterns responsible for final position" (442)

True enough, but what is not so clear is why this would be expected to happen in the first place: what makes this non-reprogramming significant? Describing what is basically a fixed setting of some sort as a 'program' also seems a bit odd.

In a later paper,

Bizzi, E., P. Dev, P. Morasso and A. Polit (1978) 'The Effect of Load Disturbances during centrally initiated movements', J. Neurophys. 41:542-556.

they calculate that the feedback loops (spinal and such higher level kinesthetic as may be operating) contribute between 10% and 30% of the spring-like restoring force.

Something that strikes me about the literature I've seen so far is that people seem to have the spinal reflex loops reasonably well in hand, but the show falls apart when it comes to kinesthesia, arguably, I think, due to the profusion of false ideas about kinesthetic feedback would actually work.

Avery.Andrews@anu.edu.au

Date: Thu Jan 21, 1993 3:56 pm PST
Subject: kugler et al

It might be worth figuring out where Turvey et. al. get the following absurd idea from:

"In control theory, the command-algorithm is separate from the power-flux that it modulates; in the neurophysiology of movement, the central nervous system is held conceptually separate from the skeletomuscular apparatus that performs the movement"

Kugler, Scott, Kelso and Turvey (1980) 'On the Concept of Coordinative Structures as Dissipative Structures', in Stelmach & Requin (eds) Tutorials in Motor Behavior, North Holland 3-48.

Absurd because people like Rack, etc. go to great lengths to establish the physical properties of the power-generators, and their effects on the properties of feedback loop.

Probably out of the same vein as the PCT criticisms that Rick regales us with.

Avery.Andrews@anu.edu.au

Date: Fri Jan 22, 1993 11:36 am PST
Subject: kugler et al/robot requirements document

[From Rick Marken (930122.0800)] Avery Andrews says --

> It might be worth figuring out where Turvey et. al. get the following absurd idea from:

> "In control theory, the command-algorithm is separate from the power-flux that it modulates; in the neurophysiology of movement, the central nervous system is held conceptually separate from the skeletomuscular apparatus that performs the movement"

This doesn't seem absurd to me; in fact, it sounds exactly correct. If by "command algorithm" they mean the error signal (or the process that converts perceptual into error signal) and by "power flux" they mean the forces exerted by the muscles as a result of influence by the error (command) signal then they are treated separately in control theory -- inasmuch as they are treated as separate variables (which they are). The "output" part of the control model says that $o = f(e)$. For systems at the lowest level of the control hierarchy, o could be called "power flux" and e could be called the "command signal". They are conceptually separate. My question for Kugler, Kelso and Turvey, then, is SO WHAT? What's wrong with treating two separate variables that are functionally related as two separate variables that are functionally related? The important thing about control theory is that it also says that $e = g(o,p,r,d)$ -- that is, the command signal is (at least in part) a result of the very "power flux" (o) that it commands. This closed loop relationship MUST be taken into account when analyzing (as you say Rack, etc do) "the physical properties of the power-generators, and their effects on the properties of feedback loop".

So I would say that the above clam is not absurd; it is just irrelevant (at least, I cannot see it's relevance to anything); it just seems like part of the constant desire by conventional psychologists (and roboticists, etc) to say something about control models of behavior that might be construed as negative -- but is usually wrong or (like the above) a non-sequitur. The goal seems to be to dismiss perceptual control models in order to get on with the real business of wasting time on "self-regulating" systems (attractors models) and other complex output generation/planning schemes.

Best regards Rick

Date: Fri Jan 22, 1993 11:28 pm PST
Subject: reviews & replies

[Avery Andrews 930123.1825]

I think the reviews & replies proposal is a great opportunity, but it also carries a certain risk of getting clobbered again, since the usual myths about feedback are probably all out there, & I don't think the current range of PCT publications deals with them adequately. So I think there's got to be some kind of 'myths about feedback' article included to function as a preface. Something along the lines of Bill's 'Objections to PCT posting' (covering feedback too slow and deafferentation), but dressed in full academic battle gear.

It might be useful to point out that PCT is basically in the same camp as the dynamics crowd (yes, Kugler, Turvey, etc.) as opposed to 'orthodox' computational cog. sci, if there actually is such a thing anymore (in the sense that I suspect that nobody really sees it as The Truth anymore, although there are people like me who think that a certain amount of useful work can still be done from that point of view).

Something else that might be true, & if so, worth pointing out, is that misunderstandings about feedback may well have directed research into areas such as acquired skills that arguably involve a lot of pre-programming, and away from routine but non-stereotyped manipulative activity.

Avery.Andrews@anu.edu.au

Date: Sat Jan 23, 1993 12:10 pm PST
Subject: camps, Devil's Biblio

[From Rick Marken (930123.1100)]

Avery Andrews (930123.1825)

> It might be useful to point out that PCT is basically in the same camp as the dynamics crowd (yes, Kugler, Turvey, etc.) as opposed to 'orthodox' computational cog. sci, if there actually is such a thing anymore

I liked to be in camps when I was a kid but not so much any more. And even then, I would NEVER have wanted to be in the Kugler, Turvey etc camp; I wanted to be in camp to explore, build models and have fun; that's why I like camp CSG.

I understand the desire to find value in the work of those who are not PCT modelers but who are working on similar problems (like the motor control problems of Kugler, Turvey, et al). The downside of trying to control one's perceptions relative to this desire is beautifully illustrated in today's contribution to the Devil's Bibliography. It is a book that Ed Ford suggested that I look at:

M.E. Ford and D. H. Ford (1987) Humans as self constructing living systems. New Jersey: Erlbaum.

(No relation to Ed, I presume).

I just got a copy yesterday and it is a goldmine for the Devil's Bibliography. D. Ford seems to be the big theorist here -- taking us to new horizons in the development of the control model. D. Ford is actually a modeler in the Carver/Scheier tradition -- diagrams are all you get. Also like Carver and Scheier he is a happy camper -- he plays contentedly in the camps of Powers, Carver and Scheier, S. Beer (cybernetics guru), Kugler, et al, Pribram, D. McClelland, Bandura, Ashby, etc. Quite eclectic. If you read the overview (chapter one) you might see the problem with this accepting attitude; in order to have it, you MUST make some glaringly basic mistakes about PCT; M.E. Ford and D.E. Ford make PLENTY.

Here are some examples for the DB.

"Feedback information enables a system to react to events after they have occurred. However, for a system to adapt efficiently in a variable environment, it must also be able to anticipate what is likely to happen in the future. Most human behavior is anticipatory in nature. Anticipatory actions are accomplished through feedforward processes". p 9

(Making camp cognition people happy).

"Behavior patterns are a function of the informational-behavioral transactions with the environment"

(Making camp information processing happy; maybe camp interactionism too? nobody's been able to figure out what the kids in that camp actually DO though -- except be "real scientists").

"Perception is 'direct' and provides accurate information about current events within and around a person" p. 22

"Human sensory-perceptual capabilities are designed to collect information useful for GUIDING PRACTICAL ACTION in the physically structured and dynamically varying terrestrial environment in which humans evolved" p 22 emphasis mine

(camp info processing and camp JJ Gibson)

There are many others in this chapter but I'm getting nauseous.

To be fair, Ford and Ford say many things about control that SOUND close to being correct -- or that are correct. But this is the problem of dealing with people who only deal with control theory as a set of diagrams and phrases -- ie. with people who don't model. It is possible to take almost ANYTHING they say as a correct description of a control system.

I think one way to show that Ford and Ford just don't really get it (no matter how often their words might overlap with PCT words) is that they NEVER

explicitly describe the central fact about living control systems -- that they control their own PERCEPTION. They talk all around this but manage to avoid this fact about control and, thus, all that it implies about what we would want to know about the behavior of living control systems -- ie. what perceptions they ARE controlling. Ford and Ford (like all those who don't get PCT) take behavior at face value -- they assume that what THEY PERCEIVE about the organism's doings is what the organism IS DOING (controlling). They have quite a way to go; I'm afraid they (like those in the rest of the camps) think they are just about there. Too bad.

Best Rick

Date: Sat Jan 23, 1993 5:32 am PST
Subject: Good, good, and good

From Greg Williams (930123)

>Rick Marken (930122.0800)

> We now apparently have several "real" robotics listening in on the net. How about asking them to provide a "requirements document" for such a robot.

I second the motion. Chris? John? Other robot researchers? Please!

>Avery Andrews 930123.1825

> So I think there's got to be some kind of 'myths about feedback' article included to function as a preface. Something along the lines of Bill's 'Objections to PCT posting' (covering feedback too slow and deafferentation), but dressed in full academic battle gear.

Last year, soon after the arm paper was returned, Bill spoke with me about working up a summary of the history of the "feedback too slow" myth for inclusion in an expanded arm paper to be submitted to BIOLOGICAL CYBERNETICS (or some other journal). I haven't had time to do it, and you seem to be well-immersed in the subject, so why not continue your explorations and write them up? You are absolutely correct that such an article is needed (whether standalone or as part of the arm documentation). You're probably halfway there, at least. Linguistics can wait, right?

As ever, Greg

Date: Sat Jan 23, 1993 2:57 pm PST
Subject: throwing stones from glass houses

[Avery Andrews 930123.930]

One problem with my writing the contra-feedback myths article is that in many respects I'm in the same situation as the people I'd be criticizing. E.g. in my post about the Schmidt article I said that you needed a high-gain system to correct small errors, but while walking the dog this morning I realized that the magnitude of the disturbing forces is what matters, not the absolute size of the errors.

One of the obstacles to PCT understanding is perhaps that it is a profoundly Newtonian theory, while most people's world views are pre-Newtonian, and the Newtonian picture is *hard* to acquire.

Avery.Andrews@anu.edu.au

Date: Sat Jan 23, 1993 4:06 pm PST
Subject: camps

[Avery.Andrews 920123.1050] Rick Marken (930123.1100)

> "Feedback information enables a system to react to events after they have occurred.

So here we have the event blunder. I'd say something along the lines that events are useful for certain purposes, but we can't assess their overall role adequately without properly understanding the continuous aspect of things.

> However, for a system to adapt efficiently in a variable environment, it must also be able to anticipate what is likely to happen in the future. Most human behavior is anticipatory in nature.

I'd be happy with this if they replaced 'most' with 'much'.

> "Behavior patterns are a function of the informational-behavioral transactions with the environment"

Gobbledegook (we agree here, I think)

> "Human sensory-perceptual capabilities are designed to collect information useful for GUIDING PRACTICAL ACTION in the physically structured and dynamically varying terrestrial environment in which humans evolved" p 22 emphasis mine

Hmm. This sounds fine to me, at least if 'are designed to collect' is replaced by 'have been selected for success in collecting'.

Avery.Andrews@anu.edu.au

Date: Sat Jan 23, 1993 6:50 pm PST
Subject: myths article

[Avery Andrews 930123.1130]

Wobbly as my grasp & background may be, I do have some ideas about how a feedback myths article might be organized, which go as follows:

1. Some examples of egregious blunders (to get people to see quickly that there is a problem).
2. The major myths & how they started.
3. Some bad effects on current research.
4. Some puzzles & anomalies that might be cleared up by the BCP approach.

Avery.Andrews@anu.edu.au

Date: Sat Jan 23, 1993 6:51 pm PST
Subject: devil's bib entry

[Avery Andrews 930123.1350]

Schmidt, R.A. (1980) 'On the Theoretical Status of Time in Motor- Program Representations', in Stelmach & Requin, *Tutorials in Motor Behavior*, North-Holland, pp. 145-166.

Discusses experiments in which levers are moved quickly, in opposition to various sorts of disturbing forces. The results suggest that any positional control has fairly low gain, but the article suggests unawareness of the role of gain in the functioning of feedback systems:

"A feedback view would, of course, predict that the limb would reach the target (against a constant-force disturbance) ..." (p. 159).

"The effects of added or subtracted spring tension on the movement endpoint support the mass-spring notion, and provide additional evidence against the idea that the terminal position is achieved by some sort of feedback process." (p. 160).

Also betrays possible confusion of continuous control with proprioceptively mediated response chaining:

"This argument was strengthened by human evidence that the processing of information leading to a new movement was slow, requiring 150 to 200 msec for the new action to begin. This kind of feedback processing, if it were to be employed in the ongoing control of a rapid motor act like throwing, would be too slow to be effective until that act is completed." (p. 148)

I am frankly not at all sure what's being addressed here: throwing is a *continuous* act, not one that is cleanly segmentable into discrete subacts, so it's not clear how response chaining would work; furthermore initiating a voluntary act is presumably different from ongoing modification of a 'program' (and, Gary Cziko has a demo to the effect that accurate throwing is possible in the face of disturbing forces - a quantitative study would seem called for).

An important point is that PCT does not challenge the existence of CPGs, rather, it simply claims that they will normally produce reference levels for perceptions (and will therefore in general be able to produce error signals and drive behavior when the afferent pathways or cut, although considerable retuning will be necessary to get passably effective behavior).

On a more positive note, the article contains a lot of interesting material, and does refute the impulse-timing view of movement control, which is more counter-PCT than the mass-spring view, and is open to the idea that afferent information modifies the outputs of Central Pattern Generators (p. 147).

It is proposed that programs have a variety of parameters, some of which seem more plausible than others. E.g. Movement time seems plausible (the pattern runs faster or slower), while Force and Muscle Selection seem very dubious. I suspect that Gary Cziko's throwing demo can be turned into a total refutation of the Force parameter (if you can throw accurately against variable disturbing forces, you can't do it with a preset Force parameter).

There is perhaps an avenue of empirical investigation into Muscle Selection as well. People appear to have a fixed handwriting style that is invariant over a substantial size range, from blackboard writing done with arm muscles, to ordinary writing done with fingers. This suggests that a size-scalable perceptual target is involved, perhaps involving kinesthetic effort perceptions, etc. If so, then *deafferented* people would not be expected to have a size-scalable handwriting style, at least when their eyes were closed. Writing on the blackboard with your eyes closed and rubber bands attached to your arms might reveal things as well. ed

Date: Sat Jan 23, 1993 9:38 am PST
Subject: Re: Good, good, and good

[Avery.Andrews 930124.0435]

I don't think I'm halfway there to a feedback too slow paper at all. The literature is huge, I've looked at only a tiny portion of it, & there are people out there in CSGNet with much better backgrounds for interpreting it than I have. Also, linguistics can't wait much longer, since teaching starts up again in a month down here. So I can do some of it, but not all of it.

Avery.Andrews@anu.edu.au

Date: Sun Jan 24, 1993 8:40 pm PST
Subject: Re: camps, Reviews

[Avery Andrews 920125.1529] (Rick Marken (930124.1000))

> If anything is "guiding practical action" in a control loop, it is the net effect of the disturbance on the controlled perceptual variable.

Well, that's pretty much what I interpreted them as saying, but you actually say it, rather than just allowing me to read you that way. The vagueness and ambiguity of a lot of this literature is certainly a major problem with it, which deserves a lot of attention.

Another likely devil's bib entry is:

Adams, J.A. (1971) 'A Closed Loop Theory of Motor Control', Journal of Motor Behavior 3:111-149.

A survey of recent theories of motor behavior that I've been looking at, and may report on shortly, cites this as the origin of the (supposedly defunct) 'closed loop' paradigm.

Avery.Andrews@anu.edu.au

Date: Sun Jan 24, 1993 9:14 pm PST
Subject: formulations

[Avery Andrews 930125.1610]

The bad guys say:

Perception guides action

What Rick Marken says may (I hope) be paraphrased as:

Perception of the net effects of disturbances on a controlled variable guides action

Which is clearly more specific, & therefore more useful.

Avery.Andrews@anu.edu.au

Date: Mon Jan 25, 1993 12:12 am PST
Subject: Re: myths article

[From Oded Maler (930125)]

Re: Feedback too slow

I posted this inquiry to a relatively-prominent researcher in motor control, and the following answer contains some references that might be interesting.

> Secondly with regard to your question- there is out from December the last issue on Current Contents in Neurobiology-motor control with reviews about role of feedback (either by Hasan or Laquaniti I dont remember) A couple of years ago there was also a review by Hasan and Stewart in a book in the series Excercise and Sport Science Reviews (1988 or 1986 or 1987). It is generally a matter of debate- a lot of things can be done without feedback. Actually there is also a paper by Ghez in the Cold Spring Harbor Symposium on the Brain (1990).

Hope this helps --Oded

Date: Mon Jan 25, 1993 4:45 pm PST
Subject: Re: More Devil's Bibliography

> I think she must have been a student of Turvey's at the time the article was written -- and under his apparently strong "trendy science" spell (I've never met him but I hear he talks a pretty convincing line of BS).

I believe that they've married since then. I've also experienced a poor "response rate" from her/them (i.e., answering letters).

Cliff Joslyn

Date: Tue Jan 26, 1993 11:28 pm PST
Subject: Abs & Winstein (feedback too slow)

[Avery Andrews 930127.1710] Rick Marken (930112.0800)

> If you want to read more amusing statements about feedback being "too slow" made by authoritative leaders in the study of human movement control, try the article by Abbs and Winstein in M. Jeannerod (ED) Attention and Performance XIII, Hillsdale, Erlbaum, 1990

I had a look at it, and didn't find the discussion about speed silly at all. They debunked the 200ms myth, showed that feedback thru the oral track could occur in as little as 12 msec, noted that feedback from the distal arms was faster than the proximal arms, maybe in the 30 msec range, etc. It isn't my field, but I didn't see any figures that were at variance with common sense, & the general trend of the literature.

What I did find intriguing was the following assertion:

"Technically, a feedback system is one in which an error signal directly drives a corrective adjustment >at the site where the error is introduced<" (p. 366, my emphasis)

This was supposed to entail that compensatory lip adjustment couldn't be feedback (this is when someone is trying to make, say, a /w/, which requires the lips to come close together, and one lip is disturbed, & the other goes further to make up for it).

Does anyone know any basis for this 'technical' restriction on the scope of feedback? It seems patently wrong, even on the basis of the classic examples of thermostat and ships rudder, where the error might be introduced in front hall, when Fred leaves the front door open for a while, and the compensatory adjustment is made in the basement by the furnace starting.

I didn't have the time to read the whole article carefully, but I did notice that the authors are caught up in the stampede of enthusiasm for 'motor programs', which still appear to me to be a rather vague and woolly concept that we might hope to get somewhere by replacing with the ECS.

Avery.Andrews@anu.edu.au

Date: Thu Jan 28, 1993 3:43 pm PST
Subject: feedback too slow (summary)

[Avery.Andrews 930129.1000]

Here's what I get for the 'feedback too slow' issue (for humans - insects, etc. is a whole nother ballgame). It's not really an article-weight subject, since the standard literature actually seems to have sorted itself out pretty well. To talk about feedback w.r.t. high-speed movements (the jab of Muhammad Ali, professional piano trills) you clearly need to really know physiology and the mathematics of control theory, while for ordinary-speed actions it's entirely clear that feedback is fast enough. One way in which this could be improved is to find more standard references for the basic facts, so that

anybody could find one in a library near them. But the ones given are sufficiently authoritative.

The next topic I want to look at is PCT versus Central Pattern Generators: the 'closed loop' theories of Jack Adams (the very one who wandered into Bill's class, almost certainly) got clobbered when people became convinced of the existence of CPGs, but this is a non-issue for PCT, since there's no reason why there can't be CPG's whose output is treated as a reference level. But this may take longer - I've essentially spent the last month full time on PCT, but I can't keep on doing this.

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People who make generic claims that feedback is too slow are probably (a) thinking of highly practiced rapid movements (b) thinking of the 200-150 ms reaction time for visually presented stimuli. But more recent work shows that ongoing actions can be modified by sensory information much more quickly than that, as reviewed by for example by Schmidt (1982:216-220), Schmidt (1988:164-177), Abbs and Winstein (1990:631-635). (Note the more-than-doubling of the length of the section of Schmidt devoted to this issue. The discussion of the 'wineglass effect' in the 1988 book is particularly interesting from a PCT perspective (Johanssen and Westling 1984, Westling and Johanssen 1984.)

There are two kinds of effects that have been studied for various kinds of muscles, the 'short latency' spinal reflexes, with latencies ranging from 30ms (upper arms), 15 ms (fingers) to 6ms (orofacial). These have relatively low gain, and don't do much to resist serious applied disturbances. Then there are the 'long latency' reflexes involving the brainstem and cerebellum, whose latencies range from 70ms (upper arms) to 44-55ms (fingers) to 20-30ms (orofacial).

From this it seems clear that feedback will not be very useful in controlling certain high-speed movements, such as the left jab of Muhammad Ali, clocked at 40ms. On the other hand it might be possible to trigger it through a kinesthetic feedback loop: set a reference level for a distant position of the fist, then step change for a closer one (deceleration + return), with the timing pre-arranged so that the deceleration is mostly achieved via elastic restoring forces from the opponent's body. But the feedback would not be helping to solve any problems, but would just be creating them (when the grossly out of date sensory information about the movement comes back up the line). Polit and Bizzi (1979) also have some evidence that highly practiced pointing gestures may be acquired as patterns of alpha-gamma activation, since these gestures can be produced accurately with minimal change only 2-4 days after deafferentation (much shorter than the usual recovery period), as long as the starting posture is the same as when the gestures were learned, and no disturbances are applied.

Piano trills, on the other hand, don't offer as severe a problem as one might expect. If a trill contains 16 notes per second, then the two fingers will be cycling at 8 cps, giving a period of 125ms, which should be manageable (barely) by a loop with a 55ms lag (and maybe professional pianists can reduce these lags somewhat). [It would be good for an actual control theorist to check this - I'm just repeating stuff I've read without understanding the math behind.]

In general then, the question of whether feedback is too slow has to be asked for specific kinds of movements involving specific muscles, and for ordinary manipulations of objects the answer is clearly that it is not, although certain fast & practiced movements are obviously exceptions.

One issue that perhaps deserves mention is the speed of feedback in the initiation of 'responses'. Feedback control of the initiation of a response can be thought of as modulating the production of the response w.r.t the extent to which its intended effects already exist. But most significant responses (such as the withdrawal response from heat) are already heavily modulated by various kinds of sensory information, so adding in information about the extent to which the effect of the response already exists is not

going to add significantly to the time budget. Camhi's overall assessment is (p. 111):

"Indeed, there is increasing reason to believe that reafferent feedback plays a significant role in the control of most or all categories of behavior. Neither the speed of the movements nor their directional or other properties should be used as a priori argument against their involvement of such feedback."

The serious speed-of-feedback issues are (a) stability when the frequency of variations in the reference level is too fast w.r.t. the time-delays in the system (b) whether feedback can be helpful in adjusting the ongoing response to current conditions. These problems need to be looked at on a case-to-case basis, with full knowledge of the relevant technical details. In many cases, it seems to me that the results look pretty good for PCT. For example, Camhi (1985) argues for the plausibility of closed-loop control of the rate of turn in the 'escape-reflex' of the cockroach.

A final contaminating factor deserves mention: in Robotics, it seems to normally be assumed that paths and trajectories should be plotted in advance, which involves considerable computational expense (as discussed briefly by Schmidt, for example). This may well be appropriate for robots, since it is perhaps not a good idea to have these expensive, dangerous and very stupid machines deciding what they are going to do on a moment-to-moment basis. Furthermore, in order to make money for their owners, robots have to replace highly practiced assembly-line workers, so that you need something to play the role of practice, which will either be actual practice (adaptive control) or planning. But considerations that are important for Robotics are not necessarily the main priorities for normal human movement.

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Date: Thu Jan 28, 1993 10:25 pm PST
Subject: Re: feedback too slow (summary)

[From Rick Marken (930128.2200)] Avery.Andrews (930129.1000)

> People who make generic claims that feedback is too slow are probably (a) thinking of highly practiced rapid movements (b) thinking of the 200-150 ms reaction time for visually presented stimuli.

Before starting the article (or whatever it is) here, why not spend a moment explaining what the hell people might mean by the idea that "feedback is too

slow". I think the whole concept is ridiculous because it is based on a sequential state, cause effect concept of a control loop -- one which leaves out time (and, hence, a large hunk of reality). How can feedback be "too slow" in a closed loop where feedback is present (as a perception) at the same time that the cause of that feedback (error) is present. There are phase relationships between the continuous variables in the loop --the result of transport lags and slowing factors around the loop. Lags that are too long or slowing factors that are too great can create instabilities in the loop (they can also create stabilities). But it would be necessary to know what is meant by "feedback too slow" and then to test it in a model before one could say whether "feedback too slow" (what ever that is) would be a problem. In fact, without a definition of what "feedback too slow" means in term of closed loop control, it is difficult to know what is being measured in these reaction time experiments. If you apply an impulse disturbance to a controlled variable, there will be a change in the output variable; what is the "reaction time" here? The time to maximum output, the time until this output has some other effect (like pressing a button), the time until the derivative of the output is maximum, minimum? And when you decide what reaction time is, has it been measured in the same way by all these experimenters? And if it has, what is the reason for this reaction time - is it a transport lag, slowing factor, some of both? Without a model, how can they even tell what the reason for the reaction time might be?

The point is that most of the data presented in these papers is probably useless because it is not collected in the context of a working control model. There are lags and slowing factors in control systems so people are bound to find response latencies when they apply sudden disturbances to things people are (or are expected to be) controlling; the observed results in these studies (16 ms, 150 ms) may look very scientific and all but they are almost certainly useless for modelling behavior. And there is no question that measures of "response latency" that are collected in this way (with no understanding of the behavior of closed loop systems) say nothing about what variables people can and cannot control. Using these reaction times as a basis for showing the limitations of feedback control is just silly -- and an impediment to real research on control. This is what the article should be about.

Best Rick

Date: Thu Jan 28, 1993 11:27 pm PST
Subject: feedback too slow (summary)

[Avery Andrews 930129.1823] Rick Marken (930128.2200)

> Before starting the article (or whatever it is) here, why not spend a moment explaining what the hell people might mean by the idea that "feedback is too slow".

Because I suspect that most of the people who repeat this slogan don't mean anything by it, but are just repeating a formula they've picked up in order to avoid doing some work. It's useless to try to analyze every single way somebody might get into this frame of mind - what is useful is a bit of prose showing that even from the conventional viewpoint you can't just assert this.

> And there is no question that measures of "response latency" that are collected in this way (with no understanding of the behavior of closed loop systems) say nothing about what variables people can and cannot control. Using these reaction times as a basis for showing the limitations of feedback control is just silly -- and an impediment to real research on control. This is what the article should be about.

Well, I'm just not game to go around claiming that Houk & Rymer, or P.M.H. Rack, or Abbs & Winstein don't know what they're doing. If you are, go ahead.

Avery.Andrews@anu.edu.au

Date: Thu Jan 28, 1993 11:30 pm PST
Subject: Adams (1971)

[Avery.Andrews 930129.1700]

I started looking into Adams 1971, & it looks like it will be a real motherlode for the devils bibliography. Exhibit 1:

"There is a reference that specifies the desired value for the system, and the output of the system is fed back and compared to the reference for error-detection, and, if necessary, corrected. The automatic home furnace is a common example. The thermostat setting is the desired value, and the heat output of the furnace is fed back and compared against this reference" (Adams, J.A. 'Closed Loop Theory of Motor Learning', JMN,m3:11-150; p. 116

Here the blunder is that what the thermostat is measuring is the actual output of the furnace, as opposed to the result of the furnace output & all other influences on the air temperature in the immediate vicinity of the thermometer. This mistake is probably due to the typical Wiener-style and chemical engineering diagrams of feedback systems (like the ones at the beginning of Houk and Rymer 1981), where you have a box labelled 'controlled system' with the comparator, effectors, etc. outside of this box (maybe this is where Kugler, Turvey et.al. get their strange ideas about control theory from).

This blunder cross-fertilizes with another, the apparently classic distinction between exteroception and proprioception, which figures in a quote on the next page:

"For James, feedback acts as stimuli, and has no more status than an exteroceptive stimulus which starts the sequence, like a light on a display."

Of course, James was just talking about response-chaining, but he seems to have been closer to the right idea nonetheless.

So maybe a point to emphasize when talking to psychologists is that in PCT there is no 'proprioception' or 'exteroception', but just 'perception' (this actually is rather Einsteinian - maybe I'm getting the point of what Martin was saying a few days ago). This point can be enhanced by observing that Schmidt (1982, 1988) discussing some problems with these notions of perception, cites with approval a proposal to introduce a third, blended term, 'exproprioception' (for 'movements of our body in relation to the environment').

Avery.Andrews@anu.edu.au

Date: Fri Jan 29, 1993 8:16 am PST
Subject: Slow feedback

[From Bill Powers (930128.2200)] Avery Andrews (930129.1000) --

You've made a start on a formidable paper. I hope that others will join with you in bringing it to completion. I think it will be known in the future as a fundamental work in the field of PCT.

I would like to clear up one point that is still unclear, which is control of fast movements, or rather the obvious lack of it:

> From this it seems clear that feedback will not be very useful in controlling certain high-speed movements, such as the left jab of Muhammad Ali, clocked at 40ms.

Imagine a control system that has a time-constant of 40 milliseconds (we don't actually have to imagine it; the Little Man models it). This control system can control the position of an arm, so that one or more perceived joint angles

follows a smoothly varying reference signal, moving the arm under complete control at all times.

But now take this very same control system, and instead of giving it a reference signal that passes smoothly from one magnitude to another, make the reference signal jump instantaneously -- in, say ten milliseconds -- from a frequency of 200 impulses per second to a frequency of 500 impulses per second, with no passage through intermediate frequencies.

The arm will move from the initial position corresponding to 200 impulses per second to the final position corresponding to 500 impulses per second. If the control system is critically damped, it will do so in a single swift move that goes to the new position in an exponential approach. With a time constant of 40 milliseconds, it will go 63% of the way to the new position in 40 milliseconds, 86% of the way in 80 milliseconds, and 95% of the way in 120 milliseconds. This represents the fastest speed at which the control system can correct a suddenly-appearing error of any magnitude, large or small.

During this maximum-speed movement, there is naturally no control. The control system is already producing the largest output it can consistent with stopping in the new position. Any disturbance that came and went during the approach would simply cause a deviation from the nominal path. So, paradoxically it may seem, the control system's control action is uncontrolled after a step-change in the reference signal -- although the final position is as controlled as ever.

This is simply the nature of control. As the speed of movement increases, due to more and more abrupt changes in the reference signal, the error signal becomes larger and larger (producing faster and faster movement) until the change becomes a true step- change, at which point the movement will reach its maximum speed. The resistance to disturbances that appear during the movement will be, at low speeds, essentially the same as for static reference signals. As the rate of change of the reference signal increases, the resistance to disturbance during the transition becomes smaller and smaller, until in the limit it vanishes. There is therefore no inconsistency between saying that slow movements are controlled while the fastest ones are not. There is no need to posit one organization for slow movements and another for fast ones. The same control system, with the same parameters, explains the behavior we see over the whole range.

There is another side to this story. One common idea about the hierarchy of control is that within it, goals are set and then the control systems alter perceptions to match them. But this appearance, I think, is misleading. On the time scale appropriate to any level of control, behavior is not a process of error correction. On that time scale, errors never become large. Instead, as reference signals vary, perceptions simply follow them. The changes we see reflect changes in reference signals, not the process of error correction. On the appropriate time scale, which has been called the "specious present," error correction takes no time.

Best to all, Bill P.

Date: Fri Jan 29, 1993 10:25 am PST
Subject: What is feed-back too slow for?

[From Oded Maler (930129)] Rick Marken (930128.2200) Avery.Andrews (930129.1000)

>> People who make generic claims that feedback is too slow are probably (a) thinking of highly practiced rapid movements (b) thinking of the 200-150 ms reaction time for visually presented stimuli.

> Before starting the article (or whatever it is) here, why not spend a moment explaining what the hell people might mean by the idea that "feedback is too slow". I think the whole concept is ridiculous because it is based on a sequential state, cause effect concept of a control loop -- one which leaves out time (and, hence, a large hunk of reality). How can feedback be "too slow" in a closed loop where feedback is present (as

a perception) at the same time that the cause of that feedback (error) is present. There are phase relationships between the continuous variables in the loop --the result of transport lags and slowing factors around the loop. Lags that are too long or slowing factors that are too great can create instabilities in the loop (they can also create stabilities). But it would be necessary to know what is meant by "feedback too slow" and then to test it in a model before one could say whether "feedback too slow" (what ever that is) would be a problem. In fact, without a definition of what "feedback too slow" means in term of closed loop control, it is difficult to know what * is being measured in these reaction time experiments. If you ply an impulse disturbance to a controlled variable, there will be a change in the output variable; what is the "reaction time" here? The time to maximum output, the time until this output has some other effect (like pressing a button), the time until the derivative of the output is maximum, minimum? And when you decide what reaction time is, has it been measured in the same way by all these experimenters? And if it has, what is the reason for this reaction time - is it a transport lag, slowing factor, some of both? Without a model, how can they even tell what the reason for the reaction time might be? etc.

I think this exchange clarifies some important points and shows which parts of the elephant's body (to use the by-now-classical metaphor) are observed by "blind" non-PCTers and which by "blind" PCTer.

The answer to the question "what is feed-back slow for" you must invoke some "objective" performance criterion independent of the internal perceptual coordinates of the acting individual. You must assume that "playing a piano trill correctly at some speed" or "knocking out a boxing champion" has some more or less agreed-upon meaning. Then you can build a mathematical model of that act and the component involved (muscles, nerves and their reaction time) and show WITHOUT USING THE CONTROL MODEL that it is impossible for information to travel and affect the muscle at the time scale between the initiation of the action and its outcome. The nature of these impossibility/lower-bound arguments is that they consider ideal situations and thus apply as well to the "correct situation". By showing that an ideal pianist or boxer with ideal "objective sensors" and "objective effectors" cannot achieve something because of timing constraints you show a-forteriori (?) the a realistic (i.e. PCT-based) pianist/boxer with the same timing constraints cannot do it either. It is the same like proving, based upon bio-chemical and physical reasoning that no human is capable of, say jumping above 10m. This argument is true, regardless of whether he is commanding the muscles via hierarchical servoing, inverse-dynamics calculations or coin tossing. (I think that some of Martin's attempts explain information-theoretic constraints were along a similar line, and maybe he was right in the statement he made back then concerning the "real" understanding of PCT :-)

The emphasis of the important PCT insight that within the individual "it's all perception" in contrast to the naive objectivism of, say, cognitive psychology, should not be exaggated into a solipsist neglect of the external environment. The question of how and under what conditions people can achieve certain "objective" performance, in other words, what guarantees that a system organized in a certain way survives ("objectively") in a given environment, deserves more attention and better answers than "otherwise, reorganization will continue".

> The point is that most of the data presented in these papers is probably useless because it is not collected in the context of a working control model. There are lags and slowing factors in control systems so people are bound to find response latencies when they apply sudden disturbances to things people are (or are expected to be) controlling; the observed results in these studies (16 ms, 150 ms) may look very scientific and all but they are almost certainly useless for modelling behavior. And there is no question that measures of "response latency" that are collected in this way (with no understanding of the behavior of closed loop systems) say nothing about what variables people can and cannot control. Using these reaction times as a basis for showing the limitations of feedback control is just silly -- and an impediment to real research on control.

Maybe Rick is right about that, but still some neurophysiological evidence about basic properties of nerves and muscles can replace this data and prove the uselessness of feedback for certain kinds of actions.

--Oded

Date: Fri Jan 29, 1993 10:27 am PST
Subject: slow feedback

[Avery Andrews 930130.0420] Bill Powers (930128.2200)

I think I've taken the too-slow issue as far as I can without learning a lot more fundamental stuff. & my sense of the subject is that to go much further you'd probably have to do real research on specific types of movements. But if the dumb objections that people have to PCT can be sorted out, I think the trills & jabs will pretty much take care of themselves. & I hope my little piece is sufficient to deal with the ignorant forms of the too slow argument. My perception is that the next issues to look at are (a) pattern-generators (b) the idea that the output of the effectors is what is controlled (as in the Jack Adams quote, and Abbs & Winstein's 'technical' definition of feedback. This is sort of like what you call the 'objectification blunder' in QAPR, but I seem to want to call it the 'output blunder'.

I think I'll have to pass on saying more about language for the moment - it really is a much more difficult collection of issues.

Avery.Andrews@anu.edu.au

Date: Fri Jan 29, 1993 3:01 pm PST
Subject: FB 2 slow; devil's bib

[From Bill Powers (930129.1300)] Rick Marken, Avery Andrews (930129)

Response latency is measured in a lot of ways, as Rick says. Most of them leave you with little idea of what the actual transport lag through the nervous system is. I saw something recently in which subjects indicated a response by saying "HA!" into a microphone. Apparently the experimenter thought this was such a simple thing to say that saying it required no time. I suspect that any linguist would be laughing by now, thinking of what a sonogram of "Ha!" looks like. Just imagine the diaphragm beginning to tense, the pressure building up and starting to leak through the throat, the hiss, and finally the "Ah" sound building up to a measurable level. By the time that sound starts, the moment that the nervous-system output actually began is fading into the distant past.

Or consider indicating a response by pressing down on a key, or releasing a key. Depending on the relative sensitivity of the shoulder-muscle systems, the biceps and triceps, and the forearm muscles that operate the fingers (not to mention the strength of the spring under the key), the first tendency of the finger on the key to move might be either in the right or the wrong direction. If the biceps/triceps respond the most, initially, the dynamics of the arm will make the finger press down harder instead of releasing, or rise further off the key instead of depressing. By adjusting parameters you can make the Little Man model do these things quite clearly, to varying degrees. So an unknown part of the "response latency" consists of the dynamics of arm segment movements under angular acceleration.

And of course as Rick said, where do you place the threshold for detecting presence of absence of a response? At the 10% point? 50%? 90%? Just above the noise level? The inflection point in the movement? The point of maximum velocity?

When Bob Clark and I measured reaction times for mechanical disturbances of human arms, we used an electromyograph so we could pick up the moment when nerve impulses reached the muscles; we ignored the actual mechanical movements occurring after that. To my mind that is the only meaningful way to talk about a reaction time.

Heck, I've seen experiments with rats in mazes in which "response latency" was defined as the time it took the rat to get from the photocell at the entrance to the maze to the photocell at the entrance to the box where the cheese was.

Avery has it right: "Feedback is too slow" is a slogan, repeated because someone else said it in an authoritative manner.

Rick's question is highly germane: too slow for what? If feedback is too slow for control within 40 milliseconds, it's too slow, even if it's present. This is true in a model and also in the real system. The feedback pathways aren't surgically removed just before a 40-millisecond disturbance occurs. They just don't do much good. Look at the patellar reflex (knee-jerk). The rubber hammer puts in an impulse stretch of the tendon. The control system tries to correct the error, but it's much too late; the disturbance is gone. So the leg kicks upward, trying to oppose a disturbance that's not there any more. Feedback was too slow. So what? It was still a control system.

In the discussions of fast and slow movements, there seems to be an idea floating around that the nature of the physical system changes depending on the speed of the movement. This can't be true. Either you have a system hooked up as a control system, or you don't. If the control system isn't present for fast movements, it isn't present for slow movements, either. If a feedback connection is present for slow movements, it's still there during fast movements even if it's responding too slowly to do any good.

It doesn't matter whether you practice a movement a lot or are doing it for the first time. The parameters of control may slowly improve with practice, but you can never get to the point where you can dispense with the feedback. In fact, the better you get, the more important the feedback becomes in assuring an accurate action despite the speed. No matter how good the control gets, on the other hand, you can always present the system with a step- change in reference signal, or a disturbance in the form of a brief enough impulse, so the system is asked to perform beyond its capacities. In that case there's no control during the transition. But nothing about the control system has physically or functionally changed. If you took away the feedback, the movement wouldn't end up in the right place.

I hate to say it, but such statements about fast and slow or practice and unpracticed movements simply show the lack of a working model in the background.

Oded Maler (930129) --

> The answer to the question "what is feed-back slow for" you must invoke some "objective" performance criterion independent of the internal perceptual coordinates of the acting individual.

Feedback is too slow to permit a person to catch a passing bullet. So what? Arbitrary objective criteria don't have anything to do with the interior design of the behaving system, which behaves to achieve its own aims, not those of an external observer. The control systems in the human body are exactly fast enough to permit the kind of behavior that human beings are able to accomplish. They are not fast enough to accomplish ends that human beings can't accomplish, even though an artificial control system with faster feedback might be able to do them.

> You must assume that "playing a piano trill correctly at some speed" or "knocking out a boxing champion" has some more or less agreed-upon meaning.

This won't help to answer the question of whether feedback is too slow to explain any particular instance of behavior. I certainly can't play a piano trill "correctly" in terms of objective criteria that might apply to my friend Sam Randlett, who teaches concert pianists. Yet my feedback is exactly fast enough to account for the speed with which I CAN play a trill (Sam would fall asleep waiting for the next note). I can't knock out a boxing champion, or a

drunk paraplegic, yet my feedback control systems are just exactly fast enough to make my fist move as fast as it actually moves.

- > Then you can build a mathematical model of that act and the component involved (muscles, nerves and their reaction time) and show WITHOUT USING THE CONTROL MODEL that it is impossible for information to travel and affect the muscle at the time scale between the initiation of the action and its outcome.

Again this misses the point. With the Little Man arm model, I can find the maximum speed of movement at which it is impossible for feedback signals to make any change in the movement between its initiation and its outcome. That speed is one in which a movement occurs in response to a step-change in the reference signal, and its time constant is about 40 milliseconds, just like Mohammed Ali. The initiation of the act consists of an instantaneous change in the reference signal. The control system begins with maximum possible error, which then reduces asymptotically to zero with a 40-millisecond time constant. This is simply the fastest speed at which the error can be reduced.

The determination of the delays is quite independent of the model; it's due to neural transport lags and to the leaky-integrator form of the muscle response. The feedback model incorporates these factors, and when the parameters are adjusted for the nicest and fastest error correction possible under those conditions, you get a 40-millisecond time constant of error correction. The feedback model is acting as fast as physically possible.

If you didn't have the feedback present, the model wouldn't be able to execute the same movement anywhere nearly as fast. The feedback model starts, immediately after the step-change in reference signal, with an error signal that would move the arm something like 10 times as far as the actual distance to the reference or intended end-point. This produces a tremendous acceleration, on the order of 20 gravities in linear terms. But as soon as the arm begins to move, the error signal begins to drop (about 9 milliseconds later, actually). Now the effective target location is not so far beyond the intended end-point. This process continues, the effective end-point coming back inward toward the intended end point while the arm moves outward toward the intended end-point. Before the arm reaches the endpoint, the rate feedback actually moves the effective target point to the negative side of the intended end-point, decelerating the arm. All this happens automatically with no particular computational difficulties, and the arm comes to rest at exactly the right position, 100 or 150 milliseconds after the initiation.

If you wanted an open-loop system to bring an arm to an endpoint after a step-change in the initiating signal, you would have to use a much smaller signal, one tenth as large, to avoid overshooting the intended end-point. As a result, the initial acceleration would be much less, and the movement would take far longer.

Actually, the problems would be much worse than that. You couldn't actually use a step-change in the driving signal, because without the feedback you'd have a mass on a spring with very inadequate damping. You would need a full blown motor program that would apply a complex waveform to the muscle, to prevent oscillations. Of course you could then use a larger driving signal, and in fact supply the same driving signal that would be observed during the operation of the feedback system. You could then achieve equal speed -- but at what cost!

- > By showing that an ideal pianist or boxer with ideal "objective sensors" and "objective effectors" cannot achieve something because of timing constraints you show a-priori (?) that a realistic (i.e. PCT-based) pianist/boxer with the same timing constraints cannot do it either.

This is more like my point. In fact, real boxer/pianists come very close to achieving the theoretical limits of performance. Models, of course, can always work better than real people, because we can give them more favorable properties. But if we match masses, delays, and time-constants, we can then adjust the model to reproduce the human behavior reasonably well. The Little Man, given very strong muscles, can in fact move the fingertip from one point to another with a 40-millisecond time constant, about the same as Ali or Joe

Louis. But not a lot faster, given the delays and time-constants. Trying to push for still faster movements takes the system to the edge of instability. I presume that's why the real system doesn't go any faster, even for the most practiced practitioners.

- > The emphasis of the important PCT insight that within the individual "it's all perception" in contrast to the naive objectivism of, say, cognitive psychology, should not be exaggerated into a solipsist neglect of the external environment.

I think I'm safe from that accusation. I probably spend 90 percent of the time required to produce a model like the Little Man in constructing a realistic physical model of the environment with which the system interacts. The actual control-system model is trivially simple in comparison. Even in our simple tracking models, we include more in the environment than most other modelers do -- for example, disturbances that directly affect the outcome, and often nonlinearities and changes in parameters.

- > The question of how and under what conditions people can achieve certain "objective" performance, in other words, what guarantees that a system organized in a certain way survives ("objectively") in a given environment, deserves more attention and better answers than "otherwise, reorganization will continue".

It certainly does, and we have never used reorganization to make up for lack of specificity in a model. We match models to performance by adjusting parameters, thus answering the question as to what guarantees success (equal to that of the human) in the task. "Survival" isn't so much of a concern; so far all of our experimental subjects have survived. Most of our talk about reorganization is by way of speculating about how we would go about including it in a model, when we finally get around to doing that.

Of course when we're just doing shirtsleeve conjecturing or Big Picture fantasizing, anything goes. It's all reorganization. Heck, maybe it's all chocolate syrup.

- > ... but still some neurophysiological evidence about basic properties of nerves and muscles can replace this data and prove the uselessness of feedback for certain kinds of actions.

I suspect that it would also prove the uselessness of supposing that such actions can actually occur. Given such data, which I certainly used a good deal of in designing the Little Man, we can find out how good a control system can be built around those properties, and that is in fact what I did. I learned enough to know that I am not going to waste my time trying to devise a central pattern generator that could reproduce the same performance within even a factor of ten worse. Anyone else is welcome to try. I wish someone would. Then we could drop this whole stupid subject of open-loop behavior.

Avery Andrews (930130.0420) --

- > My perception is that the next issues to look at are (a) pattern-generators (b) the idea that the output of the effectors is what is controlled (as in the Jack Adams quote, and Abbs & Winstein's 'technical' definition of feedback. This is sort of like what you call the 'objectification blunder' in QAPR, but I seem to want to call it the 'output blunder'.

Yes. I really appreciate all the time, research, and thought you've given to PCT in your off-season, so while I agree that these are important subjects I don't mean to imply that you're expected to go on doing all the work. Linguistics calls, I know.

Pattern generators aren't a big problem, actually. It's true that a pattern generator can always be devised to duplicate the performance of a control system in a specific situation, at least over a brief period of time. But when you consider the data that must be available to the generator, and the accuracy with which calculations must be carried out, and the fact that

physiological machinery like nerves and muscles change with use, and that there are independent and unpredictable disturbances that can act directly on the outcome and that can't be sensed or anticipated, and that behaviors can last for hours and days, each move beginning where the last one left off -- the whole idea begins to look highly impractical. The simplest behavior requires a supercomputer to carry it out.

The pattern generator can be part of a control system, as you've noted, but that's not the usual idea. The normal open-loop pattern generator is actually ruled out because the output blunder must be ruled out. Pattern generators rely on output devices that behave uniformly and with infinite precision, and on total absence of disturbances that enter the output chain after the effectors. The only way that I know of to deal with effectors of variable properties and independent disturbances downstream from the effector is through feedback control. There's just nothing else that will work. So the output blunder is really the most important one.

This blunder isn't quite so easy to find in the literature, not because it isn't there but because it's like a black hole in the middle of an explanation: invisible unless you realize that something ought to be there. Consider the idea of an "orienting response." This is a response that orients the body or head toward a stimulus like a sound. But what kind of response could make the angle of the head end up in exactly the right direction regardless of the orientation of the body or the location of the stimulus? On one occasion the head might have to turn 2 degrees on the neck; on another, 90 degrees; on still another, 40 degrees the other way. How can a response be so specifically what is objectively required, despite large differences in initial conditions? The whole problem is wrapped up and concealed in that little word, "toward."

There are lots of cases in which responses are simply named after the effect that the organism's actions produce, thus skipping the entire question of how the nervous system can cause the same remote consequence to occur over and over under varying conditions. Just by calling all outcomes of actions "responses" you can leap right over the black hole without giving it a glance. The shoelace-tying response. The problem-solving response. The balancing response. The verbal response. The tracking-the-target response. The putting-it-on-the-Visa-card response. The car-steering response. You can take it for granted that any time the word "response" is used, it's really referring to an outcome, not the action that happens to be required this time to create that outcome.

The output blunder is hard to detect because hardly anyone even realizes that there's a problem here. It's not discussed one way or the other. What's wrong with saying that you respond to a question by stating the answer?

Bill P.

Date: Fri Jan 29, 1993 8:58 pm PST
Subject: Powers on (not so) Slow feedback

[from Gary Cziko 930130.0430 GMT] Bill Powers (930128.2200) elucidated:

- > Imagine a control system that has a time-constant of 40 milliseconds (we don't actually have to imagine it; the Little Man models it). This control system can control the position of an arm, so that one or more perceived joint angles follows a smoothly varying reference signal, moving the arm under complete control at all times.
- > But now take this very same control system, and instead of giving it a reference signal that passes smoothly from one magnitude to another, make the reference signal jump instantaneously -- in, say ten milliseconds -- from a frequency of 200 impulses per second to a frequency of 500 impulses per second, with no passage through intermediate frequencies. .
.
- > During this maximum-speed movement, there is naturally no control. The control system is already producing the largest output it can consistent

with stopping in the new position. Any disturbance that came and went during the approach would simply cause a deviation from the nominal path. So, paradoxically it may seem, the control system's control action is uncontrolled after a step-change in the reference signal -- although the final position is as controlled as ever.

Wouldn't research to show this be quite easy to do? Simply apply disturbances (big rubber band like I used at Durango) during an action that was performed at various speeds. If the HPCT model is right, then for slower actions the path of the limb will be controlled as well as its final position. At higher speeds, the path will be less well-controlled but the final position will still be. While at the highest speeds, only the final position will be controlled.

Might it take only a big rubber band and a well-place video camera to do this research? (A more sophisticated way of introducing disturbances would be a knee or elbow brace with the physical resistance of the joint manipulable by remote control--in your spare time, Bill).

But for some reason I have the intuition that at the very highest speeds even the final position will not be well-controlled, at least not without some patch-up correction at the end of the movement. But I want my intuitions to be wrong here.

Looks like a good project for a master's thesis.--Gary

P.S. It's possible that my College of Education might in the near future inherit our campus's Department of Kinesiology. Could be fun.

Gary Cziko

Date: Sat Jan 30, 1993 12:41 pm PST
Subject: feedback too slow -- stack

[From Rick Marken (930130.1200)]

Avery Andrews (930129.1823)

> Well, I'm just not game to go around claiming that Houk & Rymer, or P.M.H. Rack, or Abbs & Winstein don't know what they're doing. If you are, go ahead.

I'm certainly willing to go around claiming (if it is true) that they don't know doggie breath about control theory. I'm one of those protestant type scientists that Bill just mentioned -- I've just got no respect for the priesthood (or the rabbinate, for that matter).

Bill has gone to some lengths to explain how "reaction time" might fit into the behavior of a control system. I plan to make a HyperCard stack that will illustrate the "feedback too slow" issues that are involved when you are actually dealing with a control system. The stack will be based on a single control system; the user should be able to vary the time constant and transport lag of the system; the user should also be able to apply an impulse or step disturbance to the controlled variable. The user should also be able to select a fixed reference input or one that shifts between two values at a selected time rate. Any other suggestions for such a stack? The idea is to show what would happen in a typical "reaction time" experiment if the subject were a control system. Bill has explained this very clearly but maybe it would help to actually see it dynamically (I know this can be done with the Little Man Demo but the stack might help isolate and clarify the problems with the idea that "feedback is too slow" in a control system). If I actually succeed in building such a stack maybe Avery could use the results of experiments on the stack in his critique of the "feedback too slow" myth.

Best Rick

Date: Sat Jan 30, 1993 6:26 pm PST
Subject: slow feedback, output error, etc.

[Avery Andrews 930130.1100 (mostly Bill Powers (930129.1300))

Feedback too slow:

My understanding was that the famous left jab *takes* 40ms from start to finish. If this is true, it clearly can't be done by tracking a step-change in a position reference-level. I don't think it's true that you necessarily either have a control system (all the time) or not - just run the positional information through an inhibitable interneuron on its way to the comparator, & you can switch off the feedback by turning on the inhibition. & it doesn't disturb me that you would need a pretty fancy circuit element to generate the commands for these movements - after all, it take a tremendous amount of practice to acquire such things. And there's all this talk about 'refractory periods' and the like which could be evidence for various kinds of inhibitions being switched on and off.

& I'd agree that 'for what' is the right thing to say if someone says 'feedback is too slow' & you want to respond brusquely, but I thought my slightly subtler way of saying the same thing would be more useful. After all, we don't want PCT to be taken up by people who just believe what we say, but by people with some capacity to draw their own conclusions from whatever happens to be in front of them. You don't have to say it all at once, and often it's better not to, I think.

Pattern Generators:

My line on them is that they're just irrelevant to the question of what the role of feedback is. People thought they were a problem because they confused feedback with peripherally mediated response chaining. What CPGs normally do is specify perceptual reference level contours. From what little I've read about gait control in insects, it seems to involve a complex mix (different from species to species) of CPGs, response-chaining effects, and actual control. I expect Adams (1971) to be a very informative source on this subject, but we shall see.

Output Blunder:

We're using the term in two different ways. Your 'output blunder' is the belief that there are effectors that just produce the results intended. Mine is a mistaken idea about what feedback means, the assumption that feedback means monitoring something 'directly' produced by the effectors (an incoherent notion, I would say, but people really do seem to believe in it). I think this is a critical mistake, which, for example, gives us the mumbo-jumbo of 'coordinative structures' rather than feedback control of relationships. There may be a better term for the blunder, but it's got to be a snappy one, since it's so fundamental. Maybe I'm just getting carried away by enthusiasm, but I'll go so far as to suggest that it may be the *most important* of the blunders.

Kugler et. al.:

One thing that would be extremely useful is for somebody who really knew their differential equations to look into Turvey, Kugler et. al. I can't really be sure whether they are saying profound and useful things (while being thoroughly dishonest in their portrayal of what other people are doing), or whether they are just making straightforward things look deep, dark and difficult by putting them in the most abstract mathematical setting they can find (e.g. is 'limit oscillator' just another way of talking about an oscillator whose output is a perceptual reference level). My nose tells me that there's more than a little of the latter in their story, but my math is too weak to be sure.

Linguistics:

One of the reasons the linguistics discussions are so convoluted is that people want to talk about the subject before having learned much of anything

about it, and it isn't plausible for Bruce or I to try to run a basic linguistics course on CSGNet. I don't have any problem with the stuff that Ray Allis said, or that various other people said later, but it doesn't have much to do with what linguists are talking about when they talk about something like 'the structure of the lexicon'.

Linguistics is mostly about patterns.

Unfortunately, most of the verbal patter that goes along with linguistics is nonsense, so that critically minded people without an aptitude for perceiving the patterns get bogged down in the nonsense, and never get any sense of what is going on (generative grammarians are perfectly happy with the idea of distributed representation of roots in the lexicon, for example, especially because the properties of 'strong verb tense stem formation (dig vs. dug, sing vs. sang vs. sung) are highly consistent with what you'd expect from distributed representations). If I thought that sorting out linguistics was a high priority for PCT, I'd spend more time trying to make it look sensible, but I just don't think it's the best use of my time. Something a lot more promising in the intermediate term would be PCT phonetics and then phonology, since some of the work on 'distributed compensatory responses' is actually about articulatory phonetics. & I've never seen an introductory phonetics book with a coherent discussion of the difference between happening to produce a sound and successfully controlling for producing it under a variety of circumstances.

Avery.Andrews@anu.edu.au

Date: Sat Jan 30, 1993 6:26 pm PST
Subject: slow feedback

[Avery Andrews 920130.1245] Rick Marken (930130.1200)

> I'm certainly willing to go around claiming (if it is true) that they don't know doggie breath about control theory. I'm one of those protestant type scientists that Bill just mentioned -- I've just got no respect for the priesthood (or the rabbinate, for that matter).

Neither do I, but in the preliminary stage where you're basically just trying to get people to take you seriously, it's important to start with the most plausible sounding claims, & I doubt that claiming that Rack, Winstein al. are ignorant about feedback is one of these. It seems to me that there are many *much* softer and juicier targets around. For example, I found a passage by Feldman and Berkenblit (JMB:20:369-373) where they clearly show that they think that the only alternative to sudden reset of an equilibrium point is to compute a series of intermediate EPs and then present them to the motor system at a high rate.

I'd be interested in the Hypercard stack when its done - I've actually got a Mac on my desk, but I've been ignoring it since its not networked. This is supposed to change soon, & when it does I'll pay more attention to it (and probably try my hand at Think C programming). I've got a half-finished joint simulator, but its treatment of muscles isn't any good yet, so I can't really demonstrate anything with it.

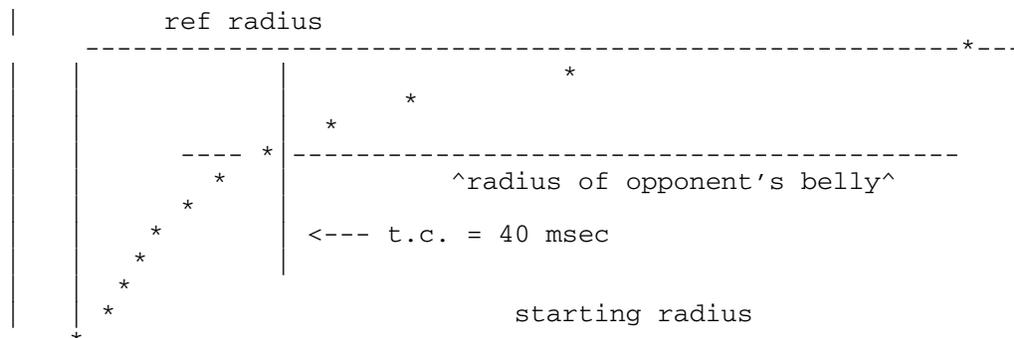
Avery.Andrews@anu.edu.au

Date: Sun Jan 31, 1993 9:02 am PST
Subject: FB 2 Slow; Misc neurology

[From Bill Powers (930131.0900)] Avery Andrews (930130.1100) --

> My understanding was that the famous left jab *takes* 40ms from start to finish. If this is true, it clearly can't be done by tracking a step-change in a position reference-level.

Let's plot radius of fist from shoulder as a function of time:



The time constant, in a linear system, is independent of the amplitude of the movement. In the real system, which is somewhat nonlinear, this is not quite true.

I have drawn the above diagram so that the reference position is set beyond the point of contact just enough so the fist reaches the opponent's gut one time-constant after the start of the movement, or 40 milliseconds.

Note that if Ali set the reference position AT the point of contact, the fist would touch the surface gently at zero velocity after 120 to 160 milliseconds. Prize fighters are taught to aim to hit a point well beyond the actual target. They develop large muscles not to push harder, but to accelerate the fist faster under the impetus of the large initial error signal that is caused by the step-change in reference signal.

> I don't think it's true that you necessarily either have a control system (all the time) or not - just run the positional information through an inhibitable interneuron on its way to the comparator, & you can switch off the feedback by turning on the inhibition. & it doesn't disturb me that you would need a pretty fancy circuit element to generate the commands for these movements - after all, it take a tremendous amount of practice to acquire such things.

It's not fair to posit undiscovered connections behaving in unobserved ways to explain a phenomenon that's adequately explained by a much simpler system using known connections behaving in known ways. And if it's speed you're after, why add more stages of neural processing?

As I've tried to point out, switching off the feedback would remove a great deal of the damping in the arm system, so instead of getting a graph like the above one you would get an underdamped oscillation of position that might take four or five diminishing oscillations to come approximately to rest.

The muscle in the above situation is given an initial driving signal exactly equal to the amount of step-change in the reference signal (because the negative feedback takes time to build up). In the first 10 or 20 milliseconds, there is practically no difference between the behavior of the control system and the behavior we would see if the feedback paths were cut. The difference is the one between the curve and a straight line having the same initial slope. The initial slope represents the acceleration of the arm's mass under a sudden maximal muscle force.

When you get rid of the feedback, you might gain a very slight amount of speed, the difference between the exponential curve and the straight-line extension of the initial slope. What you would lose would be control of the end-point -- for example, the ability to stop the arm quickly and bring the fist back after a miss.

Hang a small weight like a cup from two or three rubber bands strung together in series and see how fast you can control the height of the weight above a table with no feedback -- just by moving the end of the rubber band to a precalculated position. That's how an arm behaves with no feedback.

- > And there's all this talk about 'refractory periods' and the like which could be evidence for various kinds of inhibitions being switched on and off.

The "refractory period" is the time it takes for a neuron to recover after firing before the next impulse can be generated. It is on the order of one millisecond, and is shorter if the neuron is driven by a high-frequency input signal. The absolute refractory period -- the interval below which no amount of signal will fire the neuron -- sets the absolute maximum frequency of firing of the neuron. It is not evidence of inhibitions being switched on and off.

- > People thought they were a problem because they confused feedback with peripherally mediated response chaining.

You keep saying "response chaining." Doesn't this convey an image of one complete response occurring, setting off another stimulus and another response? This is the image we're trying to erase, because it implies that while the response is in process, the stimulus can't also be changing. In the real system, stimuli and responses are continuous, not alternating.

- > From what little I've read about gait control in insects, it seems to involve a complex mix (different from species to species) of CPGs, response-chaining effects, and actual control.

Correction: gait control does not involve such things. It is SAID to involve such things, on the basis of the only model that people have been able to think of. People who design models of neural networks have a tendency to make the neurons into on-off units, like digital circuits. They know only a few of the actual connections, and those that they do know are treated as binary elements. In the cockroach there are all kinds of position sensors and rate-of-change sensors, which are simply ignored by modelers like Randall Beer, because they don't understand control systems or, for that matter, continuous analog systems.

- > We're using the term in two different ways. Your 'output blunder' is the belief that there are effectors that just produce the results intended. Mine is a mistaken idea about what feedback means, the assumption that feedback means monitoring something 'directly' produced by the effectors (an incoherent notion, I would say, but people really do seem to believe in it).

Right you are. There are really two blunder here: one is the assumption that regular consequences can be produced by generating regular motor outputs. The other is the assumption that the controlled variable is the output quantity instead of the input quantity. Maybe we could distinguish them by calling one the output-regularity blunder and the other the output- control blunder.

- > One thing that would be extremely useful is for somebody who really knew their differential equations to look into Turvey, Kugler et. al. I can't really be sure whether they are saying profound and useful things (while being thoroughly dishonest in their portrayal of what other people are doing), or whether they are just making straightforward things look deep, dark and difficult by putting them in the most abstract mathematical setting they can find ...

I already posted a few comments on Fowler and Turvey's mathematical expertise. I think what happens in most of these cases is that the psychologist teams up with a mathematician; the mathematician just produces the equations that the psychologist seems to want, not questioning the rationale, while the psychologist accepts whatever the mathematician turns out without understanding how the result depends on the premises. The result looks very impressive, doesn't it? However, from gobbledygook terms like 'limit oscillator' you can tell who is trying to snow whom.

Linguistics: I think I'll take a vacation on all that for now.

Best to all, Bill P.

Date: Sun Jan 31, 1993 2:34 pm PST
Subject: Re: Misc neurology

[Avery Andrews 930201.0932] Bill Powers (930131.0900)

Right, I just forgot about the decelerating effect of the opponents body. If Gary's kinesiologists can be convinced, we might start to get somewhere. Where do they stand on the 'motor-action' controversy?

> There are really two blunder here: one is the assumption that regular consequences can be produced by generating regular motor outputs.

But is this really a blunder? After all, if an 'output' is used as a reference level for a control system, you do get a regular result. It is also my impression that people have in fact pretty much abandoned the 'output regularity' blunder for most kinds of movements, at least. This process was the evisceration of the motor program theory, documented in the various publications of Schmidt. Once upon a time the idea was that the motor programs specified alpha-gamma efferents, e.g. real low level motor efference, then they realized that that wouldn't work, so there are both feedback and 'parameters', operating in some unspecified way (hence no content to the theory). I suspect it isn't worth harping on the output regularity blunder any more, tho it certainly would have been in the seventies.

Avery.Andrews@anu.edu.au

Date: Sun Jan 31, 1993 2:44 pm PST
Subject: Re: Ballistics vs. Feedback Demo

[Avery Andrews 930201] (Gary Cziko 930131.1705 GMT)

In the jab story I've been pushing, the fist would be propelled the whole way, so I don't think your demo makes the point. The throwing with a rubber band attached to your hand one, however, might be useable to very good effect, since it argues, I think, that the actual throwing skill as specified as perceptual reference levels, since otherwise there's no explanation for the fact that when the elastic is attached, accurate throwing continues to be possible (a classic motor program would fail).

If this kinesiology department you might absorb is full of frustrated motor programmers wondering what to do about Kugler, Turvey et. al., there might be a real opportunity there.

Avery.Andrews@anu.edu.au

Date: Sun Jan 31, 1993 4:01 pm PST
Subject: Left jab; strategy

[Avery Andrews 930201.1059] Bill Powers (930131.0900)

So, the jab might be driven thru a kinesthetic control system, but it also might be that people temporarily deafferent themselves at low levels (remember the Bizzi monkey arms), and the fact that it requires so much practice to acquire these moves makes anticipation, etc. much more plausible as a mechanism than it normally is (as Tom Bourbon just pointed out).

I think it's important not to spend too much energy on peripheral matters where PCT expectations might not be borne out -- if you insist that the jab is done via kinesthesia, and somebody proves that it isn't, then you've lost a lot of credit. If you say that it might after all be done by kinesthesia, then somebody maybe has a nice little hard-edged research project, and while it would be nice if the answer was yes, it would not be a big deal if it wasn't. It would be quite sufficient to get people to the point where they could investigate these issues competently - there's no need to tell them what the answers are going to be.

Avery.Andrews@anu.edu.au

Date: Sun Jan 31, 1993 7:06 pm PST
Subject: fowler & turvey 1978

[Avery Andrews 930201.1400]

I've now got a copy of Fowler & Turvey, & while it's obvious that they missed a lot in their treatment of PCT, there's also the question of whether anyone has written a reorganization demo that solves the problem they pose, or anything like it (the E. coli demo doesn't count, since you don't see an actual skill being acquired).

Avery.Andrews@anu.edu.au

Date: Sun Jan 31, 1993 7:54 pm PST
Subject: fowler & turvey 1978

[From Rick Marken (930131.2000)] Avery Andrews (930201.1400)

> I've now got a copy of Fowler & Turvey, ...there's also the question of whether anyone has written a reorganization demo that solves the problem they pose

I did. It's really easy. I mentioned the results of the simulation in my "Nature of Behavior" paper (first paper in Mind Readings). It's REALLY easy to write a PCT simulation of this control system; it is amazingly efficient.

Best Rick

Date: Sun Jan 31, 1993 10:41 pm PST
Subject: inverse dynamics, fowler&turvey

[Avery.Andrews 930102.1701] Gary Cziko 930201.0402 GMT

I'm not sure what people really think they mean by 'ballistic', but from what I've seen, preprogrammed acceleration and deceleration bursts seem to be the commonest 'model' (for which there is some evidence, in some studies involving yanking on a lever, which is sometimes fixed so that it doesn't move, but the bursts happen anyway (refereed to by Schmidt, & I just realized I forgot to write it down & had to take the book back to the library I got it from today)). Scare quotes around 'model' since all the stuff that Bill and Rick say about the absence of actual models in this area is definitely true.

As far as I know, you got kinematics, dynamics & inverse dynamics right. Inverse dynamics is figuring out what forces the actuators must produce to produce a given motion.

Its interesting seeing how far you can get into a subject like motor control by means of basic scholarship and a bit of computer hacking - surprisingly far, it seems to date. My professional excuse is that people sometimes speculate that 'motor programs' were the preadaptation to syntax, so there's a reason to investigate them.

(Rick Marken (930131.2000))

Good. On a second perusal of the material, it didn't look like it would be hard to do (and also somewhat deficient as a serious model of skill acquisition). These people do seem to have a bit of a deficit in understanding written English.

Avery.Andrews@anu.edu.au

Date: Mon Feb 01, 1993 12:15 pm PST
Subject: feedback too local

[Avery Andrews 930202.0630]

In the motor control literature I've run across statements to the effect that feedback is 'technically' restricted to monitoring immediate products of the effectors (such as, presumably, rpms on a driveshaft). Hence, if you are trying to close your lips and compensate for one being disturbed by moving the other further, this is 'technically' not feedback.

Do any of the engineers on the net recognize this as an actual doctrine from courses they took, or anywhere else? Or is it perhaps just an idea that psychologists picked up somehow?

Avery.Andrews@anu.edu.au

Date: Wed Feb 03, 1993 5:52 pm PST
Subject: devils, angels

[Avery.Andrews 930204.1224]

Today's find in the library is:

Whiting, H,T.A (ed) Human Motor Actions: Bernstein Reassessed, North Holland.

Bernstein is one of the culture heroes of Bizzi et. al. on the one hand, and Kugler, Turvey, et. al., on the other, and appears to have been a major Right Thinker, having, for example, a very nice feedback diagram (pg. 358, also pg. 130 of N. Bernstein 1976 The Coordination and Regulation of Movements, Pergamon press).

The devil's bib entry is Kugler and Turvey 'An Ecological Approach to Perception and Action' pg. 373-412, esp. pg. 391-392, where they criticize Bernstein's and everybody else's conception of feedback control on various grounds, including what appears to be the idea that it involves 'an orderly sequence of symbol strings (the representational format for the quantities and the commands)'. They appear to be making the event-based blunder and several others besides, and to be criticizing feedback control in general on the basis of somebody's proposal for a symbolic 'motor program' to control walking (due in its original form to MacKay, W.A. (1980) 'The motor program: back to the computer', Trends in Neuroscience 3:97-100). I'm getting the impression that part of their strategy is to criticize actual proposals on the basis of the most bungled derivatives of those proposals that they can find (a tactic we should be careful to avoid).

On the other hand, in the next section, they make what strikes me as a sensible argument that processes that may look like feedback control w.r.t. a set-point aren't necessarily so, and that respiration rate-stabilization in fact isn't.

Then there's an interesting-looking argument by G. Hinton arguing that it is in fact a good idea to precompute torques, and discussing various features of muscles that make this easier to do than you would think (for example, the viscosity properties of the force-velocity relations give you what is in effect instant feedback control over the velocity of a limb: if the velocity is less than expected, the force exerted by the muscle will be higher, so the velocity lag will be corrected. If Bill Powers and Greg Williams haven't thought about this article, they ought to.

Date: Thu Feb 04, 1993 9:40 am PST
Subject: control article

[From Rick Marken (930204.0900)]

Hans Blom (930204) --

- > William S. Levine, Gerald E. Loeb's paper 'The Neural Control of Limb Movement'
- > 'the study of biological systems should not be confined to testing whether their performance is compatible with control schemes invented to date but must include detailed examination of their inner workings to discover new types of control'.

What do they mean by 'new types of control'? There is only one type that I'm aware of -- maintenance of a perceptual variable at a specified reference level in the context of variable disturbances. Levine and Loeb give no evidence of understanding that it is a perceptual variable that is controlled (by whatever means -- bang/ bang, continuous output, lagged output, etc) and they seem to assume that they already know which biological variables are controlled; the only problem (they think) is to figure out the mechanism by which control is implemented. Levine and Loeb (didn't they have a run in with a young kid in Chicago some years back?) seem to have forgotten to mention step one in the study of biological control systems -- the test for the controlled variable. How can they compare the performance of a living system to know "control schemes" if they don't know what is the living system is controlling? Do they explain how they know what variables are controlled by the high jumper that they mention? If so, how do they know?

Sounds like another nice entry for the Devil's Bibliography.

Best Rick

Date: Thu Feb 04, 1993 3:34 pm PST
Subject: telephoning devils

[Avery Andrews 930205.0938] (Greg Williams 930204)

Well, my internal model of Real Professors tells me that publications are much more likely to have an effect than phone calls (e.g. some chance, rather than no chance at all). Partly because the target has some time to think about what has been said, and come up with a genuinely useful response (I think this is much more time-consuming than the conventional rules for spoken debate allow for), partly because they have a strong motive to respond (so as not to be humiliated in public), and partly because you then get a chance to make an impression on the milling crowd of uncommitteds, who are the guys you're supposed to win over in order to pull off a scientific revolution.

Avery.Andrews@anu.edu.au

Date: Wed Feb 17, 1993 7:10 pm PST
Subject: I'm a thermostat

[FROM: Dennis Delprato (930217)]

No restraints or sanity hearings needed, for....

"What's it like to be a thermostat (as opposed to being talked about being a thermostat)?"

Once, again I am reminded of the problem created when control systems are talked about from an outside perspective:

>Bill Powers (930217.1030)

> As a ... third-party observer you can say ['The signal in my home thermostat is about the temperature in my house']. As a thermostat, you could not say it.

How is it that despite the many depictions of heating and cooling systems, complete with thermostat, in today's literature, virtually everyone describes what's going on as control of output? I suggest someone prepare a kindly little essay (?) spelling out how easy it is to be deceived when one looks from the outside in and even does a bang-up job of describing what they observe. Point out how one gets a very different picture when one "takes the viewpoint of the thermostat." Seems like PCT is getting closer and closer to Stephenson's Q-methodological thinking that stresses how psychology ignores the person's point of view, instead imposing the observer's point of view on the person and calling it understanding the person.

Bill reminded me that even the thermostat "has a point of view," and this is what he (Bill) is concerned with.

Date: Tue Mar 09, 1993 5:46 am PST
Subject: ANN. REV. PSYCHOL. atricle

From Greg Williams (930309)

New reference to PCT and related work: Paul Karoly, "Mechanisms of Self-Regulation: A Systems View," ANNUAL REVIEW OF PSYCHOLOGY 44, 1993, 23-52. Even cites LIVING CONTROL SYSTEMS! Wow!! And claims that it was edited by R.S. Marken!!! Ouch!!!! Oh, well, I suppose Rick could use some academic brownie points. I surely don't need any. The bottom line on Karoly: lots of devil's bibliography quotes. For example (p. 30):

Based upon a century-old insight attributed to William James (cf Powers 1989), that humans are 'unique' in nature because they can produce consistent ends by variable means, a number of contemporary (post- 1960) models of dynamic self-regulation have been developed under the imprimatur of cognitive theory, control/systems science, cognitive social learning, or European action theory. All presume that on-line regulation is a dynamic process, continuous and holistic rather than linear, built upon the operation of feedback [now hang on] (knowledge of results) and feedforward (stand-produced disequilibrium) [say what?] , sensitivity to action-produced environmental changes, the accessibility of goal representations [?], and a capacity for the selective mobilization of energy [damn straight, if they're going to move around!], attention, and relational judgment.

And so it goes....

As ever (even as Pat and the kids STILL have the flu -- remarkable!),

Greg

Date: Thu Apr 15, 1993 6:33 am PST
Subject: THE SAME DAMN LIES

[FROM: Dennis Delprato (930415)]

I made the mistake of picking up the most recent issue of the Annual Review of Psychology (where we find the state-of-the-art of self-regulation put forth). Once again I am reminded (and not only by ONE chapter) of an episode J. R. Kantor related.

After he retired from Indiana Univ., he once spent some time at Johns Hopkins where he met with graduate classes. On one occasion, to help students distinguish between what he was saying and conventional views, he asked them to bring their text to class (turned out to be one of the classics in sensation--Geldard's). At the next meeting, he proceeded to point out unjustified fundamental assumptions and alternative ways of describing what is happening. The result was that the students said this was the first they

heard of an alternative to Geldard's accounts. They rejected Kantor's ideas out of hand and the overall outcome of the episode was "a pretty bad scene." Kantor goes on, "They were all close to their Ph.D. And what are they going to do, GO OUT AND TELL THE SAME DAMN LIES."

I imagine that anyone who truly appreciates what PCT is all about at the most general level, will time and time again have occasion to think back to Kantor's run-in with some of today's leaders and the teachers of those who are beginning to take over the strange discipline of psychology.

I can't help think of the now best-forgotten "arrogance" thread of the past month or so on CSG-L. But when I read statements of the sort mainstream psychologists are wont to make, "arrogance" comes to mind. Other descriptors are "ignorance," "illusory," and "delusory." What passes for the social sciences, in general, is an interesting group of disciplines. The major criterion for widespread acceptance of even the most far-fetched ideas is that they be presented in authoritative ways. So-called science is simply used as a conservative force to perpetuate tradition. I try to be "accepting" of divergent views, but will draw the line when the views are not presented as tentative and open to test, and instead come out as downright LIES.

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Date: Tue May 04, 1993 10:42 am PST
Subject: Draft for joint paper

[FROM Dennis Delprato (930503)]

I'd like to see an attempt made for BBS. I might be able to serve a useful function in the form of a critic who looks at the ms. from the standpoint of an "outsider." PCT has so much to offer--in presenting it to others, one has to be sensitive to their point of view. "Selling" new ideas isn't easy, but is a challenge. There certainly need be no "compromises." One must, however, take into account where readers are coming from. This is one reason why one major section should consist of directly addressing objections that one finds in the literature. Also, some account of the evolution of PCT early on is important. As regards the latter, "systems" is no longer an out construction. Indeed systems are in, and a presentation should address the relationship between PCT and systems in a positive way.

Tom, could you pass on the previous objections to me?

I imagine you all are aware of the many hoops Harnad poses. I recently served as one of the reviewers of an initial submission that was presented last July, I believe. The author had to address a long list of objections from nine reviewers and the ms. is only now in its second stage, having been sent back to the reviewers. This is common for papers that eventually get published.

Date: Tue May 25, 1993 5:31 am PST
Subject: Another Devils' Bib. entry

From Greg Williams (930525)

Quoted from Edwin A. Locke (University of Maryland) and Gary P. Latham (University of Washington), A THEORY OF GOAL SETTING & TASK PERFORMANCE, Prentice-Hall, Englewood Cliffs, New Jersey, 1990, pp. 19-23. (Copyright 1990 by Prentice-Hall, Inc.)

According to the book's index, there are no other comments on PCT besides these.

"As the influence of behaviorism has declined, a neo-behaviorist theory is emerging to take its place. It is called control theory and can be viewed as a combination or integration of behaviorism, machine-computer theory (cybernetics), goal setting theory [championed by Locke and Latham], and, by implication, drive-reduction theory. It is derived directly from Miller,

Galanter, and Pribram's TOTE model (1960). The major concepts of control theory have been presented by Campion and Lord (1982), Carver and Scheier (1982), Hyland (1988), Lord and Hanges (1987), Powers (1973), and others. In brief, the theory asserts that there is INPUT (a stimulus), which is detected by a SENSOR. If there is a deviation (also called a 'disturbance'), a SIGNAL is sent to an EFFECTOR, which generates modified OUTPUT (a response). This output becomes input for the next cycle. In goal theory language, the input is feedback from previous performance, the reference signal is the goal, the comparator is the individual's conscious judgment, and the effector or response is his or her subsequent action which works to reduce the discrepancy between goal and performance.

"While control theory acknowledges the importance of goal setting, there are serious, if not irredeemable, flaws in the model. First, observe that the major 'motive' for action under control theory is to remove disturbances or discrepancies between the goal and the input (feedback). The natural state of the organism is seen to be one of motionlessness or rest. This is true of machines, but not of living organisms which are naturally active. It is, in fact, a mechanistic version of the long-discredited drive-reduction theory (Cofer & Appley, 1967). Nuttin (1984 [J. Nuttin, MOTIVATION, PLANNING AND ACTION, Erlbaum, Hillsdale, New Jersey]) has observed that in this aspect, control theory fundamentally misstates the actual source of motivation: 'The behavioral process... does not begin with a "test" of the discrepancy between the standard and the actual states of affairs. Instead, it begins with a preliminary and fundamental operation, namely the construction of the standard itself, which, as a goal, is at the origin of the action and directs its further course' (p. 145). Similarly, Bandura (in press [A. Bandura, "Reflections on Nonability Determinants of Competence," in J. Kolligan & R. Sternberg, eds., COMPETENCE CONSIDERED: PERCEPTIONS OF COMPETENCE AND INCOMPETENCE ACROSS THE LIFESPAN, Yale University Press, New Haven]) noted that GOAL SETTING IS FIRST AND FOREMOST A DISCREPANCY CREATING PROCESS. Control theory begins in the middle rather than at the beginning of the motivational sequence. To quote Bandura (in press):

Human self-motivation relies on both DISCREPANCY PRODUCTION and DISCREPANCY REDUCTION. It requires FEEDFORWARD control as well as FEEDBACK control. People initially motivate themselves through feedforward control by setting themselves valued challenging standards that create a state of disequilibrium and then mobilizing their effort on the basis of anticipatory estimation of what it would take to reach them. After people attain the standard they have been pursuing, they generally set a higher standard for themselves. The adoption of further challenges creates new motivating discrepancies to be mastered. Similarly, surpassing a standard is more likely to raise aspiration than to lower subsequent performance to conform to the surpassed standard. Self motivation thus involves a dual cyclic process of disequilibrating discrepancy production followed by equilibrating discrepancy reduction. (p. 23 of preprint)

"Figure 1-3 [not reproduced here] shows how little of the motivational process control theory, in its 'core' version, incorporates.

"The above is important because if discrepancy reduction is the major motive, as implied by control theory, then the most logical thing for an individual to do would simply be to adapt his or her goal to the input. This would guarantee that there would be no disturbance or discrepancy. Machines, of course, cannot do this because the standard has been fixed by people at a certain level (as in setting a thermostat). But people can and do change standards that diverge from present performance. If the individual's major motive were to remove disturbances, people would never do this. Control theorists argue that lower-level goals are actually caused by goals at a higher level in the individual's goal hierarchy (Carver & Scheier, 1982). But this only pushes the problem back a step. Why should people set higher-level goals if they only want to reduce tension? But in reality, people do set goals and then act to attain them; they do not focus primarily on eliminating disturbances. Removal of discrepancies and any associated tension is a CORRELATE of goal-directed action, not its cause. The causal sequence begins with setting the goal, not with removing deviations from it.

"At a fundamental level, discrepancy reduction theories such as control theory are inadequate because if people consistently acted in accordance with them by trying to eliminate all disturbances, they would all commit suicide -- because it would be the only way to totally eliminate tension. If people chose instead to stay alive but set no goals, they would soon die anyway. By the time they were forced into action by desperate, unremitting hunger pangs, it would be too late to grow and process the food they would need to survive.

"In their major work, Carver and Scheier (1981) denied that discrepancy reduction is motivated by a desire to reduce a drive or state of tension. But their own explanation as to why people act to reduce discrepancies is quite puzzling. 'The shift [of action in the direction of the goal or standard] is a natural consequence of the engagement of a discrepancy-reducing feedback loop' (p. 145). This statement, of course, explains nothing. Why is discrepancy reduction a 'natural consequence'? According to goal theory, BOTH discrepancy creation AND discrepancy reduction occur for the same reason: because people need and desire to attain goals. Such actions are required for their survival, happiness, and well-being.

"A second problem with control theory is its very use of a machine as a metaphor. The problem with such a metaphor is that it cannot be taken too literally or it becomes highly misleading (e.g., see Saundelands, Glynn, & Larson, 1988 [L.E. Sandelands, M.A. Glynn, & J.R. Larson, "Task Performance and the 'Control' of Feedback," Columbia University, unpublished manuscript]). For example, people do not operate within the deterministic, closed-loop system that control theory suggests. In response to negative feedback, for example, people can try harder or less hard. They can focus on the cause and perhaps change their strategy. They can also lower the goal to match their performance; in some cases they may raise their goal. Furthermore, they can reinterpret the discrepancy as unimportant and ignore it or can even totally deny it. They can also question the accuracy of the feedback. They can go outside the system (by leaving the situation). They can attack the person they hold responsible for the discrepancy. They can become paralyzed by self-doubt and fear and do nothing. They can drink liquor to blot out the pain. In short, they can do any number of things other than respond in machinelike fashion. Furthermore, people can feel varying degrees of satisfaction and dissatisfaction, develop varying degrees of commitment to goals, and assess their confidence in being able to reach them (Bandura, 1986). These emotions, decisions, and estimates affect what new goals they will set and how they will respond to feedback indicative of deviations from the goal (Bandura, 1988). Control theory, insofar as it stresses a mechanistic model, simply has no place for these alternatives, which basically means that it has no place for consciousness. Insofar as this is the case, the theory must fail for the same reason behaviorism failed. Without studying and measuring psychological processes, one cannot explain human action.

"One might ask why control theory could not be expanded so as to accommodate the ideas and processes noted above. Attempts have been made to do this, but when it is done, the machine language may still be retained. Hyland (1988), for example, described the effects of goal importance or commitment in terms of 'error sensitivity,' which is represented diagrammatically by a box called an 'amplifier.' Expectations and memory are represented as 'symbolic control loops.' Decision making is done not by a person but by a 'selector.' What is the benefit of translating relatively clear and well-accepted concepts that apply to human beings into computer language that is virtually incomprehensible when used to describe human cognition? The greater the number of concepts referring to states or actions of consciousness that are relabeled in terms of machine language, the more implausible and incomprehensible the whole enterprise becomes. Nuttin (1984, p. 148) wrote on this: 'When behavioral phenomena are translated into cybernetic and computer language, their motivational aspect is lost in the process. This occurs because motivation is foreign to all machines.'

"On the other hand, if additional concepts are brought into control theory and not all relabeled in machine language (e.g., Lord & Hanges, 1987), then control theory loses its distinctive character as a machine metaphor and becomes superfluous -- that is, a conglomeration of ideas borrowed from OTHER theories. And if control theory does not make the needed changes and expansions, it is inadequate to account for human action. Control theory,

therefore, seems to be caught in a triple bind from which there is no escape. If it stays strictly mechanistic, it does not work. If it uses mechanistic language to relabel concepts referring to consciousness, it is incomprehensible. And if it uses nonmechanistic concepts, it is unoriginal. It has been argued that control theory is useful because it provides a general model into which numerous other theories can be integrated (Hyland, 1988). However, a general model that is inadequate in itself cannot successfully provide an account of the phenomena of other theories.

"In their book, Carver and Scheier (1981) examined the effect of individual differences in degree of internal focus versus external focus in action. While this presentation is more plausible than the mechanistic versions of control theory, most of it actually has little to do with control theory as it relates to goal setting. For example, they discuss how expectancies and self-focus affect performance but do not examine the goal-expectancy literature (as we do in Chapter 3). And some of their conclusions (such as that self-efficacy does not affect performance directly) contradict actual research findings. Only one actual goal setting study (not in Carver and Scheier's book) has used the self-focus measure. Hollenbeck and Williams (1987) found that self-focus only affected performance as part of a triple interaction in which ability was not controlled. Thus it remains to be seen how useful the measure is, either as a moderator or as a mediator of goal setting effectiveness.

"There is also a conceptual problem with the prediction that the relation between goals and performance will be higher among those high in self-focus than those low in self-focus. Goal attainment requires, over and above any internal focus, an EXTERNAL focus; most goals refer to something one wants to achieve in the external world. Thus the individual must monitor external feedback that shows progress in relation to the goal in order to make progress toward it. Individuals might focus internally as well (a) to remind ourselves of what the goal is -- though this can also be done externally, as on a feedback chart; (b) to retain commitment by reminding themselves of why the goal is important; and (c) to assess self-efficacy. Furthermore, depending on what is focused on, (e.g., self-encouraging thoughts or self-doubt), an internal focus could either raise or lower goal-relevant effort. In sum, the relation between where one is focused and goal-relevant performance seems intuitively far more complex than is recognized by the cognitive version of control theory.

"Finally, some have argued that control theory is original because it deals with the issue of goal change (e.g., Champion & Lord, 1982). However, goal change was actually studied first by level-of-aspiration researchers in the 1930s and 1940s, so control theory can make no claim of originality here. Nor can a mechanistic model hope to deal adequately with issues involving human choice as noted above.

"In sum, the present authors do not see what control theory has added to our understanding of the process of goal setting; all it has done is to restate a very limited aspect of goal theory in another language, just as was done by behavior mod advocates. Worse, control theory, in its purest form, actually obscures understanding by ignoring or inappropriately relabeling crucial psychological processes that are involved in goal-directed action (these will be discussed in subsequent chapters)."
