CSG 9212

Date: Tue Dec 01, 1992 12:16 am PST
Subject: Re: Controlling error; Beta; planning

[From John Gabriel 921130.2304CST]

>(Bill Powers (921130.1300))-- >>John Gabriel (921130.1201) --

>>The overall transfer function (net gain, not OP AMP gain) is
>>
>> G = A/(1 - A*beta)

>The transfer function that holds between the reference signal and the
>perceptual signal is

>> G = A*beta/(1 - A*beta),

>isn't it? And if you use the standard diagram I use, in which the
>perceptual signal is subtracted from the reference signal and all other
>constants are positive, the sign in the denominator has to be positive,
>doesn't it? Otherwise, doesn't there need to be a negative sign in the
>numerator for a negative feedback system?

Yes, I'm used to thinking about poles and zeros of G, and having the poles roots of A*beta=1 is convenient, but not necessary. I tend not to bother too much with the sign of G unless I'm actually trying to deal with hardware, when it is all important.

>You're correct in saying that I always use a beta of 1. The reason has
>..... Bill's helpful discussion of normalisation and NSUs

Yes, but the normalisation is only really safe if beta is a constant, not an integro differential linear operator, or worse yet a non linear one. Otherwise there seems to be a messy change of scale/independent variable, or what have you. But I haven't worked it all out. I just keep A and beta (sort of) distinct. In spite of my (correct) claim the the boundary between them is a matter of choice. It all really depends on where you want to break the loop to measure end around loop gain.

>An alternative is to define 1 NSU as the maximum possible perceptual
>signal. I did this the other day in talking about setting the reference
>signal to 75% of maximum, and so on. This is a useful ploy when
>evaluating the effects of disturbances relative to the whole control
>range; it helps one to see how important a particular amount of error
>is.

Yes, we are just on slightly different playing fields and need to do a bit of reorganisation to agree on where it's easiest to hit a home run. But I don't feel very worried about that amount of error signal between our percepts. Each is probably best matched to its owner's decision tree, habits of a lifetime, and problems.

>>... if you are concerned with designing autopilots, as Martin is,
>>then you probably want to focus on the aircraft dynamics.

>This is a somewhat confused subject, in part because there are control

....
>Aircraft dynamics enter the control problem for stability reasons only.

Tom Baines is our fixed and rotary wing expert. He's out of town just now, but he might have some comments when he gets back. I only fly simulators.

>My approach, demonstrated in the Little Man program, is to start by
>putting the highest derivative under direct control. That system is then
>controlled to produce the demanded position. In an aircraft attitude
>control system you need another derivative. But none of this entails the
>control system itself doing any calculations of aircraft dynamics. Or to
>put it another way, the control system IS the calculation of aircraft
>dynamics.
....

Yes, I agree, and I had not perceived your neat analysis of postural dynamics. NIIIIICE - I LIKE IT!!!

And I realise it's what my friends at CS Draper lab do with three very good accelerometers, and three laser ring gyros. They integrate the measured accelerations twice to do inertial navigation. But there are non trivial difficulties, you have to recalibrate the system periodically against astronomical observations because of accumulated error in the intergrations. And I suppose that's why my postural reflexes feel funny when I stand on the edge of a cliff. I've lost my (real not artificial) horizon. Perhaps I might say that the control system is OBSERVING vehicle dynamics, as one essentially does when tracking satellite orbits to measuer gravitationl anomalies. And GPSS and Motorola's Iridium proposal is inverse satellite dynamics. I'm not really comfortable with fine distinctions between dynamics and inverse dynamics, or with distinctions between actual and virtual observables. But I agree it's ESSENTIAL to know what you are observing directly, and what you are controlling directly. This isn't easy. I think I quoted Bill C. before - THERE IS NO GROUND TRUTH. And I have not got some of this thru my fuddled brain yet. But there is a distinction between models that correlate with observation and those that don't, and the process of correcting the ones that don't is some sort of PCT.

I think the split into A and beta, and probably a split into a composite of several A's and several betas might turn out to be important. Perhaps beta is what you can directly measure, and A must be inferred from responses to disturbance - A is inside somebody's skull, and beta is outside. For the multiple feedbackpaths I've always found Bode "FeedBack Amplifiers and Network Design" McGraw Hill (either 1939 or 1945 I don't remember) illuminating and the 12 ft. shelf from MIT Rad. Lab is still a delight even though only a few things such as Valley and Wallman are really still relevant. But Bode is as fresh as the day the first copy came off the press. Real testimony to lasting scholarship. I was pleased to see that my copy of Bill's book is a fourth printing. Somebody out there is reading them guy!!

Well, enough random rambling. Bedtime - it's after midnight by now.

>>Some of this can be seen in a simple model of docking the liner.
>>To a first approximation, the Laplace Transform of beta is 1+T*p
>>where T is a characteritic time needed to change anything the liner
>>may be doing. As T -> infinity, G -> 1/T*p, our old friend the
>>Blumlein/Miller Integrator.

>I don't remember that integrator. Could you explain it?

I went off line with that explanation. It's not really very different from Bill's book where integration by feedback systems is discussed. Bill or I can post to net if anybody wants. It's also what has been known to radio amateurs as the Miller Effect, and a sort of sentimental memorial to Blumlein who was a very bright guy killed in the crash of a flying testbed for H2S in 1942.

>I would start by sensing engine thrust and putting that under control.
>Then I would add a second level sensing velocity, and put that under
>control by using the thrust control system as the output function.
>Finally I would sense position, and control that by using the velocity
>control system as the output function. If the parameters are correctly
>set (and the settings would not be critical), specifying a reference
>position will automatically move the ship to the required position, with
>velocity and acceleration becoming zero just as the reference position
>is reached. The acceleration will automatically reverse as the goal is
>approached. None of these effects has to be calculated in detail,
>coordinated, or otherwise be explicitly handled.

Yes, I should have put the remarks about the accelerometers and ring gyros here. But the only things the ship's pilot or the captain directly control are what the helmsman and the engineers do in response to orders. You can reasonably extend that to control of thrust and rudder deflection if you want to factor out the linguistic communication as reliable. But after that, the pilot is really deciding a trajectory that will end at the dock, with the ship properly oriented, without kinetic energy, and with zero thrust. Yes, from Bill's perspective it's still a control problem, but I suggest it's one with integral equations, not derivatives, because people are not very good sensors for acceleration. This is getting a bit muddy perhaps. And from my point of view, a good deal of past experience comes into play in solving those equations (which do involve vehicle dynamics if you can't sense accelerations -- acceleration really only became directly sensed for most people with the arrival on the scene of the Chrysler hemi). If past experience was not important any of us could dock an ocean liner as easily as Mark Twain navigated the Mississippi River.

I'm copying Joe Di Scenza on this - he's the "fourth man" in the conspiracy to turn data fusion on its head, but he has also commanded substantial water craft.

>I guess I had better illustrate this with a model you can try. Wolfgang
>Zocher has worked out a rather neat program that will accept simple
>ASCII input specifications for any control-system setup, and run the
>specified model, putting whatever variables you want to monitor into an
>ASCII file as numbers. This program will probably be greatly elaborated,
>but right now it can make passing simple models back and forth very
>practical. Wolfgang, are you reading this? It would be a great service
>if you were to put your program, in C, onto Bill Silvert's file server.
>I will follow it up with the modified version needed for Turbo C, and a
>compiled version that will run on PCs. Maybe someone else could compile
>a version for Macs. If everyone who's interested in modeling had a
>runnable copy of this program, then all we would have to send back and
>forth are the ASCII specification files. To run a model, you just type

>SIMCON SPECFILE

>and it runs the setup contained in the ASCII file SPECFILE.INP and puts
>the output data into SPECFILE.DAT, another ASCII file. Later we'll work

>out reading programs that can convert the ASCII files to graphical
>plots, but even as the program stands it is highly usable.

>Let's have some comments from everyone out there -- are you interested
>in getting a copy of the program? Can you compile it from C source code
>(in which case it will run on just about anything)? Talking about models
>without running them is like explaining an aircraft maneuver with your
>hands in your pockets.

GREAT. LET's TRY IT.
>Martin Taylor (921130.1340) --\

>Some very well-made points about planning. If you can plan, you don't
>need to control. I would add that at any level, there are uncontrolled
>perceptions that can introduce disturbances, so that even if all
>controlled perceptions at a given level are perfectly controlled, the
>higher levels still work with partially-uncontrolled inputs.

>>I'm not suggesting that the error signal is fed upward in place of
>>the perceptual signal; ...

>That, however, is one viable form of the model, probably applicable mainly
>to higher levels. Maybe if Wolfgang gets his program into wide use here, we
>can pass some models around and explore this alternativemodel-based form of
>the control model. It has some interesting possibilities, including the
>ability to continue controlling with intermittent input data. Such a system
>is in the imagination mode ALL THE TIME, but there is provision for
>continually comparing the model's behavior with real perceptions and using
>the difference to modify the model. This requires error signals from lower
>level systems to be passed upward, not perceptual signals.

Regards to all, and thanks for all the insights. I really feel I'm airborne at last on
some of Bill's stuff, even though I'm still not giving it the attention I ought to.
Apologies for the neglect, gotta spend SOME time feeding Uncle.

Oh well, Per Ardua ad Astra. Or better yet, Sic Itur ad Astra.

John (gabriel@caesar.eid.anl.gov)

Date: Tue Dec 01, 1992 5:33 am PST
Subject: metaprogram

[From: Bruce Nevin (Tue 92111 08:23:41)]

Bill Powers (921130.1300) --
/**********
* Wolfgang *
* Zocher has worked out a rather neat program that will accept simple *
* ASCII input specifications for any control-system setup, and run the *
* specified model, putting whatever variables you want to monitor into an *
* ASCII file as numbers. This program will probably be greatly elaborated, *
* but right now it can make passing simple models back and forth very *
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* a version for Macs. If everyone who's interested in modeling had a         *
* runnable copy of this program, then all we would have to send back and     *
* forth are the ASCII specification files.                                   *
*                                                                            *
* Let's have some comments from everyone out there -- are you interested     *
* in getting a copy of the program? Can you compile it from C source code    *
******************************************************************************/
```

I am very interested. I think I have an old Turbo Pascal package kicking around somewhere, and I should be able to get a PD C compiler off the simtel-20 program archive. And of course there is a C compiler on my mail server.

Bruce bn@bbn.com

Date: Tue Dec 01, 1992 7:23 am PST
Subject: Re A, Beta, and the VALDEZ

[From Gabriel 921201.0850CST]

Very briefly, 'cause I gotta rush.

Bill - I should probably have written G=-A/(1-A*Beta). Then we agree on all except sign of A. Re normalisation to Beta=1.

$$G=(1/Beta)(-A*Beta)/(1-A*Beta)$$

And in the limit G ~ 1/Beta, just as it always does. BUT if we stick with my convention that A is in the wetware (not entirely in the skull I should have been more careful), when we normalize, writing A'=A*Beta, we have mixed a linear operator outside the wetware i.e. system dynamics, into the APPARENT wetware. And, that's the simple statement of all my tours around Robin Hood's barn about the ship's Cdr. doing integration along trajectories. YES, that's perhaps EXACTLY what he does to get the very convenient A/A+1 formalism and the kind of tracking that Bill so rightly observes is at the heart of things.

Sincerely John G (gabriel@caesar.eid.anl.gov)

Date: Tue Dec 01, 1992 7:54 am PST
Subject: More Re VALDEZ etc.

[From Gabriel to Net 920101.0922CST]

I could not resist. If Beta is outside the wetware, I think G=1/Beta is exactly the result you need to "invert" the system dynamics, and by gosh G is exactly the inverser system dynamics, as it need to be to obtain precise tracking. AND the A/A+1 formalism shows, just as Bill says, that the wetware has no (apparent) need to know about system dynamics.

Moreover, and this gets a bit far out, stability and tracking are two different shadows on the end wall of Plato's cave for the same thing, namely the poles and zeros of G in the complex plane. So, you can't separate them, no matter how much you would like. As the

system approaches instability when you move the poles around, you see more overshoot and longer settling times after a transient until a pole sits right on p=jw and an infinitesimal input, e.g. Brownian noise, causes perpetual signal at frequency w. So, you built yourself an oscillator without intending to do so.

John (gabriel@caesar.eid.anl.gov)

Date: Tue Dec 01, 1992 9:27 am PST
Subject: Adekola

[From: Bruce Nevin (Tue 92111 10:54:35)]

A while back I saw a brief piece (_New Scientist_ 31 October 1992, p. 19) with PCT potential:

> Ade Adekola is interested in intelligent buildings that can sense people and their surroundings and interact with them. To demonstrate his theories he has built, with a grant from Sheppard Robson Architects, a moving structure that changes shape in response to interactions with people. It is called a self-inducing device, or SID.
>
> SID is a flexible mast that resembles a spine. It is held upright with 12 cables connected to 12 tensioning arms arranged in a ring around it. On each arm is an electrically controlled solenoid which can draw in or let out its cable. A tilt sensor on each arm also provides information on the structure's position. A collection of 12 buttons are linked, via a computer, to the solenoids.
>
> <Some overblown fluff about having characteristics of a living system: sensory input through the buttons that people can push, action output through solenoids, and self awareness through tilt sensors.>
>
> But the raison d'e^tre of SID is to interact with people, so if left alone the computer directs it to move in a seductive manner, what Adekola calls its "luring" mode. If people continue to ignore it, SID regresses in behaviour through a series of modes: distress, sulking, arrogant and obsessive. In its final obsessive stage it ignores any interaction it is offered.
>
> However, if someone does press a button before it becomes obsessive, SID begins to backtrack through its modes, past "luring", until it reaches "playful", when it is directly controlled by the person. But once in playful mode, it is dumbly reacting to the person's button-pushing so Adekola expects people to become bored, walk away, and SID to regress again.
>
> Adekola is now working on a flat membrane that reacts to touch and other stimulae and has its sensors and actuators embedded in the structure.

It occurs to me that an architect is less likely than an academic psychologist to resist PCT notions because of prior commitment to linear causation (S-R, etc.) explanations. Historically, the arts have been an excellent avenue for exploration and even promulgation of new theories.

Adekola's present SID seems to be interactionist without being a control system.

Bruce bn@bbn.com

Date: Tue Dec 01, 1992 12:43 pm PST
Subject: Modeling stuff

[From Bill Powers (921201.0830 MST)]

John Gabriel (921130.2304 CST) --

I keep forgetting to tell you guys at Argonne National Labs: Bob Clark, my collaborator for the first 7 years of developing what we now call PCT, and who listens in on this net from Cincinnati, was a physicist at ANL before we both went to the VA Research Hospital in Chicago in about 1953 to start a new medical physics department.

Thanks for elaborating on the Miller Effect, which I finally recognized. I expect that this arrangement was the precursor of the operational amplifier. Vacuum tubes! It brings a tear to the eye. My first analogue computer was a Philbrick that used 12AX7s in the op amps. For you transistor freaks who casually point to components in running circuits by putting a finger on them, the computing voltages had a plus and minus 100 volt range and the power supply was 300 volts.

I agree that integrators are more common than differentiators in the nervous system. But there are exceptions: the rate feedback from muscle spindles, and the transition level of perception where we perceive things like the rotating second-hand on an analogue clock. I know of one place (a spinal motor neuron) where there is negative feedback from output back to input through an interneuron that has almost an integral response -- thus turning the spinal comparator into a proportional-plus- derivative element.

The eye provides rate perception through hypersensitivity to moving objects; this applies to lateral velocities and to the "streaming" of the images toward or away from the line of approach or recession. At higher levels, rates can be detected as the difference in output between two leaky integrators.

I agree that integral equations are the likely choice. The Little Man model, aside from the basic rate detectors, uses only integrators. True differentiation in the nervous system would produce a noisy mess.

>Yes, but the normalisation is only really safe if beta is a
>constant, not an integro differential linear operator, or worse yet
>a non linear one. Otherwise there seems to be a messy change of
>scale/independent variable, or what have you. But I haven't worked
>it all out. I just keep A and beta (sort of) distinct.

In the standard PCT diagram Beta IS distinct from A: it is the perceptual input function. It can be extremely complex because it has to report the presence of apparent external phenomena like shape and orientation, and lots worse things to compute. But in this

model, the output of such an input function is always a scalar variable (for better or for worse) with different perceptions handled by different control systems. This means that when the reference signal is set to a constant value, the perceptual signal (the output of beta) is also constant in the steady state.

The function beta might have some complex dynamics in it, for purposes of stabilization. But these dynamics do not appear in subjectiveperception. If the dynamics are perceived, they are perceived by a higher-level system concerned with dynamics, and the state of the dynamic variable is again a scalar that has a steady value when it matches its reference signal. Unperceived dynamics are part of the stabilization of a control system.

Just considering the scalar perceptual signal, there is only one zero (at infinity) and no pole in any perceptual variable I know about. This is deduced not from the model but from direct experience. All perceptual variables range over a scale of values, and when they change too fast the dynamic perceptual response falls off toward zero; we perceive the average value, or nothing.

In fact, I think that the basic simplicity of perceptual signals, coupled with the hierarchical nature of the whole system, makes stabilization relatively easy. No doubt if you took some standard servo design and re-partitioned it as a hierarchy, you would probably find the same overall transfer function. I wouldn't know -- I never got deeply into standard servo analysis because I couldn't visualize how the damned thing worked once it was cast as poles and zeros and Bode plots and Laplace transforms. This is one fact about me that makes me sure that some real control engineer will eventually catch on to the HPCT princple and carry this work a lot farther than I can.

> In spite of my (correct) claim the the boundary between them is a
>matter of choice.

When we're modeling human beings, the boundary really isn't a matter of choice. There's a natural interface surface at the sensory receptors and the motor actuators (which, up to the contractile element of the muscles, can be considered part of the nervous system). Unlike special- purpose artificial control systems, natural ones move around in the environment and are constantly presented with new external feedback functions and disturbances. What's inside the boundary is relatively constant, but what's outside is infinitely variable.

This is one reason, I think, for the hierarchical arrangement. At the lowest level, all environments are alike, in that only proximal stimuli are affected and perceived by the control systems. Once those proximal variables are under control, their behavior becomes completely determined regardless of the environment (within limits). That provides a stable base for the next layer, which again puts the next level of variable under control and makes the environment for higher systems more regular. An unpredictable and chaotic environment is turned into a regular one by the control systems at each level. Each level sees to it that an output by a higher system will always have the same perceived effect on the lower-level world. This, in fact, makes higher-level control possible, as Martin has been pointing out.

I see these levels as the product of evolution. In the simplest organisms there is only one level of control of external variables, control of chemical concentrations at the cell membrane. As evolution progressed, more layers of control could be added and more abstract aspects of the perceived environment could be made predictable (although prediction is not a function of lower-level systems -- what matters isthat setting a reference signal reliably create a corresponding perceptual signal). This is how I evolve

control-system models: get the lowest level of variable under control, then the next, and so on.

>Yes, from Bill's perspective it's still a control problem, but I
>suggest it's one with integral equations, not derivatives, because
>people are not very good sensors for acceleration.

In the human arm, acceleration is measured in a very simple way: by tendon receptors, which sense applied force. Of course this means that actual acceleration varies with the load. Velocity is sensed by rate effects in the stretch receptors, as mentioned.

The hierarchical control relationships on a ship are interesting. The captain in the conning tower says to the helm, "Half ahead." The helmsman moves the handle on the annunciator back and forth to ring a bell down in the engine room and sets it to "Half ahead." In a little while, the pointer on the annunciator comes to the same position, as the motor mac below, after setting the engine speed to half ahead, acknowledges. Seeing the pointer match the setting of the handle (and hearing a ding), the helmsman replies to the captain, "Half ahead, sir." Everybody's controlling perceptions -- even the motor mac, to whom "half ahead" means an indication on an RPM gauge. Lots of lags in the system, but with a big ship they don't matter so much.

Basic principle of designing organisms: never trust that an output did what it was supposed to do.

Wolfgang Zocher (921201) --

Looks as though there is plenty of interest in your program. I don't think you need a lot of error-handling in an early version of it; we are all on the net and can help each other debug when neccessary. I'd like to see something out there soon so we can get on with the Primer series. I'll admit that the error handling do you have in it has proven quite useful!

In setting up some fairly complex (three-level) systems, I've noticed that the order in which you do the block computations makes a difference. If you compute forward around a loop, there is only a one- iteration lag from input to output. But if you go backward, from output back to input, each computing block puts in a one-iteration lag. This makes loops within loops rather ambiguous as to their exact timing.

The solution is to make all blocks compute in parallel. This means that each block should compute a new output on the basis of its inputs from the OLD outputs of all other contributing blocks (including its own). Then after all computations are finished, all of the old outputs would be replaced by the new outputs computed on this iteration. This means giving each block two output values, an old value and a new value. After each pass through the program, a cleanup routine pops all the new values into the old value positions.

This gets rid of the last disadvantage of doing analog computations on a digital machine, other than the finite iterations. In real neuralsystems, all computing blocks are operating all the time, not in sequence. Also, there is a realistic relationship between total transport lag and number of computing blocks around any given loop. LIkewise of importance for revising programs, you can write the program steps in any order without affecting the timing. This makes patches and changes easy.

This would mean that you don't really need a separate "lag" function. All you need is an elementary routine that makes the new output equal to the input. To get n iterations of lag, you just put n of these units in series: 20 lag 19; 21 lag 20; 22 lag 21 ...

I'll work on this if you like, although since you're also modifying the program it would probably be best if you handled all changes.

When the version is frozen, I will translate it into Turbo Pascal unless you want that chore, too. So don't get too wordy. Maybe Rick would volunteer to translate it into BASIC. Any other volunteers for other languages? Who's on Fortran?

Best to all, Bill P.

Date: Tue Dec 01, 1992 1:38 pm PST
Subject: Re: Adekola

>RE: Bruce Nevin (Tue 92111 10:54:35)]
>

> Adekola is now working on a flat membrane that reacts to
> touch and other stimulae and has its sensors and
> actuators embedded in the structure.

>It occurs to me that an architect is less likely than an academic
>psychologist to resist PCT notions because of prior commitment to
>linear causation (S-R, etc.) explanations. Historically, the
>arts have been an excellent avenue for exploration and even
>promulgation of new theories.

Thanks for the post on Adekola, a copy of which I will send to the only architect with whom I have discussed PCT notions. He was fascinated with the dynamic process those notions describe and explain. I suspect part of this is for the reason you suggest (see above); another reason may be that this architect is a "student" of the Frank Lloyd Wright school of architecture which requires that architects spend a good deal of time observing and talking with clients about their interests and activities before proceeding to design a structure that will provide more opportunities for than disturbances to the pursuit of those interests. Whether one is designing a social structure, or a plan by which group members can fit different actions together to realize group interests that none can realize alone, or a physical structure to aid and abet those actions on the one hand and minimize outsiders' disturbances to those interests and actions on the other hand, PCT notions are very helpful.

Date: Tue Dec 01, 1992 1:41 pm PST
Subject: RE: Adekola

From tom bourbon [921201 13:01 CST]

Re: bruce nevin [921201] on Adekola

Nice post -- and your observation about architects and people in the arts, in general, squares with my experience. They have no theoretical commitment and no agenda. Consequently, many of them are open to PCT. Adekola's SID reminds me of some very

ingenious and creative work by a couple of young performance artists in Bordeaux, France, hwo are preparing a show in which a few small mobile robots, some bearing TV cameras with images shown on the wall, will interact with members of the mobile audience. Some robots will be programmed like E. coli, others, like characters from the "CROWD" program. The artists were fascinated and excited by their first exposure to the PCT demonstrations and immediately started to work on a major production. Perhaps that is our niche!

Regards to all, Tom Bourbon

Date: Tue Dec 01, 1992 2:23 pm PST
Subject: Re: Modeling stuff

[From Gabriel 921201.1332CST]
I'm pretty sure I agree about the reality behind everything Bill P. says. But I see it from a slightly different perspective, so we find we disagree a bit about perceptions onto the "display window". Bill, being Navy (is there never Ex Navy, only Retired, just like USMC? - probably not) knows all the fine details about docking a ship, and yes, there are delays, and lots of intermediate circuits - not only the linguistic ones - and some SEND/ACK protocols perhaps a bit like Martin's - Yes, I got the signal, and yes, the engines are now at 1/2 speed, so the message was understood.

I don't think enough about the neurobiology. As we were reminded in the post about olfactory sense, the wetware can teach the hardware a thing or two. And I have a real feeling that this is always true. What Bill C. calls integration or association (in the psychological, not the Leibnitz sense).

And, in the end, unless you happen to think that way (as I tend to do) the Laplace Trnsformations are only so much technobabble. I usually find the "mathematical" analysis a good test against insight. If I can put it in mathematics, there's probably nothing wrong with my logic, but perhaps still errors in my efforts as a field naturalist. And at times, but only for those who are comfortable with the technobabble, a line or two of algebra can illuminate an argument that I for one, would otherwise miss.

Best to All, and many thanks to Bill P.

John (gabriel@caesar.eid.anl.gov)

Date: Tue Dec 01, 1992 3:45 pm PST
Subject: What's Wrong with Moral Relativism?

[Rick Marken (921201.1300)]

I invited a fellow named Tom Zentall to join csg-l. He wrote a friendly review of my "Blind men" paper to Harnad (I still don't know if that paper was accepted -- but I can guess) and I found out that he has done research on "hyperactive kids" -- his theory being that they are (in PCT terms) trying to control a perception called "stimulation" at an unusually high level and their "hyperactivity" is just the observable effort to control that perception. Anyway, I hope he joins; always nice to have "real" psychologists (or whatever) examining PCT.

Now. To the title story.

On the way home from work last week I was listening to a piece on the news and a very bright sounding person was being interviewed about something (forgot what) but at one point he felt it necessary to say some- thing like "this is not moral relativism or anything". The fellow was talking about some pretty liberal stuff (hey, it was Public Radio) so it stuck me that this "moral relativism" thing is something that is fairly equally despised by right and left -- ie. just my kind of stuff. So it got me to thinking about moral relativism in PCT terms.

The first thing I ask myself was "what is a moral"? It seems to me that morals (from a PCT perspective) are a kind of perceptual variable. The word probably refers to several different types of variable -- program (rule), principle and even system concept. But let's just consider "lower order" perceptual variables -- rules. One moral "rule" variable was mentioned recently; honesty. Different levels of honesty can be perceived in our relationships; and we can act to produce different levels of the honesty perception in our own relation- ships. Honesty is a controllable perceptual variable; a particular setting of this variable would probably be referred to as a "moral"; "Thou shalt not steal", "honesty is the best policy" are phrases that refer to states of a rule which can be seen in the behavior of people as a particular level of honesty. In this context, moral relativism simply refers to the fact that one CAN (not should) control the perception of honesty at different levels. "Relativism" also implies that the variability of this variable is relative to "other things" -- in PCT these "other things" are what make the reference for a perception vary, viz. 1)higher level references and 2) disturbances to the corresponding higher level con- trolled perception. The higher level controlled perception, in this case, might be a principle, like "justice" (also a variable - but let's assume a fixed reference for the perception of a certain level of the "justice" perception). If the higher level reference is fixed, then the only thing that can cause variation in the reference for "honesty" is a disturbance to the perception of the justice principle. A disturbance to the perception of justice might be learning that Mr. X made you broke by swindling you. If the opportunity presented itself to you, you might be willing to steal back the money from Mr. X -- temporarily controlling your honesty at a lower than usual level to keep up the perception of justice in the world (of course, you might not if other principles would be violated by doing that).

The point is that moral relativism simply recognizes a fact about human organization at higher levels of control; it's not a matter of whether people should or should not be this way; it is whether they ARE this way -- ie. are they control systems? When we start thinking about control of higher order variables like this (and, hopefully, studying it too) we will see how PCT takes us beyond morality (Thus Spake Zarathustra -- I bet ol' Nieztche would have liked PCT) to functionality; from behaving by rule to behaving with grace (Zen behavior?) --ie. being in control.

Regards Rick

Date: Tue Dec 01, 1992 3:46 pm PST
Subject: Re: Controlling error; Beta; planning

[Martin Taylor 921201 16:00] (Bill Powers 921130.1300)

>Martin Taylor (921130.1340) --\
>
>Some very well-made points about planning. If you can plan, you don't
>need to control. I would add that at any level, there are uncontrolled
>perceptions that can introduce disturbances, so that even if all

>controlled perceptions at a given level are perfectly controlled, the
>higher levels still work with partially-uncontrolled inputs.

I'm afraid I don't follow. If ECS A provides as its output reference signals for ECSs X, Y, Z, ..., and they provide the desired percept (perfectly, by hypothesis), how can the percept of A be uncontrolled? The hypothetical extreme situation that I was proposing was that A would be assured that the percepts it demanded of X, Y, Z, ... would be delivered. Any disturbance applied by the world would be eliminated by the perfect control at the lower level, wouldn't it? If the sensory values applied to the PIF of X don't change, then the output shouldn't change. (I should think this would apply even when we are at time-based levels such as sequence or program, but my intuition gets a bit dicey under these conditions).

Or are you referring to the degrees-of-freedom problem, that says that not all perceptions are controllable simultaneously, but that any perceptual signal is potentially an input to any higher ECS? If so, I agree, but it raises an interesting point about imagination. I have thought of imagination as a "what if" kind of exercise, issuing virtual output signals that would bring the percetual signal colser to the reference if they were real. Now we are talking about a "what if I did nothing" exercise, imagining what the perceptual signal would be, and comparing it with what is to get the new information. The information rate is presumably somewhat higher for uncontrolled perceptual signals than for controlled, but it is still less than a naive description of the temporal pattern of incoming sensory data would require.

>It [passing the error up vice the perceptual signal] has some interesting
>possibilities, including the ability to continue controlling with
>intermittent input data. Such a system is in the imagination mode ALL
>THE TIME, but there is provision for continually comparing the model's
>behavior with real perceptions and using the difference to modify the
>model. This requires error signals from lower level systems to be passed
>upward, not perceptual signals.

I didn't want to move the input to the PIF from the lower-level perceptual signal to the lower-level error signal, because to do so seems to me inherently less accurate than using the perceptual signal directly. My point was that the imagination mode provides the subjective probability distribution in the ECS that determines the information being supplied by the perceptual signal, and if the lower levels are doing what they are asked to do, then they are providing only a very little information to the higher ECS. I would hazard a guess that in a well organized system in good control, the information rate is reduced by a factor of G/N at each level, where G is the average (geometric, I suppose) gain of the lower level system, and N is the number of lower-level systems contributing sensory input to the higher-level system. G/N then would be an appropriate inter-level slowing factor for simulations. There would have to be a modification of this for uncontrolled inputs.

Note that the low information rate can be achieved by connections that provide information at a high rate intermittently, just as readily as by systems that provide it continuously but slowly. There's nothing in this that argues one way or another for whether the sensory data comes from lower-level perceptual signals or error signals, but it does demand that the higher-level ECS be in imagination mode all the time.

Martin

Date: Tue Dec 01, 1992 5:04 pm PST

i.n.kurtzer 921201.1815

since the past few posts have centered on control system engineering, i figured i would tell a joke i was told over this past holiday by a control systems engineer :

an airplane was flying and everything seemed normal and then
it crashed without warning, why? there were too many poles on the right.

first, i hope a didn't offend anyone, it was not intentional (now that is funny) and secondly i hope someone found the joke funny or at least understood the punchline (i did not and don't know why its funny,except the two meanings of "pole").

Date: Tue Dec 01, 1992 8:31 pm PST
Subject: Joke

Yes, good joke. Don't worry about being "politically correct" with jokes among your friends like CSGNET. I'm from England, and I spent ten years in New Zealand. Australians and New Zealanders have lots of jokes about people from England, they begin with comments about the foreigners being pomegranates (pink, not sunburned) and go on to their being kippers (two faced and gutless). But nobody sensible is upset. I think having too many poles on the right destabilising the autopilot is very funny, and I have Polish friends. And anyhow, anybody who wants to be offensively funny at the expense of Poland should remember how much Europe and America owe to Marian Rejewski and his colleagues as well as to Frank Rowlett, William Friedman, and probably Adrian Albert too. So Poland has much to be proud of from many centuries, and the Poles I know think about their proud history, and laugh at the jokes with everybody else.
John Gabriel (gabriel@eid.anl.gov)

Date: Wed Dec 02, 1992 8:53 am PST
Subject: For the record

From: Tom Bourbon [921202 -- 9:40 CST]

For the record, last week Bill Powers and I learned that our manuscript, "Models and their worlds," was rejected for a third time. After working on the paper for over six years, we agree that we will not resubmit to a conventional journal. Instead, we will release the paper as a technical report, if anyone is foolish enough to want a copy. (According to our editors and all but one reviewer during the past couple of years, the paper has few-to-no redeeming qualities.)

We have not yet settled on the procedure for distributing this forsaken manuscript. When we do, we will post an announcement to CSG-L and will include a note in the CSG Newsletter.

Best wishes, Tom Bourbon

Date: Wed Dec 02, 1992 9:32 am PST
Subject: Deja vu all over again

Bill Cunningham (921202.1100)

Hate to admit this, but I first heard the Poles in right half plane joke a little over 30 years ago in much the same circumstances. The accompanying comment was that the Poles in the left half plane were all designing roller coasters. Exciting or unexciting, depending on how you deal with imagination and reality.

My apologies to the control engineers amongst us. The rest don't want to know, lest it Bode ill for them.

Bill P and John Gabriel:

For perhaps 12-14 years, I've considered organizations in terms of feedback control systems, starting with the observation that volunteer organizations behave like Type 0 systems with low loop gain. There has to be enough error signal to motivate the volunteers into action. A well run volunteer organization is typically a first order system with moderate gain, but only if the leader is the integrator. If the integrator is one of the workers and the leader is not, the worker gets disgusted and leaves. If the gain is too high, too many workers unvolunteer. PCT explains why. Attempting to make a volunteer organization into a second order system doesn't work. A professional organization, however, relies on first order thinkers at the working level and will fail if the organization is not second order or higher.

An imprecise metaphor, perhaps. But part of the background that led to fascination with PCT.

Bill C.

Date: Wed Dec 02, 1992 1:32 pm PST
Subject: *INTRO TO CSGnet*

INTRODUCTION TO THE CONTROL SYSTEMS GROUP NETWORK (CSGnet)
AND TO THE CONTROL SYSTEMS GROUP
Prepared by Dag Forssell 921118.

This introduction provides information about:

Our subject: Perceptual Control Theory
The evolution of the control paradigm
Demonstrating the Phenomenon of Control
The purpose of CSGnet
CSGnet participants
Asking questions
The Control Systems Group
Subscribing to CSGnet
How to obtain text and program files
Literature references

OUR SUBJECT: PERCEPTUAL CONTROL THEORY

Here are two introductions by Bill and Mary Powers:

There have been two paradigms in the behavioral sciences since the 1600's. One was the idea that events impinging on

organisms make them behave as they do. The other, which was developed in the 1930's, is PERCEPTUAL CONTROL THEORY (PCT). Perceptual Control Theory explains how organisms control what happens to them. This means all organisms from the amoeba to humankind. It explains why one organism can't control another without physical violence. It explains why people deprived of any major part of their ability to control soon become dysfunctional, lose interest in life, pine away and die. It explains what a goal is, how goals relate to action, how action affects perceptions and how perceptions define the reality in which we live and move and have our being. Perceptual Control Theory is the first scientific theory that can handle all these phenomena within a single, testable concept of how living systems work.

William T. Powers, November 3, 1991

PERCEPTUAL CONTROL THEORY

While the existence of control mechanisms and processes (such as feedback) in living systems is generally recognized, the implications of control organization go far beyond what is generally accepted. We believe that a fundamental characteristic of organisms is their ability to control; that they are, in fact, living control systems. To distinguish this approach from others using some version of control theory but forcing it to fit conventional approaches, we call ours Perceptual Control Theory, or PCT.

PCT requires a major shift in thinking from the traditional approach: that what is controlled is not behavior, but perception. Modelling behavior as a dependent variable, as a response to stimuli, provides no explanation for the phenomenon of achieving consistent ends through varying means, and requires an extensive use of statistics to achieve modest (to the point of meaningless) correlations. Attempts to model behavior as planned and computed output can be demonstrated to require levels of precise calculation that are unobtainable in a physical system, and impossible in a real environment that is changing from one moment to the next. The PCT model views behavior as the means by which a perceived state of affairs is brought to and maintained at a reference state. This approach provides a physically plausible explanation for the consistency of outcomes and the variability of means.

The PCT model has been used to simulate phenomena as diverse as bacterial chemotaxis, tracking a target, and behavior in crowds. In its elaborated form, a hierarchy of perceptual control systems (HPCT), it has lent itself to a computer simulation of tracking, including learning to track, and to new approaches to education, management, and psychotherapy.

Control systems are not new in the life sciences. However, numerous misapprehensions exist, passed down from what was learned about control theory by non-engineers 40 or 50 years

ago without further reference to newer developments or correction of initial misunderstandings. References in the literature to the desirability of positive feedback and the assertion that systems with feedback are slower than S-R systems are simply false, and concerns about stability are unfounded.

The primary barrier to the adoption of PCT concepts is the belief - or hope - that control theory can simply be absorbed into the mainstream life sciences without disturbing the status quo. It is very hard to believe that one's training and life work, and that of one's mentors, and their mentors, must be fundamentally revised. Therefore, PCT appeals to those who feel some dissatisfaction with the status quo, or who are attracted to the idea of a generative model with broad application throughout the life sciences (plus AI and robotics). There are very few people working in PCT research. Much of its promise is still simply promise, and it meets resistance from all sides. It is frustrating but also tremendously exciting to be a part of the group who believe that they are participating in the birth of a true science of life.

Mary Powers, November 1992

THE EVOLUTION OF THE CONTROL PARADIGM

The PCT paradigm originates in 1927, when an engineer named Harold Black invented the negative feedback amplifier, which is a control device. This invention led to the development of purposeful machines. Purposeful machines have built-in intent to achieve consistent ends by variable means under changing conditions.

The discovery and formalization of the phenomenon of control is the first alternative to the cause-effect perspective ever proposed in any science.

The first discussion of purposeful machines and people came in 1943 in a paper called: Behavior, Purpose and Teleology by Rosenblueth, Wiener and Bigelow. This paper also argued that purpose belongs in science as a real phenomenon in the present. Purpose does not mean that somehow the future influences the present.

The first specific suggestion on how to use the concept of control to understand people came in 1957 in a paper entitled: A General Feedback Theory of Human Behavior by McFarland, Powers and Clark.

In 1973 William T. (Bill) Powers published a seminal book called "Behavior: the Control of Perception," which still is the major reference for PCT. See literature below.

This book spells out a complete model of how the human brain and

nervous system works like a living perceptual control system. Our brain can be viewed as a system that controls its own perceptions. This view suggests explanations for many previously mysterious aspects of how people interact with their world.

Perceptual Control Theory has been accepted by independently thinking psychologists, scientists and other interested people. The result is that an association has been formed (the Control System Group), several books published, this CSGnet set up and that at latest count 16 professors are teaching PCT in American universities today.

DEMONSTRATING THE PHENOMENON OF CONTROL

The phenomenon of control is largely unrecognized in science today. It is not well understood in important aspects even by many control engineers. Yet the phenomenon of control, when it is recognized and understood, provides a powerful enhancement to scientific perspectives.

It is essential to recognize that this phenomenon exists and deserves an explanation before any of the discourse on CSGnet will make sense.

Please download the introductory demonstration dem1a.exe, which is an interactive program and/or rubberbd.txt, which is a text telling you how to demonstrate the phenomenon to yourself and a friend using only two rubber bands.

THE PURPOSE OF CSGnet:

CSGnet provides a forum for development, use and testing of PCT.

CSGnet PARTICIPANTS

Many interests and backgrounds are represented here. Psychology, Sociology, Linguistics, Artificial Intelligence, Robotics, Social Work, Neurology, Modeling and Testing. All are represented and discussed. As of December 1992 there were over 130 individuals from 18 countries subscribed to CSGnet.

ASKING QUESTIONS

Please introduce yourself with a statement of your professional interests and background. It will help someone answer if you spell out which demonstrations, introductory papers and references you have taken the time to digest.

THE CONTROL SYSTEMS GROUP

The CSG is an organization of people in the behavioral, social, and life sciences who see the potential in PCT for increased understanding in their own fields and for the unification of diverse and fragmented specialties.

Annual dues are $45 for full members and $5 for students (subsidized).

An annual meeting is held in Durango, Colorado, on the campus of Fort Lewis College. In 1993 it will begin in the evening of Wednesday, July 28, and end Sunday morning, Aug, 1. There will be 7 plenary meetings (mornings and evenings), with afternoons, mealtimes, and late night free for further discussion or recreation. Full details will be available on the net or by mail after April 1, 1993.

Net subscribers find it useful to have thematic collections of some of the network discussions, and it enables non-net members to keep up with them. Threads from this net are published on a quarterly basis in a booklet called the Closed Loop. These booklets are distributed to members and are available separately. A complimentary copy of Closed Loop will be sent upon request. Back issues are available: Volume 1 (4 issues) is $12. Single issues of Volume 2, beginning with Jan. 1992, are $6 each.

For membership information and back issues of Closed Loop, write: CSG, c/o Mary Powers, 73 Ridge Place CR 510, Durango, CO 81301-8136.

SUBSCRIBING TO CSGnet

When you subscribe to CSGnet, you get this message, CSGINTRO.DOC. But you may have received it from a friend who printed it, seen it on a demodisk, or seen it on Usenet. To subscribe, send a message as follows: (Internet address followed by two message commands, one per line)

```
LISTSERV@VMD.CSO.UIUC.EDU
Subscribe CSG-L Lastname, Firstname, Affiliation, City, State.
help
```

(Lastname, Firstname, etc is optional commentary, but helpful).
("help" requests a list of most commonly used commands).
(The Bitnet address is: LISTSERV@UIUCVMD).
(This server is not sensitive to upper or lower case letters).

CSGnet can also be accessed via Usenet (NetNews) where it is listed as the newsgroup bit.listserv.csg-l.

For more information about accessing CSGnet, contact Gary Cziko, the network manager, at G-CZIKO@UIUC.EDU

HOW TO OBTAIN TEXT AND PROGRAM FILES

A number of ASCII documents and binary computer programs are available on a fileserver maintained by Bill Silvert. It is possible to download all these files via e-mail. If you are on internet, it is easiest to obtain binary program files via anonymous FTP. If you are on MCI mail, you have read about how you can transfer binary files with Kermit or Zmodem protocols. (Type help at the MCI mail prompt for directions). But the server cannot send binary files over the internet mail network, so download uue.scr first, then request the binary files uuencoded as ASCII files. The Internet address for the server is BIOME.BIO.NS.CA. CSGnet files are kept in the subdirectory pub/csg.

To get basic information and a current listing of available documents, send a message as follows: (Internet)

```
To:         SERVER@BIOME.BIO.NS.CA.

Commands:   help
            ftp
            get csg/Index
            end
```

```
"help"            requests commands and explanations.
"ftp"             asks details on anonymous FTP for internet.
"get csg/Index"   requests the Index for the csg subdirectory.
```

Pay attention to letter case for commands! DOS is not dos.

As part of the index (of the csg directory), you may be looking at:

```
programs/msdos:
dem1a.exe   128437  Bill Power's demonstr of perceptual control
dem2a.exe   123649  Bill Power's modelling of control

documents/forssell:
uud.scr      53406  ASCII Compile uud.exe w DOS debug Dir @ end.
```

If you want dem1a.exe (uuencoded) to get a "live" demonstration of the phenomenon of control, and the ASCII file uud.scr with directions at the end on how to use DOS debug to compile uud.exe to decode it, send the following message commands:

```
uue csg/programs/msdos/dem1a.exe
get csg/programs/forssell/uud.scr
```

The uuencoded dem1a.exe will be sent in four parts. Remove headers and use an editor to make it into one file (starting with table and ending with end) before you use uud.exe to restore the file. dem1a.exe is a self-extracting archive file. Put it in it's own directory before you execute it. You get complete documentation and a running program.

LITERATURE REFERENCES

For a complete list of CSG-related publications, get the file biblio.pct from the fileserver as described above. Here are some selected books and papers on Perceptual Control Theory:

Powers, William T. (1973). _Behavior: The control of perception_. Hawthorne, NY: Aldine DeGruyter.
The basic text.

*Robertson, Richard J. and Powers, William T. (Eds.). (1990). _Introduction to modern psychology: The control theory view_. Gravel Switch, KY: CSG Book. ($25 postpaid)
College-level text.

*Powers, William T. (1989). _Living control systems: Selected papers_. Gravel Switch, KY: CSG Books. ($16.50 postpaid)
Previously published papers, 1960-1988.

*Powers, William T. (1992). _Living control systems II: Selected papers_. Gravel Switch, KY: CSG Books. ($22 postpaid)
Previously unpublished papers, 1959-1990.

*Marken, Richard S. (1992). _Mind readings: Experimental studies of purpose_. Gravel Switch, KY: CSG Books. ($18 postpaid)
Research papers exploring control.

Marken, Richard S. (Ed.). (1990). Purposeful Behavior: The control theory approach. _American Behavioral Scientist_, _34_(1). (Thousand Oaks, CA: Sage Publications)
11 articles on control theory.

Runkel, Philip J. (1990). _Casting nets and testing specimens_. New York: Praeger.
When statistics are appropriate; when models are required.

Hershberger, Wayne. (Ed.). (1989). _Volitional action: Conation and control_ (Advances in Psychology No. 62). NY: North-Holland.
25 articles (not all PCT)

Ford, Edward E. (1989). _Freedom from stress_. Scottsdale AZ: Brandt Publishing.
A self-help book. PCT in a counseling framework.

Gibbons, Hugh. (1990). _The death of Jeffrey Stapleton: Exploring the way lawyers think_. Concord, NH: Franklin Pierce Law Center.
A text for law students using control theory.

McClelland, Kent. (1992). _Perceptual control and sociological theory_. Not yet published. Available from the author,
Grinnell University, Grinnell, Iowa.

McPhail, Clark. (1990). _The myth of the madding crowd_.

New York: Aldine de Gruyter.
     Introduces control theory to explain group behavior.

McPhail, Clark., Powers, William T., & Tucker, Charles W.
(1992). Simulating individual and collective action In temporary
gatherings. _Social Science Computer Review_, _10_(1), 1-28.
     Computer simulation of control systems in groups.

Petrie, Hugh G. (1981). _The dilemma of inquiry and learning_.
Chicago: University of Chicago Press.
     Introduces PCT to educational theory.

Richardson, George P. (1991). _Feedback thought in social
science and systems theory_. Philadelphia: University of
Pennsylvania Press.
     A review of systems thinking, including PCT.

 *These items are available from CSG Books, 460 Black Lick Road,
  Gravel Switch, KY, 40328.

Date: Wed Dec 02, 1992 3:46 pm PST
Subject: Re: Controlling error

[From Audra Wenzlow (92.12.02)]   Rick Marken --

The notion of "controlling perceptions" has always disturbed me precisely because it does not include the reference signal in its description.

>If, however, we watched a perceptual signal and its reference speci-
>fication, the behavior would be far more interesting -- the percpetion
>would track rather precisly the any variations in the value of
>the reference;

It seems to me that what you are saying is that we control the difference between the perception and the reference -- which is exactly what I mean by controlling error. Without a reference setting, controlling a perception would have no meaning.

For instance, in the rubber band experiment, I don't really care where the knot is, only how far I perceive it to be from the reference point. In other words, I am not controlling my perception, but the difference between my perception and my reference level. The fact that controlling occurs relative to some reference setting is precisely what seems to be disregarded when we say that we "control our perceptions."

I understand that how I see this error is also a perception, but the distinction between "the perception of how far away my perceptions are from my reference signal," and my perceptions themselves should be made. (I had to read that over again a few times as well.)

By controlling the difference (or error), we automatically seem to be controlling our perceptions, because the error is a function of only this perception and the reference level. If you control the error -- you control the perception, but not purposively. So in the rubber band example, the person seems to be controlling for "where" the knot is, but is truly controlling for how far away the knot is from his/her reference.

I agree that it is necessary to stress the notion of "perception" vs. objective reality in PCT. However, talking about "controlling perceptions" seems similar to talking about controlling others -- it is displacing where the actual purposive control occurs (or only telling part of the story).

Sorry about the trouble -- only trying to control my errors.

Audra Wenzlow

[Background info: I am a graduate student in Educational Policy Studies at the UofI. My background is in statistics. I am in one of Gary Cziko's classes and am also sitting in on one taught by Clark McPhail. I have been listening to the net most of this fall semester.]

Date: Wed Dec 02, 1992 3:49 pm PST
Subject: Moral relativism; uncontrolled perceptions; Wolfgang's program

[From Bill Powers (921202.0830)] Rick Marken (921201.1300) -

Moral relativism, I think, is a problem for people with conflicting system concepts. If a certain practice, for example birth control, is immoral for people who are devoted to one religion but not for others, how are the people for whom it is immoral to think about those for whom it is not? Many people take the moral precepts that go with their system concepts as absolutes (or think they do). After all, if it is absolutely immoral for you to practice birth control, how can you be "tolerant" of other religions that preach no such prohibition? How can something that's absolutely wrong for you to do be OK for others to do? This is the problem with fundamentalism of any kind, isn't it? A true moral absolutist, obviously, considers his or her morals to be the absolutely right ones, and therefore must act to correct deviations from that moral code by ANYONE, not just by those who share the same principles. This requirement clearly conflicts with goals such as getting along with others, respecting their rights, belief in freedom of thought, and so forth. Many people resolve this conflict by saying that others are free to do as they wish, but that their violations of the moral precepts will catch up with them later -- God, for example, will get even with them after they're dead. Some consider it their duty to try to save others from their sins -- persuade them to change their moral codes. But most just ignore this problem until something forces them to experience the conflict.

The problem with moral absolutism is that nobody is actually able to carry it off. Consider a moral precept that is chiselled in stone: Thou Shalt Not Kill. In a normal social context this principle is apparently accepted by all right-thinking people. But there have been very few people whose system concepts allow them to adhere to this principle when their property or lives are threatened or when their country goes to war or when murderers are caught. Or when they want a Big Mac. Then this principle is revealed as a variable, for which we set the reference level sometimes high and sometimes low, depending on circumstances. Circumstances make moral relativists of us all.

Martin Taylor (921130.1340) --

>If ECS A provides as its output reference signals for ECSs X, Y, Z,
>..., and they provide the desired percept (perfectly, by
>hypothesis), how can the percept of A be uncontrolled?

Most of the world that we experience is uncontrolled. Consider controlling for a relationship such as "following the leader." You have control over your position in

space, but not the leader's position. When the leader moves, you can maintain the relationship called "following" only by moving yourself. You must perceive the leader's position in order to control this relationship, but you can't affect the leader's position by your own actions.

Most social perceptions involve perceptions of variables you control directly and perceptions of what other people are doing, which you can't control. Even holding a conversation with someone is impossible unlessyou adjust what you can control (what you communicate) to fit with what the other is controlling (what the other intends to communicate).

Even in interacting with the physical world, there are variables you can perceive but not control. When you take a drink out of a glass, the liquid level falls. There is nothing you can do to alter that relationship between your action and its effect on the perception of the liquid level. You just have to learn to take it into account in your control processes.

Once you see a few examples of this, I'm sure that many more will occur to you.

>I didn't want to move the input to the PIF from the lower-level
>perceptual signal to the lower-level error signal, because to do so
>seems to me inherently less accurate than using the perceptual
>signal directly.

At the higher levels, where I think this arrangement would be more likely than at the lower, we have already agreed that a great deal of imaginary information is involved in controlling most perceptions. I'm simply carrying that idea to its logical conclusion, by making the imaginary part (the model) an explicit feature of control, and envisioning a continual process of updating the model. It's easy to show that if the model is good, the arrangement I proposed (last winter?) is exactly equivalent to the "canonical" model. The upper level perceives the output of the model PLUS the lower-level error signal. The lower- level error signal is exactly a measure of how far from correct the model is. So if you perceive the sum of model behavior and error, the result is just like perceiving the lower-level perceptual signal.

When Wolfgang's model is distributed, we can try out this concept.

Dick Robertson (921201.1800) --

You forgot the hard carriage returns again -- here's what I got of your
post:

To Bill Powers - My condolences about the Science art. rej'n. I saw
recently wh
new csg list? Thanks, Dick

Wolfgang Zocher has agreed to make his program public now so it can be used on an interim basis. I'll post it to Bill Silvert's server as Turbo C code, and also as a self-extracting .EXE file for PCs. We will do everything we can to help people get this program on their own computers and learn to run it.

Best to all,   Bill P.

Date: Wed Dec 02, 1992 3:50 pm PST
Subject: Re: Controlling error

>[From Gabriel to NET 921202:1447CST] in re >(Audra Wenzlow (92.12.02))

>The notion of "controlling perceptions" has always disturbed me
>precisely because it does not include the reference signal in its
>description.

Yes, it's worried me too at times. I think there are several variants, for example, when I have a set of "percepts" whatever these may be, and I want to establish a new one somebody else is trying to explain to me. I don't actually want to make the new one coincide with one of mine if it's really new, I want to "compare and contrast" with what I already know.

I have a lot of technobabble (a WONDERFUL Bill C. phrase) about this, and can decorate it with interminable mathematics, but the bottom line I think I need everybody to recognise and integrate with their own percepts (how about that for recursion) is just the preceding paragraph.

Not quite error reduction, but distance quantification perhaps. And I think everybody has different mappings to measure distance, just like the difference between Mercator and stereographic projection in the Atlas. Mercator is OK for compass bearings, but it's more wrong about distances the further north you go.

And so, the further you go from pure mathematics and theoretical physics or computer science and electrical engineering, the more likely I am to be wrong.

I don't want to be publicly invidious about any of my other friends on the net (I hope I have no enemies). Quot Homines, tot Sententiae. You all know who you are. We all try to understand each other's mappings, but this can often be done only to within some rather large epsilon.

And working too hard at it can either/both disturb conduct of the serious business of finding out new things, or lead to interminable discussion of how many angels can be perceived to dance on the head of a pin.

But on the other hand, ignoring the differences always generates lots of heat and little light - perhaps just a smoldering resentment (I've never seen this on the NET, perhaps we REALLY ARE a community of scholars!!).

Sincerely John Gabriel (gabriel@caesar.eid.anl.gov)

Date: Wed Dec 02, 1992 5:36 pm PST
Subject: Re: Adekola

(ps 921203.1400) [From: Bruce Nevin (Tue 92111 10:54:35)]

> But the raison d'e^tre of SID is to interact with people,
> so if left alone the computer directs it to move in a
> seductive manner, what Adekola calls its "luring" mode.

as the list's expert on flirting (not!), i'm fascinated by what it would mean for an object to be flirtatious. (i'm not doubting that it can be. i'm interested in knowing how we know.)

--penni

Date: Wed Dec 02, 1992 6:46 pm PST
Subject: Where are reference signals and error signals?

[From Bill Powers (921202.1730)] Audra Wenzlow (921202)--John Gabriel (921202)

The puzzle you are working on is the same one I went through in developing the definitions of perceptions in the hierarchy. I expect that Rick Marken will be along with a message similar to mine shortly.

Audra says:

>For instance, in the rubber band experiment, I don't really care
>where the knot is, only how far I perceive it to be from the
>reference point. In other words, I am not controlling my
>perception, but the difference between my perception and my
>reference level.

When you describe it this way, it seems that the knot's position is the controlled variable, while the position of the spot where you are keeping the knot is the reference signal. This creates a puzzle, because in the HPCT model reference signals pass downward from higher systems into the comparators of lower systems, and don't come in through the senses. Only perceptions originate in the senses. So is a reference signal also a perceptual signal? This leads immediately to

>I understand that how I see this error is also a perception, but
>the distinction between "the perception of how far away my
>perceptions are from my reference signal," and my perceptions
>themselves should be made.

So now we also have error signals originating in the senses and becoming perceptual signals. If you try to draw the control-system diagram so that not only perceptions, but reference signals and error signals are inputs from the environment, you will soon end up in a conceptual mess.

This is not a trivial problem; you are astute to have pursued it to this point.

Behind my answer to it lies a fundamental postulate of HPCT, which is that the world we experience exists ONLY in perceptual signals. We do not perceive error signals. We do not perceive reference signals. The original reason for proposing the imagination connection was precisely to provide a way to get the information in a downgoing reference signal, which is not perceived, into the perceptual channels where all perception takes place. Only in this way can we maintain consistency with the postulate that all experience is of the perceptual signals, and still explain how we sometimes -- I stress sometimes -- can know the reference condition directly. And I believe that this postulate is essential in maintaining the overall consistency of the model with experiment and experience.

If the postulate is true, then the target spot where the knot is supposed to be is not a reference signal. It is a perception. So is the position of the knot; that's another perception. The answer to the puzzle is now staring you in the face. To show you what it

is, all I have to do is change the instructions (or you can do it yourself): keep the knot 4 inches to the right of the spot.

This makes it clear that you are not controlling the knot in isolation. You are perceiving both the knot and the spot, and you are controlling a RELATIONSHIP between them. The reference condition for this relationship, one assumes, often unconsciously, is "knot over spot." But that is just one possible state of the relationship; if you always pick that relationship, you will not realize that other reference- relationships are possible, and the role of the spot will seem ambiguous. In fact, the reference condition can be any state of the knot-to-spot spatial relationship.

When you pick a target position like "knot 4 inches right of the spot", you can now realize that you do not perceive the reference relationship. You perceive only the actual relationship. If the knot is 8 inches right of the spot, that is what you perceive, and nothing else. You do not see a knot that is 4 inches to the right of the spot; the only knot you see is 8 inches to the right of the spot (alternatively, the spot is 8 inches left of the knot). You "know" somehow that the knot and spot are too far apart (or too close together), but you have no picture of the correct relationship in your perceptions. This sense of "knowing" that what you see isn't "right" is as close as you will get to perceiving the reference signal or the error signal with your eyes open. The only way to get closer is to close your eyes and visualize the knot in the relationship you mean by "four inches to the right of the spot." Now the reference signal is routed into the perceptual channels, and you are perceiving the reference condition. Not everyone can do this easily; some people seem unable to do it at all with visual images.

As soon as you open your eyes, the imagined relationship is replaced by the real one; the knot is now too far from the spot. You are no longer imagining the reference condition, but perceiving the actual "wrong" condition. As you act, the sense of wrongness diminishes and finally vanishes -- but you are never perceiving anything but the actual relationship.

This is why we have a model. The reference signal and error signal in the model are not part of experience. They are an explanation for how action and experience come to be related as they are. We can't verify their existence by looking at experience, because all that experience contains is a perceptual report on the actual current state of affairs. We can only test the conceptual structure of the control system indirectly, by showing that it accounts for what we observe. Once in a great while we can trace out some neural circuits, like those of the spinal reflexes, and show that the physical architecture is consistent with the model.

Relationships are the most difficult types of perceptions to understand in the PCT model, because when we think of how the model works, we are using our own relationship perceptions very heavily. The process of comparison involves a relationship between the perceptual and reference signals: perception smaller than, equal to, or greater than the reference setting. But that relationship is detected automatically, outside the purview of direct experience. It occurs at EVERY level, notjust the relationship level; spinal motor neurons carry out this process of comparison while controlling mere intensity signals. We have to distinguish carefully between the behavior of the model, all parts of which we view in the mind's eye using all our natural levels of perception, and our experiences of the world and our actions on it, which we view directly. Just remember that in this model all experience is perception, and all perception is the output of a sense-organ or a higher input function. Reference signals move in the opposite direction, outward or downward, and save for imagination do not appear in the perceived world.

It's ALL perception.

Best,   Bill P.

Date: Wed Dec 02, 1992 10:43 pm PST
Subject: Philosophy of Science

[From Dag Forssell (921202)]

I am working on a presentation to show how PCT can supplement the Deming Management Philosophy (where it falls short). To that end, I reviewed Deming's discussion of "Theory of knowledge."

In a footnote, Dr. Deming says: "My favorite treatise on this subject is Clarence Irving Lewis, _Mind and World order_ Shribners, 1929. Reprinted by Dover Press, New York. My advice to a reader is to start with ch. 6, 7 or 8, not with page 1." He provides no other reference.

QUESTION: Has anyone on the net any knowledge or opinion on this text? I guess Dr. Deming (now 92) grew up and formed his opinions before Thomas S. Kuhn was born. Much of his teaching sounds like Popper. ----------

Brief note to Gary: Thanks for pipsqueak. I appreciate feedback. I looked for your paper in today's posting of intro. Guess it is too soon. When you do make it available, will you post an electronic version? If by request only, please send it my way.

Best to all, Dag

Date: Thu Dec 03, 1992 1:01 am PST
Subject: Re GROUND TRUTH - Specially for Bill C.

[From Gabriel 921203.0231CST to NET and Bill C.]

Yea, THERE AINT NO SUCH THING AS GROUND TRUTH, But, perhaps I can tell you what there is:-

A monotonic increasing (or at least non decreasing) set of propositions about the real world, and a set of Bayesian estimates of confidence about same. I'd like to say something about increase of confidence in truth or falsity, but I can't quite. But I can use Caratheodory's Theorem - Zeits f. Physik about 1910 - ref in Margenau & Murphy Math of Physics & Chemistry about 1945 (can't find my copy) to say that total Shannon Info accumulated does not decrease - you may mislay information, but you can't get rid of it. Not absolutely sure of preceding sentence, but bottom line seems to me that we have less to worry about from Bishop Berkeley than we thought - the tree may cease to be when noone's about in the quad, but that doesn't matter our knowledge of the tree certainly persists. A REAL philosopher's view about intelligence (the nominally oxmoronic, but actually very real Army kind). How about that for epistemology!!! Note by the way the Margenau & Murphy is not lost, only mislaid. Uh Oh - what if somebody took and burned the book??, and after my brain cells and those of everybody else reading this msg have returned to their elemental constituents? Well, I suppose there's still some philosophy to do after all, and the good bishop was really right. But I don't care - it really is suffiecient for most purposes that the knowledge of the tree persists, even if somebody chopped it down last night and burned it. That's just another Bayesian truth. I suppose

we get back in the long run to Diodorus - Does a stone at the bottom of the ocean that has not and never will be seen exist?? - sound of one hand clapping etc.

John G. (gabriel@caesar.eid.anl.gov)

Date: Thu Dec 03, 1992 11:26 am PST
Subject: Deja vu all over again

Hey Cunningham, don't you think we've had enough of the complex jokes?
Too much tension among the poles always leads to chaos.

Date: Thu Dec 03, 1992 5:38 pm PST
Subject: error control

The following message was sent 2 days ago but did not get posted to the net--a more up to date reply will come later but this post is relevant to Rick's comments.

Rick,
Interesting point about keeping the "perceptual control" idea for the sake of rigor and aesethetic concerns. I suppose it is true that error signals are not as interesting.

But let me be sure you know what I am saying. First, saying that we primarily control error does not take anything away from PCT at all; all the convincing graphs that you speak of are just as relevant--the model is the same--it still behaves exactly the same. I am not claiming that perceptions are not controlled--I still contend that this is true.

What I am saying is that perceptions are controlled (or perceptions are brought under control) by virtue of the fact that the Organism controls its error levels. The Organism controls error. As a necessary result, percpetions are controlled. I don't want to deemphasize the importance of the perceptual control idea for it is powerful in relation to traditional ideas of output control. But it should all be understood in the context that the organism knows nothing about perceptions. It knows only about error and controls its perceptions because over time the control of those perceptions has resulted in a less error-ridden state of being. Sure we experience these perceptions, but the organism does not Know about them, nor does it really "care" about them--it cares about error.

You said that "Zero-error" settings are implicit, but not truly specified? I don't know if this is correct--actually I doubt it: referencing the error states to zero must be accomplished somehow neurobiologically. But even if this were not the case, the fact that these error states Are uninteresting (that they stay near zero) IS impressive (even if it isn't as interesting).

Perceptions are controlled because organisms control error.

Mark "It is impossible to do only one thing."

Date: Thu Dec 03, 1992 5:46 pm PST
Subject: retry to Bill P

[from Dick Robertson]

No, I didn't forget about carriage returns. THere is something wrong with this stupid mailing program. But let me try again. Please send us the new arm demo c/o me at northeastern Il U 5500 n. St Louis 60625. The dept will scare up the money. Also please send me your private e-mail address. Hope this gets through.
Best, Dick

Date: Thu Dec 03, 1992 6:34 pm PST
Subject: Unknown perceptions?

[From Bill Powers (921203.1630)] Mark Olson (921203.1519) --

> But it should all be understood in the context that the organism
>knows nothing about perceptions. It knows only about error and
>controls its perceptions because over time the control of those
>perceptions has resulted in a less error-ridden state of being.
>Sure we experience these perceptions, but the organism does not
>Know about them, nor does it really "care" about them--it cares
>about error.

How is Knowing (with a capital K) different from knowing? What is different between an error to which you're not paying attention (like the one that's responsible for turning your eyes as you read) and an error you "know" about?

Saying that the organism knows nothing about perceptions, even though "we experience these perceptions," is pretty bold: it denies the whole world that you see, hear, feel, taste, and smell, which seems to me pretty much the point of it all. It also means that we don't "know" what we experience -- or something. Come to think of it, what the hell DO you mean? Do we experience our knowledge of our perceptions, or perceive our experiences of our knowledge, or ... ??? I have a feeling of being smothered in soft fluffy words.

Error is the signal that represents the discrepancy between perception and reference. You can't very well have an error signal without a perception and a reference signal, and it's surely the reference signal that determines whether a given level of a perception constitutes an error -- and if so, how much and of what sign. The error signal depends on perception and reference. The perception depends on error, the action driven by error, and external disturbances. This is a closed loop. You can't boil it down to a single most important component. Slogans aside, a control system controls, which is a process that involves perception, reference, comparison, and action, plus the external part of the loop. Take away any one of those and you don't have control any more.

When you talk about "the organism," remember that you're talking about yourself.

Best, Bill P.

Date: Thu Dec 03, 1992 11:25 pm PST
Subject: RE: Unknown perceptions?

From Tom Bourbon [921204.0105] Bill Powers [921203.1630]

I share your befuddlement at Mark Olson's [921203.1519] post on "Knowing" error, but not "knowing" perceptions. In fact, I have been following the entire flurry of posts about "controlling error," trying to find a place to grab hold and post a reply, but I haven't figured out what people are trying to accomplish with that thread. Are those who say

PCTers should talk about "behavior: the control of error," doing so out of a desire to make PCT more palatable to traditional behavioral scientists? If so, forget it. Nothing you can do at the level of slogans will help. In fact, there is an entire community of people known as "control theorists" who make a specialty out of sugar coating control theory, presenting it as just another "framework" or "perspective" to bake into the big ecclectic pie that is psychology. (I discussed that group in posts a few weeks back, as part of the thread on 'Why 99%?') All they have done is degrade the PCT model and create a widespread impression that it is not really different from anything else in psychology. Those popularizers are so successful that editors and reviewers cite them as part of the justification for rejecting manuscripts in which we use the PCT model to actually model behavior and its consequences.

To the "control of error" group, is that your concern -- selling PCT more effectively? If so, that is a grand undertaking and I wish you success; but don't sacrifice the core of the model in the process.

Regards to all, Tom Bourbon

Date: Fri Dec 04, 1992 4:55 am PST
From: Hortideas Publishing / MCI ID: 497-2767

TO: * Dag Forssell / MCI ID: 474-2580
Subject: Ford & Ford books

From Greg Williams (921203 - direct)

Hi Dag,

Just yesterday I FINALLY made it to the UK library to get the Ford references which I promised so long ago. I apologize for the delay, and for not commenting on your several recent posts -- I assure you that you are not being singled out, as personal busyness has been preventing me from ANY net discussion.

The refs:

Donald H. Ford [Penn State], HUMANS AS SELF-CONSTRUCTING LIVING SYSTEMS: A DEVELOPMENTAL PERSPECTIVE ON BEHAVIOR AND PERSONALITY, Lawrence Erlbaum Associates, Publishers, 365 Broadway, Hillsdale, NJ 07642, 1987, xiv + 787 pp., ISBN 0-89859-666-1/0-8058-0017-4 pbk.

Martin E. Ford [Stanford] and Donald H. Ford, eds., HUMANS AS SELF-CONSTRUCTING LIVING SYSTEMS: PUTTING THE FRAMEWORK TO WORK, Erlbaum, 1987, xiv + 409 pp., ISBN 0-89859-667-X/0-8058-0193-6 pbk.

I'd be interested in what you think of these books.

Best wishes, Greg

Date: Fri Dec 04, 1992 6:03 am PST
Subject: Little Man: missing file

[From Bill Powers (921204.0700)]

For those who have the Little Man program version 2 and wish to recompile it from scratch: there is a missing .obj file called RAWHANF.OBJ This is used only for reading a joystick, but the program won't compile without it. The assembler source code is included, however.

Here is the missing file uuencoded. If you need this file and can't uudecode this, drop me a line and I'll send you a corrected disk right away -- specify size.

section 1 of uuencode 4.13 of file RAWHANF.OBJ   by R.E.M.

```
begin 644 RAWHANF.OBJ
M@!4`$UQ40UQ!4TU<4D%72$%.1BY!4TVVB"````!4=7)B;R!!<W-E;6)L97(@+
M(%9E<G-I;VX@,2XP,8B(&P!`Z>1X,1<37%1#7$%335Q205=(04Y&+D%33=N(-
M`P!`Z4R6`@``:(@#`,">%Y83``Q205=(04Y&7U1%6%0$0T]$18&8!P!('P`"!
M`P'TE@P`!5]$051!!$1!5$'"F`<`2```!`4!#Y8(``9$1U)/55"+F@0`!O\"C
M6Y`.```!!U]R87=H86X```!ZB`4`0.$<`#:(!`!`H@&1B`4`0.H$`$6("`!`W
MXQ@`!@`J!8@)`$#C&0```"0`#X@)`$#C&@```"0!#8@+`$#C&P```",````,O
MB`L`0.,<````(P`$``>("P!`XQT````C``$`"8@+`$#C'@```",`!0`$B!P`Y
M0.@`$UQ40UQ!4TU<4D%72$%.1BY!4TWD>#$7VY1#```!"@````L``0`,``,`/
M#0`&``X`"0`/``L`$``,`!$`#0`2`!(`$P`5`!0`%@`5`!@`%@`:`!<`&P`8`
M`!T`&0`>`!2(#`!`Y@1#2$%."@(/D'V(#`!`Y@),4!D```$5`'F($0!`Y@=?C
M4D%72$%.'````0``_:`C``$``%6+[+H!`HIF!C+`^N[W00+__[D``^R$Q.#[F
+^XO!7<MQB@(``'0`8
``
end
sum -r/size 54778/741 section (from "begin" to "end")
sum -r/size 51491/506 entire input file
```

Best, and sorry -- Bill P.

Date: Fri Dec 04, 1992 6:48 am PST
Subject: foggy day

[From: Bruce Nevin (Fri 92124 09:10:27)]

(m-olson@UIUC.EDU Thu, 3 Dec 1992 16:18:00) --
(Bill Powers (921203.1630)) -- (Tom Bourbon [921204.0105]) --

I too palpably felt an epistemological fog settling in as I read

```
/*************************************************************************
*                                                                        *
* But it should all be understood in the context that the organism       *
* knows nothing about perceptions.  It knows only about error and        *
* controls its perceptions because over time the control of those        *
* perceptions has resulted in a less error-ridden state of being.        *
* Sure we experience these perceptions, but the organism does not        *
* Know about them, nor does it really "care" about them--it cares        *
* about error.                                                           *
*************************************************************************/
```

But it is useful to present new ideas in different ways and from different angles, as Bill perhaps more than any has demonstrated again and again in his writing here. Not merely useful, even necessary. To "get it" subjectively, to really connect the

subjective here/now me this is it experience that I have (you have, the person you are talking to has) with PCT it is perception and not error that you "know about" and "care about". And until that occurs and pointing out something to someone becomes an even greater marvel, in the act of pointing, than the thing pointed at <oops! recursion alert! :-) > some basic revaluation of system concepts has not yet taken place, some revelation, conversion, recognition, assimilation, integration, call it what you will. But on the way to that epiphany can be many steps, and for some people introducing the signals in the ECS model might be helped by saying "in an important sense we could say that the organism controls error, though it sure feels to us as though it is perceptions that matter." As to what matters most, an argument could be made that it is the reference perception that is the focus (of attention? As Poo would say, `Oh help!'), and that real-time perceptual input becomes apparent to us only by way of error. I'm not sure how well that would stand up (more epistemological fog), but it seems at least as plausible.

Bruce bn@bbn.com

Date: Fri Dec 04, 1992 7:08 am PST
Subject: Little Man: no missing file

It turns out that the "missing" file isn't used. To fix up the compiling, edit the Project file arm.prj to remove the line referring to RAWHANF.OBJ.

Double sorry, Bill P.

Date: Fri Dec 04, 1992 7:08 am PST
Subject: foggy bottom

[From: Bruce Nevin (Fri 92124 09:19:02)]

/***
* an argument could be made that it is the reference *
* perception that is the focus *
* and that real-time perceptual input becomes apparent *
* to us only by way of error. I'm not sure how well that would *
* stand up (more epistemological fog), but it seems at least as *
* plausible. *
***/

Scratch this last. I evidently went under. As Poo would say, "Oh Help AND Bother!" But perhaps it remains true that it is at least as plausible as making error the focus (of attention ? . . .)

It appears that when one is too hasty and distracted to pay attention (to perceptual input) there can be no error signals (wrt that perceptual input). No gain, no pain, I suppose.

Bruce bn@bbn.com

Date: Fri Dec 04, 1992 10:15 am PST
Subject: RE: foggy day

From Tom Bourbon [921204.1010]

Bruce Nevin [921204.0910] -- nicely put! My declaration of defuddlement was not intended as a criticism of people who are posting about "control of error." I often -- very often -- have the same sensation while writing about PCT -- my own words defy my understanding.

It is certainly true that a person can focus on the fact that control systems reduce error to or near zero, then conclude that the systems control error. After all, the numbers are there to see, in any simulation. And Bill's demos (and Rick's spreadsheet) lay those nimbers out for anyone to see, in real time. (Oops. I perceive an error, but I can't get back to it on this editor! But the error I see -- nimber -- is not an error in the environment. Out there, nimber and number are just stuff on a screen -- not even that, if you press the issue. There is error only if and when I decide I should see something otherthan what I do see. Oops. There it is again.)

Tom Bourbon

Date: Fri Dec 04, 1992 11:18 am PST
Subject: PCT: What's important

[From Bill Powers (921204.0830)]

The most important concept of PCT is not control of perception or error, but purposive behavior.

One of the main things that PCT does for us is to get intention and purpose back into theories of life in a simple form. People like Brentano and Searle and many others have tried to deal with intentionality under the assumption that the folk meaning has been disposed of. So they have come up with vague ideas like "aboutness" and "directedness" to explain this sense of doing something on purpose, intentionally. They have purpose (an active process) mixed up with attention (a form of selective but passive observation). I've read a bit of the philosophy of intentionality -- it keeps cropping up -- and my impression is that these people are groping in the dark. Another case of tossing aside the nuggets on the surface and looking at the ground where they were for the obscure answer.

The original meaning of purposive or intentional behavior, the one that conventional science eventually threw out, was that organisms can select a future state of the world and act to bring it into being. We drive cars to work in order to arrive at a parking place and get to work on time. We mix ingredients together with the intention of baking a cake. We write papers for the purpose of publishing them.

All examples of purposive behavior like these have been argued away by scientists who didn't know how to explain them. Part of their difficulty in accepting phenomena of purpose came from flaws in the folk concepts of purpose and intention. People have the idea that if you have a firm and forceful intention, it will be more likely to succeed than if you hold the intention "weakly." I had a dog who had the same idea. When she wanted to go outside, she would stare fiercely at the back door, and later at the doorknob, obviously willing the door to be open as hard as she could. And of course it always worked, eventually, because the humans in the house liked the effort of getting up and opening the door better than the alternative. But the dog evidently never connected what the humans did with what the door finally did. Other dogs would bark or run to find a human being. This one had a different theory of purpose.

From similar sorts of experience, people have got the idea that purposes and intentions have some sort of supernatural effect on the world, even on events that they can't affect by their actions. So this leads to belief in magic of various kinds -- no wonder science was put off by the commonsense concept of purpose.

One of the arguments often brought up against the idea of doing things intentionally (when the debate was still going on) was that intentions don't always work out. Between the "intentional action" and the outcome it was supposed to produce, all sorts of accidents can occur to prevent that outcome. The position against which this argument was presented was assumed to be that purposes and intentions, by their nature, had some magical assurance of succeeding, as per the folk and canine misunderstanding.

Another argument was that the future effects of purposive behavior would have to act backward through time to cause the behaviors that led tothem. Behind this argument was the assumption that all behaviors are caused by external effects on the organism. Purposes were thought of as future events stimulating the organism in the present in a particular way, so as to create the future event. This, too, was assumed to be the folk concept of purpose, and perhaps it was. People are not generally very clear about what they mean by "the future."

The idea of intentional actions carried the implication that there was something different between an intentional action and some other kind of "ordinary" action. In either case, both science and common sense considered behavior in terms of action-events that had future consequences. Do something now; later on something else happens as a result. The implicit concept was that once you're performed a causal act, natural processes are set in motion that can no longer be altered. This concept was behind many objections to purpose, especially objections that raised the issue of unsuccessful purposive acts.

Control theory would suggest that there is no peculiar quality that makes one act purposive and another not purpose. It is not the action, but perception of the outcome that is purposive. Control theory also shows that if some outcome is to be produced on purpose, it is necessary to form an inner perception of that outcome, a reference signal, and act continuously until the perceived outcome matches the reference signal.

Purposes or intentions, as explained by PCT, have no effect on the external world. Only actions affect the external world. A purpose, even though it feels directed outward, is really a specification for input. Only action can alter the perceived world to bring it nearer to the specification (if th4e world doesn't spontaneously cooperate, by luck).

There is no effect of the future on the present; the reference signal is a present-time phenomenon that is carried through time in the actor. Behavior is not a series of events, but a continuous process that is updated as often as necessary to assure progress toward the intended state. The actions performed on the way to achieving a purpose will change with every disturbance that tends to interfere with progress in the right direction. The only way for an intention to fail to be achieved would be for external events too powerful to resist to overcome the organism's maximum opposing effort.

So PCT offers a clear explanation of purpose and intention, one to which the old arguments simply don't apply. Yet PCT also explains how the folk concepts of purpose might have appeared to be true under pardonable misapprehensions about the way the world worked. The mistake made by science was to argue against bad theories of intention and then assume that these arguments got rid of the phenomenon of intention, too. The tacit assumption, also, was that if science in the early part of the 20th Century couldn't

explain the phenomena of purpose, they must not exist. The hubris of our forebears was limitless.

I suggest, therefore, that arguing about whether error signals or perceptual signals or reference signals are the essence of behavioral organization is a side-issue of little importance in comparison with what is really important about PCT. In effect, PCT goes back about 80 years and settles an argument that everyone involved in it at the time settled incorrectly. All the philosophical thinking about purpose that has taken place since then, if it did not include the phenomenon of control, has been vacuous and amounts to nothing. Vast volumes of words on this subject can now be consigned to the wastebasket or the delete key. If we study such words at all in the future, it will only be to learn how people of good intent can use words to fool themselves into thinking they understand something.

Best, Bill P.

Date: Fri Dec 04, 1992 12:15 pm PST
Subject: Where are reference signals and error si

[From Rick Marken (921204.0930)]

Sorry about the paper, Tom and Bill. This is getting pretty boring, no? Again, I say welcome to the club. Now you know what I've been going through for the last 10 years. And you think I'm just naturally grumpy. Hell, I used to be a really nice guy.

Bruce Nevin--

Thanks for reformatting my moral relativism post; that was absolutely great of you.I hope the margins on this baby are better.

Bill Powers (921202.1730) --

>Audra Wenzlow (921202)-- >John Gabriel (921202) --

>The puzzle you are working on is the same one I went through in
>developing the definitions of perceptions in the hierarchy. I expect
>that Rick Marken will be along with a message similar to mine shortly.

Unfortunately, I was out of town until now so I could not provide the requisite redundancy. I certainly could not have said it better.

Now, off to read the rest of the mail.

Regards Rick

Date: Fri Dec 04, 1992 1:28 pm PST
Subject: error control

[From Rick Marken (921204.1030)]

Finished going over the mail. Still no word on status of "Blind men" paper as I thought there might be. Thus, I am not able to rant and stomp about having it rejected -- yet.

Mark Olsen says:

>But let me be sure you know what I am saying. First, saying that we primarily
>control error does not take anything away from PCT at all

I know; I was just suggesting that the idea that error is controlled is just an incorrect description of how a control system works.

>What I am saying is that perceptions are controlled (or perceptions are brought
>under control) by virtue of the fact that the Organism controls its error
>levels. The Organism controls error.

But this is not actually true; if you add a disturbance to the error signal it will not be resisted: it acts like an offset to the reference for the controlled perception (I think I posted the algebra for this some time ago). I have no philosophical problem with the idea that organisms control error -- it's just that, if organisms are control systems, then they just don't control error because that's not how control systems work (at least, given my understanding of the meaning of words like "control", "error", "perception", etc -- they all map into the model, which is the final arbiter of "what's going on").

>But it should all be understood in the context that the organism
>knows nothing about perceptions.

I think you must mean that organisms don't generally understand that their experience IS perception; of course, there are notable exceptions, like epistemologists and PCTers, but it turns out that it's not necessary to know that "it's all perception" in order to control. Most organisms probably just think they're "doing things" -- when, in fact, they are controlling their perceptions.

Best   Rick

Date: Fri Dec 04, 1992 2:01 pm PST
Subject: Moral relativism

[From Rick Marken (921204.1100)]

Well, I see that there was only one reply so far to my moral relativism post.

Bill Powers (921202.0830) says --

>Circumstances make moral relativists of us all.

Thanks. Excellent thoughts, Bill. But, gee, I thought that this topic would stir up some debate -- PCT and morals or something. But nooo.

Well, I am relieved to see that this issue is so non-controversial.
I guess everyone finds the "relativistic" implications of PCT for
moral behavior non-problematic. Good. Back to modelling.

Best   Rick

Date: Fri Dec 04, 1992 2:08 pm PST

Subject: Selection and Consequences

[from Gary Cziko 921204.1701 GMT]

In the course of biological evolution, changes in genotype caused by mutation and genetic recombination have various phenotypic consequences for organisms. These consequences of structure and behavior can be "bad" (death) or "good" (survival and reproduction). Genotypes are therefore selected by their consequences.

But "good" behavior involves being able to resist environmental disturbances (including the behaviors of other organisms). This means being able to vary behavior in order to control the consequences.

Thus evolution (selection BY consequences) leads to control (selection OF consequences) because the latter is in itself a "good" consequence.

Just thinking out loud. I wonder what will be the consequences?--Gary

P.S. a Rick: Il me parece que tu tiens todavia problemes con ton nuevo addresse. Was ist los?

Gary A. Cziko

Date: Fri Dec 04, 1992 2:09 pm PST
Subject: Re: error control

[From Audra Wenzlow (921204)]   (Rick Marken)---

>But this is not actually true; if you add a disturbance to the error
>signal it will not be resisted: it acts like an offset to the reference for
>the controlled perception (I think I posted the algebra for this some
>time ago). ...
>-- it's just that, if organisms are control systems, then
>they just don't control error because that's not how control systems
>work (at least, given my understanding of the meaning of words like
>"control", "error", "perception", etc -- they all map into the model,
>which is the final arbiter of "what's going on").

Would you mind posting the "algebra" again? Maybe this would help me understand your point, as it was exactly the model itself that made me think that we try to control errors instead of perceptions.

Thanks in advance,   Audra

Date: Fri Dec 04, 1992 2:10 pm PST
Subject: RE: foggy day

Error does not and logically cannot exist objectively or independently (unlike, perhaps, some perceptions, but that's a different argument). Error always exists REALTIVE to a given STANDARD: as a DEPARTURE from some asserted, assumed, or given "correct" state. And what is that state? The reference level. So while reference levels, as imagined perceptions, can exist without errors, errors must have RLs to exist; thus RLs are logically prior to errors.

Put another way, an error is a FUNCTION of two arguments: the imagined RL, and the "real world" perception. An epistemology based on error will beg the question as to the nature of the RL and the perception. Thus in PCT (Powers, not Perceptual, Control Theory), it is imagination and perception which must be primitive.

No "epiphany" or "leap of faith" is necessary to reach this understanding. It follows simply from clear thought. Bill's knot shift helps, too.

| Cliff Joslyn,

Date: Fri Dec 04, 1992 3:01 pm PST
Subject: RE: foggy day

[From Audra Wenzlow (921204)] (Cliff Joslyn) --

>Thus in PCT (Powers, not Perceptual, Control Theory), it is
>imagination and perception which must be primitive.

Just to clarify where I stand: I do not disagree with anything you say. The issue did not pertain to whether errors or perceptions are "more primitive" than the other. The question was whether we intentionally try to control our errors (and thus control perceptions -- as would necessarily follow from the relationship of errors, perceptions, and references), or we purposively control our perceptions. In other words, what is it that the control system really cares about: the errors (i.e. the difference between the reference and the perception) or the perceptions themselves. Would a perception be controlled without a reference?

Anyway, I'll drop the subject as it not essential to the model. Just wanted to let you know that as you presumed, I surely didn't need a "leap of faith" to understand your argument.

Thanks, everyone, for the responses to my "purposive controlling" dillema.

Audra

Date: Fri Dec 04, 1992 3:33 pm PST
Subject: Disturbing Error

[from Gary Cziko 921294.2100 GMT] Rick Marken (921204.1030) said:

>if you add a disturbance to the error
>signal it will not be resisted: it acts like an offset to the reference for
>the controlled perception (I think I posted the algebra for this some
>time ago)

I don't remember seeing the algebra, and this does not make sense to me. If error = reference signal - perception, a good control system will keep error near zero. If I add in a disturbance to the error so that, e.g., error = reference signal - perception + 5, then error will still be kept near zero. The system will do what it has to do to get rid of the effect of the +5. How is this not controlling error?--Gary

Date: Fri Dec 04, 1992 4:25 pm PST

Subject: error control-clarify

Tom and others
Sorry it takes a while to reply--I read from the net and write from another program. I wrote some replies in the last few days from home but my modem connections sorta killed them. ANyway...

Am I simply trying to make PCT more palatable to non-PCT'ers? Absolutely not!! I know all about the watering down of control theory and it bothers me also. As I have said before, Nothing is changed in saying "controlling error" and I do not think controlling perceptions is Wrong by any means. What I am saying is that Both are right if it is understood at what level or perspective from which one is speaking. Controlling error is correct at a "deeper" level. Perhaps I should give up the notion of trying to change what we say and simply concentrate on simply making a point about what the organism is Really doing.

In this regard, I think (?) Audra and I are talking about something different, even though I think we started off talking about the same thing.

I think Bruce is catching what I am saying. Our EXPERIENCE is that we control perceptions. No argument there. But that does not mean that that is what we are REALLY doing (which is controlling error, suprise suprise). Yes, of course we have lots of excellent evidence that we control perceptions--I agree that such evidence is evidence of controlling perceptions. But understand WHY it is that way...neural structures have reorganized in such a way that perceptions are brought under control. Those structures exist because they have been successful in accomplishing what all us organisms have in common--controlling error, which is in some way (that I unfortunately cannot ascertain yet) epistemically related to Survival. We didn't Consciously come up with these reorganizations--the organism has mechanisms by which these processes occur. Then there are these experiences that we have which may or may not related to what is really going on, more or less. It's like the idea that one figures out an answer, and THEN (not WHEN) becomes aware of it.

From what I know of PCTers, I thought that most of you held such similar conceptions of our mechanistic selves, but perhaps I have built up "What PCTers think" less from observation than I think: Somehthing illogical like, "I think like PCTers. I think x. Thus PCTers think x." Anyway, the point of this paragraph is that I don't expect you to disagree, even though it Seems we are opposing views.

Now I don't think this is simply a semantic point, although it is at one level. It SEEMS really critical to me. But perhaps someone who sees what I am saying and agrees would say "Its a good point, but it really isn't THAT critical." I can't tell--I'm just me.

Carpe' Diem   Mark   "It is impossible to do only one thing."

Date: Fri Dec 04, 1992 4:27 pm PST
Subject: Re: error control

[From Rick Marken (921204.1430)]   Audra Wenzlow (921204) --

>Would you mind posting the "algebra" again? Maybe this would help
>me understand your point, as it was exactly the model itself that made
>me think that we try to control errors instead of perceptions.

La plaisaire est de mio (both of my addresses works fine, Gary, in whatever language we choose -- Koo Koo Ka Choo).

Here are the two simultaneous equations for a linear negative feedback control system.

(1) o = k1 (r - p + de)

(2) p = k2 (o + d)

where o is the output variable, r is the reference signal, p is the perceptual signal, de is the disturbance added to the error (this is the new part -- the error is (r-p) and I'm just adding a disturbance to it -- like adding a neural signal into the error signal path) and d is the environmental disturbance. k1 is the system amplification -- it transforms the error signal (with the error disturbance,de, added in this case) into a physical output variable, o. So k1 is a BIG number compared to the other coefficient, k2, which turns a physical variable (o + d) into a perceptual signal. In fact, k2 is likely to be a fraction.

Solving the two equations simultaneously for p I get:

p = [k1k2/(1+k1k2)] r + [k1k2/(1+k1k2)] de + [k2/(1+k1k2)] d

Assuming k1k2 >> 1, then the term [k1k2/(1+k1k2)] is approximately equal to 1. Assuming k1>>k2 the term [k2/(1+k1k2)] is approximately zero so we get

p = r + de

I have run this as a dynamic simulation and this is exactly what happens which suggests that our assumptions have not done any violence to reality. When a disturbance, de, is added to the error signal in a control loop, its effects are not eliminated AT ALL (as would be expected if the error, r-p, were controlled). Instead, this disturbance is completely effective and acts like a virtual reference signal; the perceptual signal is brought to the value (r+de) instead of r (as is usually the case).
 If, on the other hand, you add the same kind of disturbance to the perceptual signal (ie. after the physical variable has been converted into a neural variable) it will have very little or no effect (in the same way that the physical disturbance, d, has little or no effect). Try the derivation yourself. The two equations are

(1) o = k1 (r-p)

(2) p = k2 (o + d) + dp

where dp is the direct disturbance to the perceptual signal. You will end up with a term like dp/(1+k1k2) which, assuming k1k2 >> 1, is basically 0. And, again, simulation shows that this is indeed the way the control system works.

So, in a control system (as Bill Powers has told us over and over and over again -- apparently not frequently enough, however) the ONLY variable that is controlled is the PERCEPTUAL VARIABLE; dare I say it -- BEHAVIOR IS THE CONTROL OF PERCEPTION.

Regards   Rick

Date: Fri Dec 04, 1992 4:29 pm PST
Subject: Evolution; controlling error

[From Bill Powers (921204.1430)] Gary Cziko (921204.1701) --

Selection by consequences, hey?

I think there is something missing from this standard evolutionary scenario. Remember when Randall Beer was telling us about using the "genetic algorithm" for producing bugs that could walk to food? I asked for some details, and it turned out that these bugs were deemed worthy of surviving if they simply moved toward the food during one lifetime of trials. The ones that moved toward the food were retained for the next generation. Beer himself allowed that this was an "external" criterion.

The problem with all external criteria is that they aren't realistic: real bugs that only made a move in the right direction, but never actually got to the food, would not survive. So either the behavior has to succeed by moving the bug all the way to the food so it can be eaten, or the bug doesn't survive and the "move in the right direction" behavior goes extinct. There is no way to select for anything but complete success.

I think we must conclude from modeling efforts and from logic that in order for natural selection to work as efficiently as it needs to work, most changes must produce some positive effect on the organism in order to be retained. With survival and non-survival as the only choices, it seems to me, the chances of developing any complex organization in only 3 billion years would be much too small. Yet millions of species of organisms have produced complex variations thousands of times during their evolution. Now we're talking about making complex changes over a period of a few million years, not a few billion years.

I think that Darwinian selection is probably at the base of evolution. But something more must have evolved in order for evolution to work as we see it working over the last half billion years. I think that organisms learned long ago how to evolve themselves.

The most efficient selection criterion must be based on continuous processes, not binary ones. I can think of one even within Darwinian natural selection -- an effect on age of successful reproduction. If a species can change so as to reproduce (and rear young) at an earlier age, the species population will grow faster. This criterion would work on a continuous scale; as the age of successful reproducing varies, the preponderance of organisms of that kind would also vary. But no organism or species would have to go extinct for this selection effect to be evident. All the organisms involved could live to the age of reproduction, yet the organisms that reproduced earlier in life would tend to have the higher populations. There must be lots of other continuous effects that don't involve just the crude criterion of survival to the age of reproduction.

But I am still bothered by the idea of the environment, the stupid environment, selecting anything. The problem is a lot like that of S-R theory. Why should the environment stimulate organisms to do just those things that are good for the organism? Why should the environment select for survival of a species instead of its extinction? The nonliving environment has no power to select for anything -- it can't tell whether a change in the behavior of an organism gets the organism closer to a more viable form, nor does it care.

We already know that natural variation provides the raw material for change. It's clear that the variations that promote survival to the age of reproduction are the ones that will propagate. That part of the logic of "normal" evolution is clear. But I think that at some point organisms became able (1) to mutate on purpose, and (2) to compare the consequences of mutation against some internal criterion, on a continuous scale, as a basis for mutating sooner or for delaying the next mutation. The behavior of E. coli,

which uses this principle, is a very high-level behavior compared with what simpler organisms do. Why should we think that this is the first appearance of this principle in living systems? I see no basic problem with looking for this same principle at simpler and simpler levels until we get clear to the level of DNA.

The E. coli method of evolution would be far more efficient than blind variation and BLIND retention. The logic of natural selection is OK as far as it goes. But I think it leaves something out: the power of purposeful retention.

Audra Wenzlow (921204) --

The term "control" in ordinary parlance has no fixed definition. As a result, people are free to adopt some favorite meaning for the term and assume that this is meant whenever someone else uses the same word. Control theory has given us a complete definition, one that includes all the ordinary meanings as special cases. By the same token, it shows that ordinary usages ARE special cases.

>... it was exactly the model itself that made
>me think that we try to control errors instead of perceptions.

I suggest that it was the model PLUS your favorite meaning of control that you used before learning about control theory.

One informal usage of the word control is in the sense of minimizing or restraining or limiting or preventing. Thus we talk about using 2-4-D to control weeds, or of going on a diet to control weight, or exerting tight self-control to prevent ourselves from doing something impulsive that we might regret. This is the sense of control that means minimizing or reducing or preventing or holding within bounds.

On the other hand, we also speak about controlling the aim of a gun, or controlling the rate of growth of a crystal, or controlling temperature, or controlling the altitude of an airplane. This set of meanings certainly has nothing to do with minimizing or restraining or limiting or preventing. Instead, it implies some specific state of something that is to be brought about and maintained. Controlling temperature or the growth rate of a crystal could imply acting either to decrease or increase the temperature or growth rate to produce a specific temperature or rate; the meaning is certainly not to make thesevariables tend toward zero. In this sense of controlling, the object is to create (if necessary) and then maintain some specific non-zero state of a variable.

Control theory handles both of these meanings in terms of a reference signal. If you set the reference signal to zero, then you will be controlling the variable toward the state of least amount possible. If you then set the reference level to a higher value, you will act to increase the amount of the variable, or bring it to some definitely nonzero state.

So the more general meaning of control is to specify the state of some variable and act to bring it to that state and keep it there.

Control of error signals does not fit this more general concept of control. Error signals are always brought toward zero, so they fit the informal meaning of minimizing or restraining or limiting or preventing. But the action of the control system on error signals does not fit the more general idea of acting to bring a variable to any state, zero or otherwise, and keep it there. If you try to propose that error signals could be brought to some nonzero state on purpose, then you have to specify how the system knows whether the reference signal is in the right nonzero state or is somewhat different from

it. In other words, you have to put another comparator at the output of the comparator, and supply another reference signal for it telling it how much error is wanted. This is completely redundant with the original model: the effect is exactly equivalent to a shift in the original reference signal.

Under the most general definition of control, only perceptions among all the signals inside a control system are controllable in all senses of the term.

What I'm looking for from you (and Mark) is some indication that you understand this argument and either agree or disagree with it. if you disagree, I'd like to know what the basis is.

Gary Cziko (921204.2100 GMT) --

>If I add in a disturbance to the error so that, e.g.,
>error = reference signal - perception + 5, then error will still be
>kept near zero.

But the perceptual signal will now be controlled about a level of
reference signal + 5.

Best to all, Bill P.

Date: Fri Dec 04, 1992 5:23 pm PST
Subject: Evolution; controlling error

[From Gary Cziko 921204.2345 GMT] Bill Powers (921204.1430)

I was hoping that my ruminations would disturb you in such a way as to evoke some more thoughts on a CT view of evolution. I will need time to digest them. Thanks.

In the meantime, I don't need much time to digest:

>>If I add in a disturbance to the error so that, e.g.,
>>error = reference signal - perception + 5, then error will still be
>>kept near zero.
>
>But the perceptual signal will now be controlled about a level of
>reference signal + 5.

But doesn't this then prove Audra's point showing that the perception is not controlled (since it has changed as a result of the disturbance added to the error signal) but rather the error is controlled (it has not changed since it is still near zero)?

Nuts--I wanted Audra to be wrong about this and you and Marken right! It's very disturbing to see someone with a background in STATISTICS do this to us.--Gary

Date: Fri Dec 04, 1992 5:26 pm PST
Subject: Disturbing Error

[From Rick Marken (921204.1530)]

Am I a busy little netter today or what?

Gary Cziko (921294.2100 GMT) --

>I don't remember seeing the algebra, and this does not make sense to me.
>If error = reference signal - perception, a good control system will keep
>error near zero. If I add in a disturbance to the error so that, e.g.,
>error = reference signal - perception + 5, then error will still be kept
>near zero. The system will do what it has to do to get rid of the effect
>of the +5. How is this not controlling error?--Gary

Ah, the problem of time lags. You should have the algebraic solution before you get this.

Actually, your intuition is based on VERY GOOD circular control system reasoning. So this result (about the effect of directly disturbing the error signal) is kind of counter-intuitive to your well - developed counter-intuitive PCT intuition. Will the circle be unbroken??

Love ya Rick

Date: Fri Dec 04, 1992 6:32 pm PST
Subject: Evolution; controlling error

[From Rick Marken (921204.1800)] Gary Cziko (921204.2345 GMT)--

>But doesn't this then prove Audra's point showing that the perception is
>not controlled (since it has changed as a result of the disturbance added
>to the error signal) but rather the error is controlled (it has not changed
>since it is still near zero)?

Read my algebra. The perception is no more disturbed by the addition of the disturbance than it is when the reference level varies. The error signal, however, is disturbed -- it DOES NOT go to zero -- it goes to e + de. The error signal varies (mainly) as de. Adding dp to the perception in the same way DOES NOT result in p equalling p + dp. So p is controlled relative to r (or r + de) but e is NOT CONTROLLED-- it is not maintained against disturbance at some reference level -- the purported reference being zero.

>Nuts--I wanted Audra to be wrong about this and you and Marken right! It's
>very disturbing to see someone with a background in STATISTICS do this to
>us.--Gary

Well, I don't want to be competitive about this. Audra is trying to learn this stuff and, I think, asking the right kinds of questions. Her intuition about controlling error, though wrong, at least does make us look more closely at this phenomenon (control); and I think she led us to some interesting (and surprising) discoveries about the behavior of control systems.

Best Rick

Date: Fri Dec 04, 1992 8:40 pm PST
Subject: Rick's proof: error not controlled

[From Bill Powers (921204.2100)] Rick Marken (921204.1800) --

>Read my algebra.

Rick, you have taken the most direct route to the correct answer while I was still circling around. The error signal is not controlled. The way you prove it is to add a disturbance to it and see whether the disturbance is resisted. It is not resisted. Therefore the error signal is not controlled: QED.

This gives me once again an opportunity to harp on HOW disturbances are applied. You don't disturb a variable by just saying it has changed. You have to establish a physical link in the model through which a disturbing variable can contribute an effect to the disturbed variable without FORCING the disturbed variable to change. If you FORCE the disturbed variable to change by a known amount, you have broken the control loop because you're preventing other influences from also having their effects on the disturbed variable.

So, to explain to others, the way you inserted a disturbance of the error signal was like this:

```
                 ref sig     disturbance of error                   |                 |
percep --> COMP --err sig--> ADDER -->  effective error         |
|    from input                                    to output     function
function
```

There is only one other way to disturb the error signal: add another signal to the comparator. But that is equivalent to changing the reference signal. Rick's approach is the only way to add a disturbance to the error signal itself. And doing that CAUSES an error between perception and reference.

Best,   Bill P.

Date: Sat Dec 05, 1992 10:12 am PST
Subject: Re: controlling error

[from Mary Powers 9212.05]   Mark, Audra:

Bill said something like this the other day, but I think it bears repeating/rephrasing. When you try to pick one part of a control system as being the most important - control of error - you are missing the point that all "parts" are equally important. Basically you are thinking lineally - here's this part and then this one and this one and AHA! this one, which is the "really important" one. When all the parts are lying around, sensors and comparators and so on, you don't have a control system. When they are linked together, you do. All are essential. Asserting that one part is more important than the others is only to say that we CAN perceive and categorize control systems as having parts, but that doesn't mean that it's useful to do so. The interesting thing about control systems is what emerges from the linking together of their components. Evaluating the worth of one part over others is reductionism - good for intellectual arguments but not likely to be very productive.

Date: Sat Dec 05, 1992 10:12 am PST
Subject: Controlling error, NOT!

[from Gary Cziko 921205.1800 GMT]   Rick Marken (921204.1800) reminded me to:

>Read my algebra. The perception is no more disturbed by the addition of
>the disturbance than it is when the reference level varies. The error
>signal, however, is disturbed -- it DOES NOT go to zero -- it goes to
>e + de. The error signal varies (mainly) as de. Adding dp to the perception
>in the same way DOES NOT result in p equalling p + dp. So p is controlled
>relative to r (or r + de) but e is NOT CONTROLLED-- it is not maintained
>against disturbance at some reference level -- the purported reference
>being zero.

Yes, I should have read your algebra more carefully. It says, in effect, that there is no way for the control system to "know" if a disturbance to the error signal is in fact a disturbance to the error signal or a "legitimate" change in the reference level. In other words, disturbances to the error signal are not resisted, but are rather complied with. Now I am having trouble figuring out what led to think otherwise. Looking at the canonical control system diagram actually made it clearer than the algebra.

Isn't it interesting, however, that all this discussion about what is controlled did not involve any difference of opinion concerning the control model itself? Everyone agreed that the arrow and boxes and functions were OK. And yet there was lot of discussion concerning the use of WORDS to describe what was going on. This is interesting since suggests that even if we have an explicit and useful mathematical model of a phenomenon, people still want to be able to understand things using WORDS. Agreeing on the math does not seem to be enough. We seem want to find agreement on the words as well, even though the words can never be as explicit as the formal mathematical model. As Bruce Nevin has often reminded us, we gotta use words to talk to each other.--Gary

Date: Sat Dec 05, 1992 1:38 pm PST
Subject: Controlling error, NOT!

[From Rick Marken (921205.1300)] Gary Cziko (921205.1800 GMT)--

>Isn't it interesting, however, that all this discussion about what is
>controlled did not involve any difference of opinion concerning the control
>model itself? Everyone agreed that the arrow and boxes and functions were
>OK. And yet there was lot of discussion concerning the use of WORDS to
>describe what was going on.

Yes, indeed. It is very interesting. But I think it reveals more than the importance we ascribe to putting things into words. I think this is related to an old discussion that, I think, is worth bringing up again in this context. It has to do with how it is possible that people who understand control theory often have no advantage in understanding PCT (control theory as it is applied to living systems) -- in fact, they are often less able to "get" PCT than complete novices (we have had experience with this on the net so I speak from experience).

How could this be? After all, a control theorist knows the control model; s/he works with real, physical control systems on a daily basis. S/he know all the mathematics of control system theory. How could such a person (usually an engineer of some kind) not understand PCT (as they often don't). The problem is similar to that involved in seeing that control systems don't control error -- it's just not something that anyone usually has any reason to think about. For the engineer, what s/he never has any reason to think about is the fact that the system is actually controlling a percpetual representation of the variable being controlled.

The concerns of the engineer are quite different from those of the life scientist. The engineer builds a control system (like a thermostat) to control some variable. So the engineer knows what variable is to be controlled; his/her problem is to design the system so that it controls well. So the engineer must know about the dynamics of control, the parameters of control, etc. The engineer might even try tricks like predictive control (trying to predict disturbances, like seasonal changes in outdoor temperature) to improve the control exhibited by the system.

The life scientist, on the other hand, is dealing with a control system that has already been built. The most important thing that is not known about this system is precisely what the engineer already knows, namely, what variable it is controlling. The life scientist is less interested in the parameters and dynamics of control (at least, at first); his/her main goal is to figure out what is being controlled -- ie., do the test for the controlled variable (engineers don't need to do this, unless they've got problems with the sensors they are using or they are trying to control some complex combination of sensor outputs). Doing the test properly with living systems requires that the tester understand that the system is controlling a perceptual representation of something; that what an organism is "doing" is controlling a percpetual variable that may not be perceptually obvious (or even perceptible at all, like the high frequency sounds of the bat) to the tester.

So, what seem like disagreements over how to describe the operation of a system that everyone agrees has such and such components hooked up in such and such ways (like a control system) can have important substantive implications that go well beyond the question of "how shall I put this?" Failure to understand that control systems control perceptions -- while of no significance to a control engineer -- prevented those control engineers (other than Bill Powers, that is) who did apply control theory to the behavior of living systems from discovering that behavior is the control of perception; in other words, this little oversight kept these control engineers from making the greatest scientific observation since the apple fell on Newton.

Best Rick

Date: Sat Dec 05, 1992 4:02 pm PST
Subject: Re: controlling error

[from Gary Cziko 921205.1830 GMT] Mary Powers 921205 said to Mark and Audra:

>The interesting
>thing about control systems is what emerges from the linking
>together of their components. Evaluating the worth of one part
>over others is reductionism - good for intellectual arguments but
>not likely to be very productive.

Of course you realize that many "humanistic" psychologists would see the type of theorizing that goes on in HPCT as this same type of reductionism. And in a way HPCT certainly IS reductionism. But has Mary points out, reducing past a certain point will "kill" the phenomenon you are trying to understand.

When I try to explain PCT to people, I show them rubber bands and then Bill's Demo 2 and how well it accounts for tracking behavior. When they then ask me what tracking and rubberbands has to do with "real" behavior, I try to explain that the elemental control system is the furthest we can reduce our model and still have it behave appropriately,

even for these simplest of purposeful behaviors. Reducing things further than this (e.g., to Stimulus -> Response, or Cognition -> Response, or Stimulus -> Cognition -> Response) just doesn't make it (except in those very special cases as described so well in Rick Marken's "Blind Men" paper).

So there doesn't seem to be wrong with reductionism per se. It just a matter of how far to go before you shift into a very different domain. Dissect the loop and you've lost purpose, the very phenomenon you were trying to understand.

Repeating again part of Mary's comment:

>The interesting
>thing about control systems is what emerges from the linking
>together of their components.

Note Mary's use of the word "emerges." "Emergence" seems to have become a very trendy word these days. Stephen Levy's book _Artificial Life_ uses it all over the place. Putting certain components together in a certain way can result in a phenomenon which could not be predicted by examining the individual parts in isolation. Perhaps those of us wanting to disseminate an understanding of control systems and PCT might do well to use this angle "emergence" to make our point.--Gary

Date: Sat Dec 05, 1992 6:42 pm PST
Subject: DECISION MAKERS - RKC
from Bob Clark December 5, 1992

A Few Remarks --
seem appropriate since I've been out of touch for some six months. I've been involved with two related organizations, both interested in rotary wing aircraft. One, a small Museum (Secretary/Treasurer/Editor), the other, a National Convention (Financial Chairman). Although I am now leaving these activities, the process of transferring my responsibilities to others is not yet complete.

With over 1000 posts to be reviewed, some interesting items may have overlooked. Recently there have been some, on DECISIONS, that are related to my present discussion. Rather than commenting on them, I offer a different view.

* * * * INTRODUCTION * * * *

I have been feeling the need for revisions and additions to the original Hierarchical structure for some time. As I remember it, we expected the early publications (Powers, Clark, McFarland and others) to lead to further developments and modifications. Powers, in his outstanding work: "BEHAVIOR: THE CONTROL OF PERCEPTION," further elaborated and presented those concepts. It is my impression that Powers did not, and does not now, consider that book to be complete and final. However there seems to have been little discussion of possible changes in the original Hierarchy. Rather, there seems to have been discussion of various interesting and important applications and related ideas.

Here I am summarizing some of the ideas that have interested me over the years. Many of these points can and should be developed and, perhaps, modified. Certainly I consider all of them open to review and discussion. I recognize that they are based on -- and limited by -- my own experiences and conclusions. To me, much of this material is self-evident. My purpose here is to organize these observations and relate them to the basic concepts of Hierarchical Control Systems, making them available to others for

further development. I hope that these ideas and approaches will be found intriguing, leading, in turn, to further modifications, elaborations -- and alternatives!

I am minimizing explanations to emphasize the over-all structure, viewpoints and modifications of the Hierarchy suggested here. Hopefully, this will require less space and facilitate discussion.

* * * * A DECISION MAKING ENTITY (DME, IF YOU LIKE) * * * *

I begin with "A DECISION MAKING ENTITY." This concept seems to have been over-looked, except for an implied inclusion in the "Reorganizing Function." That Function, so far as I know, has never been analyzed in terms of its structure, capabilities, limitations and its relation to the Hierarchy. Rather, "under what conditions" and "how to improve results" have been studied. These are important, of course, but where and how these activities occur and how they relate to a Hierarchical Control System is the subject of this presentation.

Making DECISIONS is an everyday occurrence for most of us. Most are routine, ["Do you want cheese on your hamburger?" "What's the best way to get to Chicago?" "Shall I cross the street before, or after, that car goes by?"] but some involve complex analysis ["How do I get funds for this project?" "Who will be willing to act as Editor?"] and may reveal unexpected conflicts. "Who," or "what," makes Decisions, and "where" they are made has received little or no attention.

It is tempting to identify this Entity as the "SELF," "EGO," or similar label. However such terms tend to include additional concepts such as "personality," "character" and other aspects of the individual. They may include guide lines commonly used by the individual in making his decisions. These anthropomorphic concepts tend to be derived from previous decisions leading to conclusions and assumptions used as the basis of Decisions and Actions. These are important, of course, but are excluded from the concept of the DME.

The Decision Making Entity, as here understood, can act without being bound by past decisions. It frequently uses them because they are readily available and alternatives may be overlooked. It has the ability to be arbitrary. It can change past decisions if they are accessible to the DME. Access can be limited by a combination of previous decisions. Consistency among decisions is not intrinsic. It is capable of contradictory actions!

DEFINITION of the DME.
The Decision Making Entity is defined in terms of its Connections (Inputs and Outputs) and its Capabilities.

CONNECTIONS AVAILABLE
INPUT A) information about the Current Condition of Physiological Systems.
a) information about the operating condition of the organism
b) information about conditions outside the organism

INPUT B) information about past events -- memories, recordings, whatever. This includes both verbal and non-verbal events. The distinguishing feature of this Connection is that the information is all from past time, although "past" can be very close to "present." These events can range from the remembered mosquito bite, to the discussion of "Real Reality," and so forth.

REFERENCE LEVELS C) information specifying the acceptable operating condition of the organism, A)a). These are the "Intrinsic Levels" of other discussions.

OUTPUTS D) information that acts throughout the Hierarchy. Usually, but not necessarily, they act by selecting inputs to the Higher Order Levels of the Hierarchy, leaving the details to the remaining lower order structures. These Outputs can be considered from two different viewpoints:
a) as outputs from the DME
b) as inputs to the many parts of the Hierarchy. Thus they act as Reference Levels throughout the Hierarchy.

CAPABILITIES
A) Directing ATTENTION.
a) Selecting the information, INPUT A), to be controlled.
b) Selecting information from past events, INPUT B), for comparison with the current situation.
c) Comparison of the current and projected ("anticipated") situations with acceptable magnitudes of the variables selected for control -- especially Intrinsic Variables, C), when they are relevant. Everyday situations usually do not directly involve Intrinsic Variables.

B) Decision Making.
a) Selecting OUTPUTS, D), to be used by the DME as Reference Levels for the Hierarchy.
b) Activating the Selected OUTPUTS for controlling the Selected Systems.

CONDITIONS REQUIRED
In order to direct its attention and make its decisions, the organism must be Conscious. Unconscious means that the DME is unable to receive information from its inputs. However the remainder of the systems may be functional, operating on the basis of the most recent settings of their reference levels. There are several interesting situations that can occur: sleep, coma, paralysis, trauma, etc. These, and others, are worth separate discussion.

A NEGATIVE FEEDBACK CONTROL SYSTEM
These connections define a Negative Feedback Control System. The stated capabilities are unique to the DME and critical to its operation.

In this view, the Feedback Signals include two categories of information: A)a) about the current operating condition of the physiological systems, and A)b), about the surrounding environment. These signals are compared to those levels selected from memory, B), and applied as inputs to the Hierarchy. In addition, the first group, A)a), is compared with the Intrinsic Reference Levels, C), for possible action. The Output Function consists of the entire Hierarchy, D).

The DME is able to direct attention to any group, sub-group, or combination of available Memories and compare the projected results with any other combination of available Memories as well as to any related Intrinsic Levels.

It is able to combine selected Memories for application as Reference Levels throughout the Hierarchy as required for the selected action.

[[NOTE: The entire set of Feedback Signals available to the DME is the set of PERCEPTIONS, CONTROLLED by BEHAVIOR, as discussed by Powers in BEHAVIOR: THE CONTROL OF PERCEPTION.]]

* * * * TWO CONTRASTING VIEWPOINTS * * * *

Each of these views is useful, "correct," if you please, depending on what your purposes may be.

First, THE DME'S VIEW OF THE WORLD -- AN S-R VIEW

The DME looks "down" his Hierarchy for ways to maintain and improve his well-being. The DME acts, like any control system, when it detects a difference between current perceptions and reference perceptions. It examines available alternatives, based on current data combined with projected results of alternative actions. It selects and then applies its selections as reference levels where needed throughout the Hierarchy. The DME has no need to "know" anything about the details of the control systems it is using. It merely applies its output signal(s) where needed and the systems respond. This applies not only within the organism, but equally to using other individuals or groups of individuals, as means to accomplish the selected results. This can be as simple as making requests, or giving orders, etc -- if the others have already internally decided to accept and act on such requests/orders.

The DME acts like an individual trying to maintain and improve his circumstances.

Second, THE HIERARCHICAL CONTROL SYSTEM'S VIEW OF THE WORLD

Viewing the world in terms of HIERARCHIES OF CONTROL SYSTEMS covers an amazing range of observations, and leads to additional study and analysis. The concept of higher order control of lower order systems through setting their reference levels is particularly simple and useful. In this View, "Behavior" consists of counter-acting, or opposing, any disturbance of its Controlled Variables. That is, it detects a difference between current perceptions and reference perceptions. A disturbance, uncontrolled at one level, tends to result in disturbance at a higher level. However, this structure has no way to change its reference levels -- nor does it have a way to change its organization. Its Memories are retained in the form of established structures and fixed reference levels. It cannot examine memories with a view to selecting alternative ways to achieve its control. Its high order reference levels are based on "remembered" events, but there is no way to "project," or "anticipate" alternatives for possible application to a given situation.

This PROBLEM arose in our early discussions as we sought to define higher levels. How can the changing behavior of an individual be described when he is blocked? Analysis working upward through the lower orders assumed (implicitly) a set of fixed reference levels, especially at the higher levels.

How could these be changed? How could the system be "re-organized?" An ad-hoc "re-organizing system" was proposed. Without actually being stated, its definition amounted to "whatever is needed in order obtain these results." Powers has discussed this concept in several places, but it seems to me to be incomplete.

I am familiar with this view since I was deeply involved in the early developments leading, ultimately to Powers BCP as well as papers and discussions among others. In my own life I have found, and continue to find, this viewpoint very useful in many ways. But it is not the only view that I find useful.

In fact, what seems to be needed is the DME as suggested here. I find combining the DME approach with the Hierarchical view provides some additional answers, and leads to some revisions of the HIERARCHY.

* * * * THE HIERARCHY REVISITED * * * *

The concept of a Decision Making Entity implies changes in the definitions of some of the Orders of the Hierarchy. In addition, some changes are also proposed for some Orders.

The over-all objective is Control of Perception (Powers) as genetically required. A Hierarchy of Control Systems is the means to that end. In the following suggestions, the guiding concept of the Hierarchy, "Higher Order Goals are accomplished through setting Reference Levels for Lower Orders," is retained.

I. ZERO ORDER SYSTEMS -- INTRINSIC SYSTEMS. These are the physiological systems underlying the Decision Making Entity. They include all systems providing neural inputs directly to the DME. They report the operating condition of the organism for comparison with Intrinsic Levels for control action through the Hierarchy. Some of these systems may, themselves, be feedback control systems (I have in mind some of the hormonal systems), but they are controlled only indirectly through the Hierarchy. Zero Order Systems also include direct neural signals representing the conditions of the external environment -- typically, the usual five senses.

II. FIRST ORDER. Control of individual muscles (or muscle fibers, if you prefer). This remains essentially the same as the original First Order. The signals, of course, are neural intensities serving as feedback signals derived from the tensions of the individual muscles. Powers (BCP Chap 7, p 82 ff) discusses this in depth from several standpoints. From the "DME's view," this order controls individual muscles. It is a "follower" system -- it simply reproduces (within its capabilities) the reference signal(s) provided.

III. SECOND ORDER. Control of "Configurations." At this level, they are considered "Static," that is, temporal variables are unspecified. Combinations of muscle systems are typical examples, but this Order need not be limited to muscle systems. Our original concept, elaborated by Powers (BCP Chap 8, p 99 ff) "Second Order, Sensations" appears to combine parts of "Zero Order" and "First Order" as presented here. In the present treatment, Second Order is pretty much the same as Powers "Third Order, Configuration Control" (BCP Chap 9, p 115 ff). Muscle systems are convenient and typical examples. Powers includes both ocular and auditory muscle systems in this Chapter. Powers includes the perception of "Objects" within this category (ibid p 125 ff). He notes the "invariance" of combinations of sensations that can be perceived as "objects." "Invariance" implies, at the minimum, some degree of short term memory to provide continuity -- invariance. From the "DME's view," this Order does not include Control of Objects. In general, each Configuration would be multidimensional, expressible in vector or matrix terms if desired. As Output Systems controlled by the "DME", these are "follower" systems like First Order Systems.

IV. THIRD ORDER. Control of Sequence. Powers (BCP Chap 11, p 137) assigns this to Fifth Order, much as we did originally. Powers places a Fourth Order, "Transitions," ahead of his Fifth Order. In the present treatment, Third Order controls the sequence of Second Order, Static Configurations, much like the frames of a movie. The frames could be re-arranged by the "DME" by means of Third Order Systems. Of course, there are intrinsic limitations, but the concept remains. These are also "follower" systems.

V. FOURTH ORDER. Control of Temporal Variables. As I recall our discussions, these variables were never made explicit. They seem to have been included within Sequences although no direct statement was made to that effect. Powers (BCP Chap's 10 & 11) seems to include these variables implicitly without recognizing them. His concept of "Transitions" also seems to implicitly include Temporal Variables. Here, "Temporal Variables" refers to such items as "fast," "slow," "tempo," "frequency (of oscillation," and the like. For example, the "DME" can apply the same tempo to a variety of situations -- it appears to be an independent parameter of systems in action. The importance of these variables seems to be generally taken for granted -- but otherwise ignored. I have found it very useful to pay attention to, and control, this Order of Variables.

VI. FIFTH ORDER. Control of -- and selection of -- Skills. Typically, muscle skills. Skills consist of temporal sequences of configuration produced by combinations of muscle tensions. Control of variables of speed, tempo, and other Time Variables are important. This Order concerns individual motor skills. This is also where the perception of "objects" belongs. The world and its contents are treated as a multiplicity of inanimate objects. For example, bowling requires a ball and an alley while using a sequence of positions performed with selected timing. Simpler skills include walking, running, etc. These are motor skills where nothing is needed but the physical equipment and the "DME's" decision. This is a general characteristic of Fifth Order Systems.

VII. SIXTH ORDER. Control of Interpersonal Relationships. This Order recognizes the differences between inanimate objects and independent active entities. This includes animals and, especially, people. The DME seeks its objectives through controlling these independent entities. Often it acts as though they were Stimulus-Response Systems. This frequently works, since many of these otherwise independent entities have decided to accept suggestions and requests as commands. When it doesn't work, the DME seeks alternative methods to reach its goals.

COMMUNICATION. Control of Communication could be considered for designating Sixth Order. However this would focus on the skills used, Fifth Order, rather than the goals of Sixth Order. Communication is essential to the control of Interpersonal Relationships. Without some form of communication, other individuals are treated as inanimate objects.

Study of the content of Communications, whether non-verbal or verbal, can help clarify the Orders of the Hierarchy as well as Perceptions of the DME. Both the topics discussed by people in everyday conversation and items published in the Media are useful for this purpose.

MODES OF SIXTH ORDER. The topics communicated can be grouped according to the Levels of the Hierarchy.

Zero Mode of Sixth Order. Illness and similar topics are very common. People often have little knowledge of their own anatomy and physical structure. But they talk about it a great deal.

First Mode of Sixth Order. Aside from reports of sore muscles, there seems to be little direct discussion of muscle systems.

Second Mode of Sixth Order. Configurations appear as comments on "posture," positions needed for various skills, and the like.

Third Mode of Sixth Order. Here is discussion of the Sequences of configuration needed to obtain desired results. This includes Sequences of positions forming movements required for a skill.

Fourth Mode of Sixth Order. Variables of Tempo, Rhythm, etc. Aside from discussions of sports events, musical concerts and the like, these variables seem to receive little explicit attention.

Fifth Mode of Sixth Order. Much attention is directed to all sorts of muscle skills. Generally, several lower order considerations are discussed, although not always explicitly. "How To" books are very popular, usually involving most of First through Fifth Modes of Sixth Order. This Mode includes all concepts of the nature of the Physical World. Math and theoretical analysis are also here. Everyday discussions commonly show very little understanding of present day physical science, math and experimental methods. Well-known errors in logic are commonly accepted as valid.

Sixth Mode of Sixth Order. This Mode concerns methods and topics of Communication among individuals and groups. Discussion of these topics, of course, uses the lower Modes as needed. Illustration from personal experiences are frequent (Third, Fourth and Fifth Modes). Reports of activities of public, and private, individuals and organizations are common. Examples, Analogies and Similes are used very frequently. Public Speaking and Teaching Skills ad Methods lie in this Mode. Rules and Regulations used to establish acceptable performance appear here. Games, organizations, social customs (Social Controls), laws, police etc are within Sixth Mode of Sixth Order. As Fifth Mode does for the Physical World, this Mode includes all Theories of Behavior -- whether magical, mystical, intuitive, or scientific, whatever that means! Everyday routine communications reflect the concepts of popular Behavioral Theories. The Control System Theory also is in this Mode.

Seventh Mode of Sixth Order. This Mode concerns control of one's own behavior to accomplish higher order objectives. It uses the concepts and methods of Sixth and Lower Orders for these purposes. Although there is relatively little discussion of these subjects, it does occur. This Mode has a corresponding Order of Control, considered as Seventh Order, Self Image.

IX. SEVENTH ORDER. Self Image and DME. Self Image includes all aspects of the individual's capabilities and organization. To examine one's Self Image requires review of one's remembered actions and interactions as they relate to one's view of individual behavior. This review would tend to include, but perhaps not "require," extrapolation to possible future situations and events. Such imagined results can be compared to objectives at all levels, with underlying emphasis on Intrinsic Levels. This is done by the DME as described in the discussion of the DME under the heading: "A NEGATIVE FEEDBACK CONTROL SYSTEM."

This discussion leaves a great many questions unanswered, and equally many fascinating subjects for investigation. I hope that this condensed outline and analysis will be found useful.

Regards, Robert K. Clark

Date: Sun Dec 06, 1992 10:04 pm PST
Subject: Re: controlling error

From Tom Bourbon 921207.0001
Gary Cziko {921205.1830), in reply to a post by Mary Powers, said something that I do not quite follow: "And in a way HPCT certainly IS reductionism." In what awy, or ways, Gary?

Does HPCT attempt to reduce psychology to another field of study? If so, which one(s) and in which way(s)?

I think this topic came up way back in the first days of CSG-L.

Tom Bourbon

Date: Sun Dec 06, 1992 10:53 pm PST
Subject: models,worlds, and thick skulls

i.n.kurtzer (921207.0009)

well i finally read "models and their worlds..." and i give it two fat thumbs up!!!a regular tour-de-force, a veritible masterpiece, a succinct identification of problems and a tenative (to be overly modest) solution to those problems; also, it was a well-placed kick in to the head/arse of psychology as it stands. however, i can understand why it might have been rejected--the paper is simply not appropriate for a journal, it is not NORMAL SCIENCE (i bet kuhn and dag will love that one), period. it IS a PROLEGOMENA TO A LIFE SCIENCE. remides one of galileo's comparison and contrast of ptolemaic and copernican gesalts. i say, emphatically: @#%$@ them all, let their ecletic plastic palace sink into an ocean of snake oil and phrenomenology, let them practice their modern numerology searching for truth within a scatterplot of mush, let them be the STATANISTS that they are (the idolatry of STAT the viceroy of the realm of NILPOINT where stillbirths are allowed to live). the have no need for midwives like yourselves who can recognize a sickly child. ah, if men determined the worth of an idea like the spartans did for the worth of a child. gee, science really is a rational progression of ideas---not-so-subtle sarcasm.

a cygnet of csg-net, or just a overblown moron i.n.kurtzer

Date: Mon Dec 07, 1992 4:52 am PST
Subject: DME decisions

From Greg Williams (921207) Robert K. Clark (921205)

Good to see you back on the net, Bob. Good timing, too -- maybe the conversation will switch from "What do we call what Bill's model does?" to "What evidence is there for Bill's and other models of perceptual control?" I myself think the latter will probably be more enlightening, both to PCTers AND nonPCTers.

>The DME is able to direct attention to any group, sub-group, or
>combination of available Memories and compare the projected
>results with any other combination of available Memories as well
>as to any related Intrinsic Levels.

Why might the DME direct attention to certain memories, rather than others, at some particular time? Do you have a theory of attention "selection" other than the broad viewpoint that the DME tries to (as you say later) "improve his well-being"? Is there some calculus for tradeoffs among various possible ways to "improve" (more or less)?

>The DME acts, like any control system, when it detects a difference between
>current perceptions and reference perceptions. It examines available

>alternatives, based on current data combined with projected results of
>alternative actions. It selects and then applies its selections as reference
>levels where needed throughout the Hierarchy. The DME has no need to "know"
>anything about the details of the control systems it is using. It merely
>applies its output signal(s) where needed and the systems respond.

It seems to me that your proposal would require the DME to run "imagination connection" trials on the alternative actions at a particular time to "examine" and "select" some of them for actual performance. Maybe the DME wouldn't need to "know" details about the parts of the hierarchy which would then actually be used, but it appears that it would have to be able to "see" the results of such use "in imagination," prior to actual performance, in order to have a basis for decision-making. Or do you have different notions about how the selection process occurs? I'm trying to understand the basics of your model at this point, and perhaps I'm headed in the wrong direction. Please clarify.

>In fact, what seems to be needed is the DME as suggested here. I find
>combining the DME approach with the Hierarchical view provides some
>additional answers, and leads to some revisions of the HIERARCHY.

It appears to me that the DME is basically directed (not completely random) reorganization. Is that a fair characterization? In the past, I've been attracted to the idea that there are BOTH random and directed types of reorganization possible in humans -- the former can get you to A solution (eventually, usually) when the latter has no clue on how to direct, but when it works, the latter is usually quicker. The problem has been in figuring out a working model for directing -- hence, my questions above. Fleshing out the mechanism(s?) of your DME's decision making would be very helpful.

I'll let others carry forth the conversation with you on recasting the hierarchy. Right now, my main interest is in models of reorganization. A fellow here in Kentucky named Robert Blackburn recently sent us a huge ms. on his theory of human decision making, and we're trying to understand that and relate it to PCT ideas.

Best wishes, Greg

Date: Mon Dec 07, 1992 7:01 am PST
Subject: control perceptions

Ok, this is my third attempt to reply to some of the messages I was able to read from my computer here at home (does anyone know how to disable call waiting?!--typing *70 doesn't work).

Anyway, from what the lack of phone interuptions allowed me to read, I realize I need to understand why it is that error is not controlled--I can't argue with mathmatics! But as Rick said, its counterintuitive to our counterintuitions.

I still feel quite strongly about what I was saying but maybe an evening with mathmatics will change that, or perhaps WORDS are the culprit.

Mark

Date: Mon Dec 07, 1992 9:54 am PST
Subject: epiphany not faith

[From: Bruce Nevin (Mon 92127 11:28:57)]

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/***************************************************************************
* Cliff Joslyn (Fri, 4 Dec 1992 15:06:24 EST)                              *
*                                                                          *
* No "epiphany" or "leap of faith" is necessary to reach this              *
* understanding. It follows simply from clear thought. Bill's knot shift   *
* helps, too.                                                              *
***************************************************************************/
```

The intellectual realization follows from clear thought. The experiential connection constitutes something more, for which I used the term "epiphany", and to which the phrase "leap of faith" does not at all apply. It is something like the difference between non-obvious conclusions reached by argumentation and obvious perceptions that are self-evident. They complement each other, evidently on different levels of the perceptual hierarchy.

Bruce bn@bbn.com

Date: Mon Dec 07, 1992 9:57 am PST
Subject: Memory in HPCT

Hi all,

Although there have been delays, I am happy to say that I am now working full-time on Martin Taylor's control project. I will be working in conjunction with Chris Love and Jeff Hunter.

Anyway, my first order of business is to get more properly grounded in control theory, so I have just finished giving "Behaviour: The Control Of Perception" a thorough reading (BTW, my formal background is in computing and AI).

I am intrigued by the model of memory presented in Chapter 15. Bill, is this still your working model, or has it changed? (I know its been a while since you wrote the book). Do you still see the basic function of memory as providing reference signals? Do you see the imagination loop as operating through memory as it does in the book? Or have the intervening years changed your conception of the problem? My conception of the diagram on page 221 is that the memory unit would take references from the higher level ECSs and remember how the world is supposed to act, so the memory is basically a little toy world inside the ECS (a toy world, that is, for all the ECSs above it that feed it references). This is a neat model, I think, especially with the switches that give the four different modes of behaviour. But I also realize there are probably many other spcific ways of implementing memory that do not conflict with the basic HPCT scheme, so I am wondering how you feel about this model now, twenty years later.

I had previously thought of imagination mode as one ECS telling upper levels that they are controlling, even though they aren't. However, talking with Martin recently, he pointed out the need to imagine that a reference has NOT been satisfied, as well as imagining that it had, I realized this model can do this as well. If the memory box is acting as a little toy world, then it can respond in imagination mode that "no, this won't work." -- the higher level ECS will experience prolonged error in imagination.

I also found it interesting that you used the RNA model of memory. Have you dropped this idea since the RNA theory has fallen out of favour?

Allan Randall, randall@dciem.dciem.dnd.ca NTT Systems, Inc. Toronto, ON

Date: Mon Dec 07, 1992 12:34 pm PST
Subject: address change

From: Tom Bourbon 921207.1210

I have new addresses for e-mail, no longer in care of Andy
Papanicolaou.

Best wishes,

Tom Bourbon
MEG Laboratory
1528 Postoffice Street
Galveston, TX 77550
USA

e-mail:
TBOURBON@UTMBEACH.BITNET
TBOURBON@BEACH.UTMB.EDU
PHONE (409) 763-6325
FAX (409) 762-9961

Date: Mon Dec 07, 1992 1:05 pm PST
Subject: Re: Rick's proof: error not controlled

[Martin Taylor 921207 10:50] (Olson, Cziko, Marken, Powers et al.)

If something is to be controlled, surely there must be a way for it to be disturbed?

(Powers 921204 21:00)
> You don't disturb a variable by just saying it has changed. You
>have to establish a physical link in the model through which a
>disturbing variable can contribute an effect to the disturbed variable
>...

Where in the world (take the literal meaning of that expression) is the possibility for disturbing the error in an ECS? It's a derived quantity, based on the difference between a desired perceptual state and the perceptual state that the world provides. That definition is changed by any possibility of disturbing the "error" directly. A new quantity may be produced, but it won't be the error any more.

To me it makes no sense to talk about controlling the error. One controls things that could be disturbed. One brings the absolute error (the difference between the controlled signal and its design value) toward zero. That's just a way of saying that one controls the signal that can be disturbed. In these discussions, that's the perceptual signal.

Martin

Date: Mon Dec 07, 1992 3:18 pm PST
Subject: My First PCT Publication Out

[from Gary Cziko 921207.1917 GMT]

I found in my mailbox today the December issue of _Educational Researcher_. Right on the cover in big letters were some interesting phrases like "Purposeful Behavior as the Control of Perception" and "Perceptual Control Theory."

The two relevant articles are:

Cziko, Gary A. (1992). Purposeful behavior as the control of perception: Implications for educational research. _Educational Researcher_, _21_(9), 10-18, 27.

Cziko, Gary A. (1992). Perceptual control theory: One threat to educational research not (yet?) faced by Amundson, Serlin, and Lehrer. _Educational Researcher_, _21_(9), 25-27.

In between is an article by Amundson, Serlin and Lehrer attacking mostly the non-PCT aspects of the first article.

In my article and reply I cite the work of Powers, Bourbon, Ford, Goldstein, Hershberger, Marken, McClelland, McPhail, Petrie and Tucker. I also make a plug for CSGnet. It will be interesting to see if this attracts more people to PCT and CSGnet. _Educational Researcher_ is received monthly by all members of the American Educational Research Association, the largest organization of educational researchers in the U.S.

The appearance of these articles in print also demonstrates that it IS possible to get things published on PCT. I must add, however, that I had a foot in the door since I packaged the article as a follow-up of previous article I had in _ER_ in 1989 and I had at least one friendly reviewer (education deans can sometimes prove very useful; we ought to have more of them in the CSG!).--Gary

P.S. Dag, I will add the first of these references to the CSGnet intro document. I trust Greg will add both of them to the official PCT bibliography.

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Date: Mon Dec 07, 1992 4:01 pm PST
From Rick Marken (921207)

Bill Powers --

I've tried to send this note to your e-mail address (the one that worked on Friday) but it keeps bouncing. My guess is that your mainframe out there at Ft. Lewis is snowed out. If so, you won't get this either. But, just in case:

The only C compiler for the Mac that I could find is Think C. We actually have a copy here at work (in the software library) but someone has it checked out! (what are the odds?). I should be able to get it by the end of this week. So, if you have some C source that I could start learning on, just send it over by e-mail. I would personally like to try to get your demo programs running on the Mac. I know that's a big file but what the hey.

It's best if you send the source to my courier4 address -- easier to download it from there.

Regards Rick

Date: Mon Dec 07, 1992 4:19 pm PST
Subject: RE: My First PCT Publication Out

Gary,

Well, that settles it! Next year at the CSG meeting, you run a workshop on how to publish articles on PCT -- the rest ot us are batting 0.000, and you have two in one issue!

congratulations! Copies to your colleagues?

Tom Bourbon

Date: Mon Dec 07, 1992 6:42 pm PST
Subject: Re: i.n.kurtzer

From: Tom Bourbon (921207.1601 CST) i.n.kurtzer (921207.0009)

>well i finally read "models and their worlds..." and i give it two
>fat thumbs up!!!a regular tour-de-force, a veritible masterpiece,
>a succinct identification of problems and a tenative (to be overly
>modest) solution to those problems;also, it was a well-placed kick
>in to the head/arse of psychology as it stands. however, i can
>understand why it might have been rejected--the paper is simply
>not appropriate for a journal, it is not NORMAL SCIENCE(i bet kuhn
>and dag will love that one),period. it IS a PROLEGOMENA TO A LIFE
>SCIENCE. remides one of galileo's comparison and contrast of
>ptolemaic and copernican gesalts.

In the future, how might we smuggle you onto a list of reviewers?! Thanks for your enthusiastic comments. Actually, your reaction calls into question your good judgment -- how could the near-unanimous opinions of our several reviewers and editors be that different from yours? In the behavioral and cognitive sciences reasoned arguments and explanations are not always welcome. A couple of years ago, Andy Papanicolaou published a little book on emotions, *Emotion: A reconsideration of the somatic theory* -- a powerful critique of cognitive theories of emotion and a reasoned reassertion of a theory once advanced by William James. The somatic (bodily sensation) model of emotion could fit nicely into the HPCT model of perception and behavior. Andy made the mistake of assessing and comparing core concepts in various theories of emotion. In the process he did not cite all of the current writers whose ideas are identical to the older ones, but who believe (or assert) that their ideas are novel -- current writers from among whom reviewers are drawn. All of the learned reviewers slammed the book; one in particular criticized it as, "logical, not scholarly." What can I say?

Regards to Isaac, and to all,

Tom Bourbon

Date: Mon Dec 07, 1992 6:49 pm PST
Subject: Re: Rick's proof: error not controlled

[From Rick Marken (921207.1400)]

Martin Taylor (921207 10:50) --

>Where in the world (take the literal meaning of that expression) is the
>possibility for disturbing the error in an ECS?

It could come from a stream of electronic pulses that is injected into an efferent neuron that carries what is functionally an error signal. Actually, I think you Canadians have a pretty good track record in the area of disturbing neural signals (some of which were unquestionably error signals) -- that Penfield fellow comes to mind.

>It's a derived quantity,
>based on the difference between a desired perceptual state and the
>perceptual state that the world provides. That definition is changed by
>any possibility of disturbing the "error" directly. A new quantity may be
>produced, but it won't be the error any more.

What's your point here? Is a perceptual signal no longer a perceptual signal if it can be disturbed directly (by injecting electronic impulses, say)?

>To me it makes no sense to talk about controlling the error. One controls
>things that could be disturbed.

I guess you didn't like my proof. But your's doesn't seem like an improvement (though it ends with the right conclusion). The problem with your proof (really, it's an argument, not a proof) is that the error signal can, unquestionably, be disturbed (see Bill's post that you referred to -- Powers 921204 21:00). You might not like to call it an error signal after that, but the fact that it can be disturbed suggests (according to your argument above) that it is controlled. And that, as Tricky once said, "would be wrong".

So I ask again -- what's your point?

Regards Rick

Date: Mon Dec 07, 1992 8:23 pm PST
Subject: Re: Evolution

(ps 921207.1600)

> [From Bill Powers (921204.1430)]
>
> But I am still bothered by the idea of the environment, the stupid
> environment, selecting anything. The problem is a lot like that of S-R
> theory. Why should the environment stimulate organisms to do just those
> things that are good for the organism? Why should the environment selectfor
> survival of a species instead of its extinction? The nonliving
> environment has no power to select for anything -- it can't tell whether
> a change in the behavior of an organism gets the organism closer to a
> more viable form, nor does it care.

because the environment is *not* ``non-living.'' take away all the living parts of earth's environment, and what do you get? something like the moon. as far as we can tell, there aren't any organisms living on the moon; the moon's non-living environment indeed doesn't care. but here (and probably anywhere there's life) organisms *do not* live in isolation in an inanimate environment, nor do species.

any bioecology, or probably any modern biology, textbook has scads of examples of how individuals, populations, and species are intimately interconnected. classic examples include predator/prey populations and your relationship w/ all the microorganisms in your gut, including especially everybody's favorite, e. coli.

cheers. –penni

9212B Done

Date: Tue Dec 08, 1992 8:59 am PST
Subject: Re: "Models and their worlds..."

Tom Bourbon (921208.0910 CST) Ray Jackson (921208.0811 GMT)

Subject: "Models and their worlds..."

>Where can a guy get a copy of "models and their worlds..."?
>Sounds like some good reading...

Hello Ray. After consultation with Greg Williams, the official CSG editor, publisher, archivist and house roofer, Bill Powers and I decided to release "models and their worlds" as a CSG technical report, available to the few people our former reviewers and editors believe might be interested. Greg will print it in the format of earlier CSG publications -- Bill's two books and Rick's book. The report is not ready, but as soon as it is you will read about it here. There will probably be a small charge, to cover Greg's costs. Stay tuned.

Best wishes, Tom Bourbon

Date: Tue Dec 08, 1992 2:30 pm PST
Subject: science vs religion

from Ed Ford (921208:1050)

I've always believed that there should be no conflict between science and religion. I've recently found evidence of this. My granddaughter, Ruth, age 5, from California, was visiting Hester and me. Hester had taken Ruth and her first cousin, Sally Ann, age 4, who lives here in Phoenix, to a Christmas tree display, and on the return trip, the two children were in the back of the car, talking. The conversation went as follows:

Sally Ann: My immune system takes care of me.
     Ruth: Well, my guardian angel takes care of me all the time.
Sally Ann: All the time?
     Ruth: Yes, all the time. She's always with me, everywhere.
Sally Ann: Well, if you just leave the body alone it will take care of itself.

Ruth: Well, my guardian angel takes care of me all the time.
Sally Ann: Well, my immune system takes care of me.

They then went on to another subject.

To the Ruth's of this world: May Christ's Blessings be yours this coming year.

To the Sally Ann's of this world: May all the joy and happiness be yours this coming year.

Best, Ed

Date: Tue Dec 08, 1992 2:58 pm PST
Subject: Position Openings

[Ray Allis 921208.1017 PST]

Any ready PCTers who could slip into these positions?

----- Begin Included Message -----
POST-DOCTORAL POSITIONS

A new research/training program supported by the Flinn Foundation will commence January 1, 1993. We are now recruiting three post-doctoral fellows for a period of up to three years each. Trainees with a Ph.D. and/or M.D. degree will participate in an interdisciplinary research program on The Adaptation and Computational Characteristics of Motor Coordination. The research program involves faculty from Arizona State University and Division of Neurobiology at the Barrow Neurological Institute in the Phoenix metropolitan area. Major areas of research focus are: Motor control deficits in Parkinsons Disease, Cerebellar Dysfunctions, Sensorimotor Impairments in the Elderly, Computational Biomechanics, Motor Learning, and adaptational characteristics of Motor Coordination. The stipends are competitive and include health insurance.

Note: Multiple positions are available, depending upon space and facilities, availability of funding, as well as faculty research interests.

Arizona State University is an equal opportunity educator and employer.

Date: Tue Dec 08, 1992 3:33 pm PST
Subject: DME

[From Rick Marken (921208.1000)]

R. K. Clark proposes an addition to the HPCT model -- a DECISION MAKING ENTITY (DME). I don't know what data motivate the addition of a DME but perhaps it has to do with Clark's claim that:

>Making DECISIONS is an everyday occurrence for most of us. Most
>are routine, ["Do you want cheese on your hamburger?" "What's
>the best way to get to Chicago?" "Shall I cross the street
>before, or after, that car goes by?"] but some involve complex

>analysis ["How do I get funds for this project?" "Who will be
>willing to act as Editor?"] and may reveal unexpected conflicts.
>"Who," or "what," makes Decisions, and "where" they are made has
>received little or no attention.

I prefer to look at decisions as the conscious result of conflict. So the cause of decision making is already a part of the model. So is the means of dealing with conflict -- reorganization; we flip a coin and do one thing (produce one perception) and tolerate the error resulting from not doing the other (producing the other perception). A better way to solve such conflicts is to "go up a level", a phenomenon that should be studied by clinicians since it is one of the great theraputic experiences (speaking subjectively) one can have -- and a sure cure for the everyday conflicts (decisions) that are the natural result of never achieving a perfectly organized control hierarchy.

The study of decision making has been popular in conventional psychology because it is an inherently statistical phenomenon. If you offer people choices between almost equally attractive perceptions, coin-flipping (statistics) is the only approach (if you don't go up a level and see the choice itself as arbitrary; but if you did that you would be kicked out of the experiment). There may be something interesting to be learned about hierarchical control and reorganization through the study of decision making (conflict). But I think we must first have a very good model of the "elements" of decision making -- conflict, in particular -- before we can make a coherent stab at decision making (which, as I said, is probably reorganization -- of the conscious variety -- to settle, not necessarily resolve, a conflict).

I think that I could get a better grasp of Clark's DME proposal if he (or anyone) could propose some experimental tests that might reveal the necessailty of the DME model and provide a testbed for evaluating this addition to HPCT.

Regards Rick

Date: Tue Dec 08, 1992 5:49 pm PST
Subject: Closed-loop evolution

[From Rick Marken (921208.1200) Bill Powers (921204.1430) --

>But I am still bothered by the idea of the environment, the stupid
>environment, selecting anything.

penni sibun (921207.1600) replies --

>because the environment is *not* ``non-living.''

Maybe Bill should have said "anything external to the organism" instead of "environment". I believe the point was that PCT would encourage biologists to look inside the individual organism for the processes that do what turn out to be the evolutionary selecting -- and not read too much into the apparent selecting performed by events outside of the organism. There could be an illusion here -- similar to the illusion that consequences (reinforcements) control behavior. But maybe not. In the reinforcement case the loop is clearly closed -- actions influence consequences (reinforcements) and consequences influence actions. And when there is a closed loop (and the sense of the feed back is negative) you have control -- in this case, control of perception of reinforcement. Things are not so clear in evolution; actions (phenotypes) certainly do influence consequences (eg. survival capability) but it is not clear that consequences influence

actions (phenotypes). One way that the latter might happen has been ruled out be research -- inheritance of acquired characteristics. (It could have been that successful actions (phenotypes) got coded back into the genes so that the phenotypes of the next generation would produce better consequences (survival capability). But this doesn't happen). Another alternative (not, as far as I know, seriously considered until recently) is that consequences (survival capability) influence actions (phenotypes) by influencing the probability of a mutation -- mutation becomes more probable when the organism is experiencing itself as surviving poorly. There was some research that suggested that such a link from action to phenotype (via mutation probability) might actually exist -- (the study was reported in Nature) but it is still controversial and not generally accepted. I have also heard of evidence that the probability of cancer (the result of a mutation) is greater when people are "stressed" -- ie. experiencing poor survival capability. So there is some (weak) evidence that "how you are doing" (actions) can influence the probability of a mutation. If this actualy happened (and I'm prepared to believe that it doesn't, even though I would like to believe that it does) it would add an "active" controlling dimension to evolution (like my e. coli model) -- one that goes beyond the currently accepted mechanism -- "Darwin's Hammer" -- which is based on the idea that there is no (direct) feedback connection from consequence (survival ability) to phenotype. This still may be the right idea, but, as Bill P. said, it seems rather inefficient. "Darwin's Hammer" is unquestionably part of the evolutionary story -- but not necessarily a big part or, even, the most important part. But we won't know until biologists start to do the right kind of research.

Best regards Rick

Date: Tue Dec 08, 1992 6:38 pm PST
Subject: Zocher's SIMCON preliminary version.

[From Bill Powers (921208.1500)]

No comments on a lot of interesting mail. Don't take it personally -- I'm trying to get a number of projects done. I'll get around to some of them later. Fortunately there are lots of competent commentators out there.

I have posted three files to Bill Silvert's server. As Bill is out of town until Jan 2, I have put them in pub/public, which is apparently the only place to which files can be uploaded. Bill, I presume, will move them later to pub/csg/programs/source.

These files are a preliminary version of Wolfgang Zocher's "SIMCON".
They can all be transmitted as ASCII files.

simcon3.c C source code with writeup

simcon3.uue uuencoded, self-extracting zipped runnable version. You need uudecode to read it, then when you execute it (simcon.exe), it expands into simcon3.exe for real. Discard everything but the latter.

simcon3.txt A writeup on how to use SIMCON. This is also in the C source code.

I discovered that I can't upload binary files to my local mainframe, so all machine-language files are going to have to be uuencoded. Bummer.

You get onto Silvert's machine with

ftp biome.bio.ns.ca
login anonymous <your email address>

after login is done,

cd pub/public
dir

That will show you the list of files.

Type
GET <filename>

After prompt, enter local file name (name for file on your mainframe).

From there on you need to know how to download from your local machine.

If you can't use FTP and want the files from me, send $1000 and a stamped self-addressed disk mailer with a formatted disk in it, and I will copy the programs and send them to you. I might even do it without the cash. I won't do it without the mailer, disk, and postage. Bill Powers, 73 Ridge Place CR 510, Durango, CO 81301.

Best, Bill P.

Date: Tue Dec 08, 1992 8:18 pm PST
TO: Gary (Ems)
  MBX: G-CZIKO@UIUC.EDU
Subject: Educational Researcher

Congratulations on these wonderful news.

[From Dag Forssell (92128)] - almost forgot - am typing "live".

I would sure like to have a hard copy as a reference item. How do I buy a single issue. Address? Price?

Can you remove the extraneous stuff and post an electronic version limited to the PCT aspects? Bill's listserver?

If you edit it and create a new version, you would have complete freedom copyright-wise, if you don't already.

Again, congratulations. Dag

Date: Tue Dec 08, 1992 8:20 pm PST
Subject: try again

Oh dear! I just read the post I sent last night from home--not a word there, huh. Well, I've got one minute to summarize what that post was supposed to say. Basically, Greg Williams sent me a post which clarified things for me really well--its alot like what

I've been saying all along, (but didn't quite know it--funny how that works). Whether it's perceptions or error that is controlled is dependent on what LEVEL from which one is speacking. From an individual ECS level, its percpetions, no doubt--the math shows that conclusively. But from the perspecive of the whole organism (the organism as an organism, as I said earlier) its error.

Basically both are equally correct from each perspective. Before I thought both were correct but one was more correct.

Anyway, its a level thing, like so many topics being--I'm suprised I didn't see it before since it fits in really nicely with what I said in my thesis concerning causality in the control loop (what causes what is depednent on the level of analysis...)

Gotta catch a bus--sory about the speelling.

(The garbled message was caused by an incoming call--*70 is not available here, unfortunately).

Mark "It is impossible to do only one thing."

Date: Tue Dec 08, 1992 8:22 pm PST
Subject: Congratulations, Gary

from Ed Ford (921208:1850)

Gary, congratulations on the two articles. Would like a copy of each as soon as possible. I want to get them to Jack Champlin and the Outcomes Based Education group.

Best, Ed

Date: Wed Dec 09, 1992 4:30 am PST
Subject: Re: Science vs. Religion

Ed,

What about the Alis and the Lis and the Krishnas in the world? Your holiday wishes left out a sizeable chunk of the world's youth, who don't buy into EITHER Christianity OR Western science. Regardless of the potential and (I believe) actual conflicts between science and certain religious ideas, it appears that the major problem is religion vs. religion.

May folks come to respect the plurality of high-order reference signals in the coming year, at least a bit more than in the past! I think that admonition applies to virtually everyone.

And stay loose (it improves your adaptability),

Greg Williams

Date: Wed Dec 09, 1992 6:26 am PST
Subject: Re: My First PCT Publication Out

Gary, all I said in my review of the article was that it should do for educational research what Chomskey's review of Skinner's Verbal Behavior did for linguistics, namely, revolutionize the field. Note I did say "should". We all know how those folks will resist disturbances to their current hierarchies, but maybe with some followup (Gary and I are planning to submit a major session at the 1994 AERA meetings), we can begin to make some headway.

Congratulations, Gary!

Hugh G. Petrie, Dean
Graduate School of Education
367 Baldy Hall
University at Buffalo
Buffalo, NY 14260
USA

716-645-2491 (Office)
716-645-2479 (FAX)
PROHUGH@UBVMSD.BITNET

Date: Wed Dec 09, 1992 10:09 am PST
Subject: Re: Science vs. Religion

Forwarding comment from office mate who sincerely believes in maintaining minimal error wrt Higher Order Reference signals. We disagree on how one receives these, however./Bill C

FROM: Paul R. O'Keefe
Subject: Re: Science vs. Religion
May the Peace of High-order Reference Signals be yours now and forever????

Date: Wed Dec 09, 1992 11:56 am PST
Subject: Science vs Religion

from Ed Ford (921209:0930)

>Greg Williams (What about the Alis and Lis and the Krishnas in the world?)

You are so right? My apologies! I liked your thought and Paul O'Keefe's thought. I'd like to repeat his.....

May the Peace of High-order Reference Signals be yours now and forever!

Best, Ed

Date: Wed Dec 09, 1992 12:15 pm PST
Subject: Congratulations

:Bill Cunningham 921209:

Gary,

Congratulations on the papers!

Despite the pain reported by others, I'm just beginning to really appreciate what a frustrating experience it is. Your success keeps us all going.

Bill C.

Date: Wed Dec 09, 1992 3:07 pm PST
Subject: controlling error (again!), sci v rel

[From Rick Marken (921209.0900)] Mark Olson --

>Whether it's perceptions
>or error that is controlled is dependent on what LEVEL from which one is
>speacking. From an individual ECS level, its percpetions,no doubt--the
>math shows that conclusively. But from the perspecive of teh whole
>organism (the organism as an organism, as I said earlier) its error.

I hate to be a pain about this but I think this debate about "controlling error" gets at the basic idea of what it means to CONTROL. I don't see how this can be a matter of perspective. The error signal is a physical variable. It is either controlled or it is not. It is a particularly interesting variable because it is a component of a negative feedback control loop, but it is still a variable like any other variable. If the determination of whether or not it is controlled is a matter of perspective then the same is true for the determination for whether ANY variable is under control. This would mean that the basic methodological tool for investigating control phenomena (the test for controlled variables) must be informed by the perspective (or level) from which it is done. This would make it VERY important for those of us who want to study control phenomena to understand clearly how perspective might influence the results of this test. So I ask you (and Greg) to explain how "perspective" (or "level") influences the results of the test for controlled variables. If the "error" variable can be "not controlled" from one perspective but "controlled" from another, then I could be coming to the wrong conclusion when I say "this variable is not controlled" because I happen to have done the test from the "wrong" perspective. It also would be possible to conclude that, say, the difference between curson and target is "controlled" from one perspective and "not controlled" from another. If this is possible, then methodologists should know how to tell what perspective they are doing the test from and how it influences their results.

Ed Ford --

>I've always believed that there should be no conflict between science and
>religion.

Greg says --

>Regardless of the potential and (I
>believe) actual conflicts between science and certain religious ideas,
>it appears that the major problem is religion vs. religion.

Righ to Greg. I think Ed's grandchildren were having a religious dispute -- no science involved at all. Using scientific terms (like immune system) to describe the cause of perceptions (health) doesn't make it science. "Science" and "religion" are words that refer to lots of different perceptual variables. For me, the best definition of science was given by Bill Powers -- "disciplined imagination"; we invent models (imagination) and then test to see if we observe in perception what the model does when "switched on" (discipline). This is a nice definition because it makes it easy to juxtapose it to what I think of as the essense of religion -- "faithful imagination". The crux of the difference is the way you ultimately test whether your imaginings are"right"; In

science, the final arbiter is God -- ie.the cause of one's perceptual experience (we call her Boss Reality). In religion, the final arbiter is People -- perceptions are made to fit the faith (too often, violently). (I should note that, by this definition, much that is called "science" is not -- Lysenkoism in the USSR is an example of religion [faith in inheritance of acquired characteristics] posing as science). To my knowledge, there is no religion that would qualify, by this definition, as a science.

Yours in Boss Reality Rick

Date: Wed Dec 09, 1992 3:49 pm PST
Subject: Re: Closed-loop control for production plants?

[Found this while browsing. Ray Allis 921209.0947 pst]

Subject: Closed-loop control for production plants?
Sender: news@news.uni-stuttgart.de (USENET News System)
Organization: UNIVERSITY OF STUTTGART, ISW
Date: Wed, 9 Dec 1992 07:30:34 GMT
X-News-Reader: VMS NEWS 1.22
Lines: 21
Content-Length: 854

To all control people, AI people, production planners,... out there:

As you may know, today's production and factory control systems (PPC, shopfloor control systems) work in an open loop manner. The dynamics of production systems (time responses, delays, stability ...) are not or only insufficiently taken into account. New developments aim on closed-loop control requiring permanent observation of relevant system variables and parameters.

Now my question: Has anyone heard about the appplication of classical or modern control theory or modelling techniques (for continuous or event driven systems) to the problem of production planning and shopfloor control? All hints and comments are appreciated.

P. Demel
Institute for Control of Machine Tools and Manufacturing Systems
University of Stuttgart
Seidenstr. 36
D-7000 Stuttgart 1
Germany

Date: Wed Dec 09, 1992 3:56 pm PST
Subject: controlling error, grandmother, perceptual realizations

[from Wayne Hershberger, 921208] Re: catching up on a month's E-mail.

First things first:
  Bill and Greg, I second Oded's recommendation that you consider _Nature_, if Mary's ploy doesn't get _Science_ to wake up and smell the coffee.

Gary, congratulations on your two _Educational Researcher_ articles. You have positioned yourself perfectly to augment (and take advantage of) the paradigm shift we all are anticipating!

Now, to address a couple threads:

Controlling Error:

> (Audra Wenzlow 92.12.02) The notion of "controlling perceptions" has always disturbed me precisely because it does not include the reference signal in its description.... Without a reference setting, controlling a perception would have no meaning....For instance, in the rubber band experiment, I don't really care where the knot is, only how far I perceive it to be from the reference point. In other words, I am not controlling my perception, but the difference between my perception and my reference level. The fact that controlling occurs relative to some reference setting is precisely what seems to be disregarded when we say that we "control our perceptions."

You are right, Audra, to be concerned about the reference signal! However, the fact that controlling occurs relative to some reference value (of a PARTICULAR perceptual dimension or scalar) is precisely what tends to be disregarded if we claim that we merely "control our error." Imagine yourself at the chalkboard engaged in the rubber band experiment, and all you can see is a meter registering how much error is present: the absolute, not the algebraic amount--an algebraic amount presupposes a perceptual dimension (e.g., right/left) and that is what you are supposing is irrelevant. What will happen? A lot of blind variation. The knot will move around like an E-Coli bacterium. Controlling error without controlling particular perceptual variables amounts to poor control of error!

The perceptual dimensions (or scalars) comprising the spatial coordinates of our psychophysical world is what allows us to control the position of a manipulandum (e.g., the knot) with a minimum of false starts--the error in the control loop is kept low precisely because the reference value is specified in such perceptual terms as "intending to keep the knot on the dot." An error signal's perceptual origin is what gives it the requisite dimensionality for efficient control (feedback must be negative); for example, as compared to the reference value, the controlled perceptual variable may be too far right, too far left, too high, too low, too bright, too dim, too loud, too soft, too near, too far, too red, too white, etc. etc. In contrast, an error which is simply "too much," is virtually a joke; such control is possible but it is a very limited (special) case.

Because reorganization actually requires blind variation, it is reasonable to assume that reorganization involves the control of intrinsic error (the Hullian behaviorists called it drive reduction and trial and error learning; Freud spoke of libido and the pleasure principle), but that is a very special case (which Mark Olson and Bruce Nevin seem to recognize).

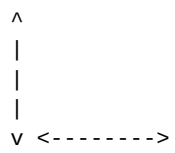
Perception: Realization vs. representation

> (Rick Marken (921020) What is your model of perception,

> Wayne? Here is my model: EV --->S--->PF--->PS, where EV is an environmental variable, S is a sensor, PF is a perceptual function and PS is a perceptual signal....In your example of "diagonal movement", PS IS the perception of diagonal movement -- constructed from the sensory inputs that are ultimately caused by the horizontal and vertical EV movements. What else is needed here -- other than the delineation of how PS results from EV

Rick, you are missing the point! The problem with this schematic (I hesitate to call it a model or to say it's yours; I recognize it as Thomas Reid's idea) is not so much what it lacks as what it presupposes, namely that there is some realization process (EV) that is distinct from and CAUSALLY a priori to the process of perception. In short, you've got the PF too localized. What you are calling PF must permeate the whole process--so you beg the question by characterizing EV as horizontal and vertical movements. Let me repeat myself, particularly the part in capital letters concerning EV:

> (Wayne Hershberger 921018) The perceptual process involves an ecological dipole. Perception is NOT simply a process of transporting a representation of some putative conceptual reality comprising one end of the dipole (the environmental pole) into the other end (the organism pole). Such a conceptualization (representationalism) begs the fundamental question of perception, which is the realization of the perceptual world in the first place. For example, consider two light emitting diodes (LEDs) moving in phase in the dark, one vertically, the other horizontally, like this (i.e., conceive it thus):



> Doing this, Gunnar Johansson found that one perceives diagonal motions. The two lights are seen as separating diagonally as the PAIR moves along the orthogonal diagonal, from lower left to upper right; and then as the PAIR moves diagonally back to the lower left the two lights are seen to approach each other, diagonally. Describing the LED's motion as vertical and horizontal is a conceptual convenience. And it is realistic. But this conceptual reality doesn't account for the diagonal motions that are perceived. And for the same reason, it doesn't account for the perception of vertical and horizontal motion (e.g., when the room lights are on), either. Why should it?
>
> The phenomenon described above illustrates the difference. One can conceptually represent (i.e., re-present) a perceptual reality as I did above in describing the LED's motion as vertical and horizontal--that is, I have described the motion (i.e., conceptually realized it) as it

> looks (i.e., is perceptually realized) in the light. But this equivalence (between the perceptual and conceptual realizations) is an accident of the room lights being on; when the room lights happen to be off, the equivalence vanishes. However, this is not to say that the perceptual realizations (vertical/horizontal motion or diagonal motion) are themselves an accident of the room lights being either on or off. THE PERCEPTUAL REALIZATION DEPENDS LAWFULLY UPON THE STATUS OF THE ROOM LIGHTS--SAME BRAIN, DIFFERENT INPUT. THE INPUT, OF COURSE, IS NEITHER "DIAGONAL MOTION" NOR "VERTICAL/HORIZONTAL MOTION," BUT RATHER SOMETHING FROM WHICH THESE ALTERNATE REALIZATIONS MAY BE ACHIEVED. PERCEPTUAL REALIZATIONS ARE NOT CAUSED, THEY ARE ACHIEVED.

It seems to me that Bill captured this perspective perfectly in a recent post--providing one recognizes that the environment is just as much an analog "computer" as the nervous system:

> (Bill Powers (921102) When you put signals into an analog computing network, you get signal variations everywhere in the network, not just at the nominal outputs. All of these variations have meaning, because they stand in continuous relationship to the inputs, according to rules that are the properties of the intervening devices. There isn't a symbol anywhere in the network: just voltages and currents behaving through time. Of course the user of the computer assigns symbolic meanings to the various signals: this is a muscle tension, that is an acceleration, the other is a sum of forces. But the computer itself operates without any such interpretations.

Grandmother Level:

Bill, it seems to me that Martin and Oded's points are well taken. A particular constellation of features or attributes (e.g, Grandmother) is not necessarily realized as a particular scalar value at some unique level (grandmother level?) of perceptual processing. After all, we don't control grandmother as such; we only control her attributes or features, such as her proximity, her disposition, etc., etc..

When Aristotle distinguished five separate senses he also identified three principles of mental association by which the various isolated sensations, hypothetically, could coalesce to form unitary percepts. Neurophysiologist have been looking for these synthetic engrams (cells) ever since, to no avail. Although contemporary neurophysiologist are still looking, others have begun to recognize that the quest is a wild goose chase. Parallel processing is not necessarily isolated parallel processing. As you say, "When you put signals into an analog computing network, you get signal variations everywhere in the network, not just at the nominal outputs. All of these variations have meaning, because they stand in continuous relationship to the inputs, according to rules that are the properties of the intervening devices."

New Software:

Bill, the 5.25" floppy you sent me recently with the latest version of Demo2 appears to be defective. Could you please send another. Could you send copies of you manuscrits as well--the current ones with Greg and Tom?

Warm regards, Wayne

Date: Wed Dec 09, 1992 4:20 pm PST
Subject: Bob Clark's proposals

[From Bill Powers (921209.0900)] Bob Clark (921205) --

>It is my impression that Powers did not, and does not now, consider
>that book to be complete and final.

Right you are.

>However there seems to have been little discussion of possible
>changes in the original Hierarchy. Rather, there seems to have
>been discussion of various interesting and important applications
>and related ideas.

I'm glad to see you opening up the discussion. "The" hierarchy is a figment of my imagination, building on OUR imagination. For most of the levels I've proposed, the only backing for the specific definitions is anecdotal and subjective. As far as I'm concerned, these or any other levels won't be "facts" until we have put them to experimental test.

I've always felt that defining the levels scientifically is a large project, which should begin by experimentally verifying that people can control variables of many different kinds -- anything anyone can think of, without regard to levels. Even the most obvious variables should be put formally to the Test, just so we can write down the parameters of control and say that we have in fact observed such-and-such a variable to be under control by a human being. This would be a beautiful thesis, or series of them. On the other hand, maybe it should be the kind of project to which all control theorists contribute, the way astronomers put in some duty time measuring double star angles and separations (the three well-spaced observations required to determine the orbital elements needing, in many cases, 1000 years to complete).

Once we have a base of hundreds of certified controlled variables, we can begin to try to put them into order. If there really is a hierarchy, the variables will fall into classes and the classes will be related in a hierarchical way. That is, in order to control a variable of one level, it will be necessary to vary a controlled variable of a lower level. And of course the only way to vary a controlled variable arbitrarily is to alter the reference signal for the system that's controlling it. Showing that this is the case leads to a new series of experiments.

When this project is done, HPCT will become a science. We will have advanced from Galileo to Newton.

In the meantime, of course, we can argue. But without experimentation, arguments are just a pastime.

I agree with Rick Marken about decisions: they represent conflicts. Unless there were at least two competing goals to satisfy, there would be no need to make a decision. You

would just do whatever is required to achieve the single goal. More commonly, there are multiple goals involved in behavior, but we have learned to organize our actions (as a result, largely, of resolving conflicts in the past) so that all thegoals can be satisfied at once. When that is the case, again no decisions are needed.

At the level I call "programs," symbol-handling processes occur which I characterize as a network of choice points. These are like the TOTE unit, in that there are tests for conditions, with the choice of a branch being determined by a rule applied to the results of the tests. The term "choice" seems to imply a decision, but in fact there are no decisions at this level either. The conditions encountered at each choice-point, plus the rules, completely determine the path to be followed next. Only when there is ambiguity or when the rule is self- contradictory (calling for more than one mutually-exclusive path to be followed) is anything like a decision required. If you have an algorithm for making decisions, you don't have to decide anything!

Note that operations occurring between choice points are sequences, lists of reference levels to be brought about in order. Sequences are the next level below programs. Programs are concerned ONLY with applying rules to select branches, as I use the term here. They involve "flow control" as they say in programming manuals. The parts of computer programs that consist only of one instruction following another belong at the sequence level here, not the program level.

If we eliminate programs -- the execution of algorithms for choosing paths -- from decision-making, what is left? As far as I can see, only the cases in which for some reason we wish to do two contradictory things at once. At that point we must reorganize or simply suffer the paralysis of conflict.

I think that we agree on this, at least to an important extent. You say

>The Decision Making Entity, as here understood, can act
>without being bound by past decisions. It frequently uses
>them because they are readily available and alternatives
>may be overlooked. It has the ability to be arbitrary.

This arbitrariness has the flavor of reorganization. But so far, at least, I have not considered systematic reorganization. Anything that could be called systematic, it seems to me, belongs in the already- organized hierarchy. At the level of logic, systematic consideration of previous choices and possible alternatives is an algorithm. As such, it can be reduced to rules governing selection of paths connecting sequences or lists of behaviors, where by behaviors I mean controlled perceptions of the consequences of acting. When we remove all algorithms by putting them into the program level of the hierarchy, all that is left of decision-making is the arbitrary part: making a change for no reason.

>DEFINITION of the DME. The Decision Making Entity is defined in
>terms of its Connections (Inputs and Outputs) and its
>Capabilities.

>CONNECTIONS AVAILABLE
>INPUT A) information about the Current Condition of
>Physiological Systems.
>INPUT B) information about past events -- memories, recordings,
>whatever.

This can be summed up by saying that the DME receives perceptions from

lower systems that are either in the Normal mode, receiving information
that comes ultimately from introceptive or extroceptive sensors, or from
lower systems that are in the imagination mode, deriving their
perceptual signals from memory.

>REFERENCE LEVELS C) information specifying the acceptable
>operating condition of the organism, A)a). These are the
>"Intrinsic Levels" of other discussions.

This makes the DME look even more like the reorganizing system, with
reference signals specified genetically.
>
>OUTPUTS D) information that acts throughout the Hierarchy.

This is typical of the reorganizing system as I perceive it. However, I think that your DME includes both learned hierarchical systems and the unlearned system I call the reorganizing system. When a "decision" is reached, it must entail some sort of action, and to create any systematic action a higher-level system must adjust reference signals for lower-level systems. Furthermore, since nature never trusts an organism's output to do what it is supposed to do, the consequences of the action must be perceived by the level issuing the reference signals, so the reference signals can be varied until the perceived result is the intended one. If this control process takes place in an organized way, it must be due to a learned system.

In my concept of the reorganizing system, I have extracted the arbitrary non-systematic kind of action from the hierarchy as a whole and given it a separate existence of its own as a built-in aspect of the organism that functions from the beginning of life. We used to call this the Negentropy System. I gave up the word because it implies things I don't believe. I now just call it the reorganizing system.

>CAPABILITIES
>A) Directing ATTENTION.
>a) Selecting the information, INPUT A), to be controlled.
>b) Selecting information from past events, INPUT B), for
>comparison with the current situation.
>c) Comparison of the current and projected ("anticipated")
>situations with acceptable magnitudes of the variables
>selected for control -- especially Intrinsic Variables, C),
>when they are relevant. Everyday situations usually do not
>directly involve Intrinsic Variables.

I will accept as part of the reorganizing system the direction of attention. The rest I have incorporated into the hierarchy itself. I am not sure what attention is for. We need to do some experiments to find out.

The comparison of current and projected magnitudes of variables with acceptable magnitudes is simply the operation of any control system atany level ("projected" magnitudes require the imagination connection). That kind of operation is adequately handled by the "canonical" control system diagram, and when Intrinsic Variables are not involved is simply the operation of the learned hierarchy of control systems. I allocate Intrinsic Variables and Intrinsic Reference Signals strictly to the reorganizing system, whose actions are arbitrary and random and serve to alter connections and weights in the learned hierarchy. That is how the learned hierarchy becomes organized.

>B) Decision Making.

>a) Selecting OUTPUTS, D), to be used by the DME as Reference
>Levels for the Hierarchy.
>b) Activating the Selected OUTPUTS for controlling the
>Selected Systems.

I handle all this in the higher levels of the hierarchy, but would leave decision making (as an arbitrary process) out of it.

>CONDITIONS REQUIRED
>In order to direct its attention and make its decisions, the
>organism must be Conscious. Unconscious means that the DME is
>unable to receive information from its inputs. However the
>remainder of the systems may be functional, operating on the
>basis of the most recent settings of their reference levels.
>There are several interesting situations that can occur: sleep,
>coma, paralysis, trauma, etc. These, and others, are worth
>separate discussion.

I have formed a similar idea of consciousness (beginning with our discussions of 35 years ago). However, I begin with awareness (which I think you include). Awareness is the capacity of the reorganizing system to receive information, regardless of its kind. I have proposed that when awareness is receiving information selectively from a portion of the hierarchy, the result is what we call consciousness. This allows us to distinguish between one phenomenon which remains the same no matter where it is applied -- awareness -- and another that changes its form depending on the source of perceptual signals received in awareness -- consciousness. Consciousness always takes on the character of the control systems to which awareness is connected.

Thus an apparent rule that seems to fit experience: you cannot be conscious of systems that are in the conscious mode. Instead, you are conscious of the lower-order world of perceptions received by those systems, and experience those perceptions with the conscious interpretation typical of the level (or levels) at which awareness is connected. This interpretation appears to be an objective property of the world.

Any system in the hierarchy can operate in the conscious or unconscious mode. In the conscious mode only, it is subject to reorganization.

Implied by this model is the possibility that awareness can be selectively connected to particular levels in the hierarchy. When that is the case, you experience the world consciously as that level perceives it, but you are unaware of applying any interpretation to the perceptions. Instead, you see those perceptions simply as part of the world. If you are operating in the logic or program level, you see the world as full of choice-points and alternatives, with natural rules that define a path through the choice-points. On the other hand, if you are operating in the relationship level, you see a world in which everything is related in some way; you see the constraints that make independent objects and events maintain a certain constancy of interaction.

And while you are attending from the viewpoint of relationships, you are not aware of any higher levels of perception and control. They are still operating, and if you ask yourself why you are paying attention to relationships, you will come up with higher-level reference signals -- what you hope to accomplish by attending to relationships. That is, you can often "go up a level" and realize that higher-level control processes were active all the time, even when not in consciousness. But as soon as you do that, you are no longer seeing a world of relationships. The nature of the conscious world changes as you move awareness from level to level.

I think that this proposal is related to your concept of "modes." However, I do not see these modes of consciousness as being modes of just one level, your DME. I see them as resulting from awareness moving from one place in the hierarchy to another. When one is attending to a lower level of perception, higher processes are still operating but they are not operating consciously. By your postulate, all modes would entail consciousness of the highest-level processes.

Maybe you're right. But I think experience argues against this view. At any rate, I think your picture is worth trying on for a fit.

>This PROBLEM arose in our early discussions as we sought to
>define higher levels. How can the changing behavior of an
>individual be described when he is blocked? Analysis working
>upward through the lower orders assumed (implicitly) a set of
>fixed reference levels, especially at the higher levels.

This is no longer a problem in the hierarchy as I currently conceive it (since 1973). Higher reference levels are no longer fixed, except at the highest level. At intermediate levels, lower-order reference signals are varied as needed to provide a higher-level system with the perceptions it needs to match its own reference signals -- which in turn are being varied as required by still higher systems. You might ask Rick Marken for his spreadsheet demonstration of this arrangement -- it will run on Lotus or Excel. It shows how a three-level hierarchy with six systems at each level can simultaneously control three levels of perceptual variables despite random disturbances from the environment, and despite considerable interaction among the controlled variables at each level.

>How could these be changed? How could the system be "re-organized?"
 They (reference levels) no longer require reorganization to be changed. Reorganization is now needed only when the learned systems are not capable of maintaining intrinsic variables at their reference levels (as a byproduct of their actions). Since the model now includes many "intellectual" functions such as classifying, ordering, reasoning,application of principles, and control of system concepts -- all of which are learned -- the reorganizing system does not have to carry out any rational processes.

>... what seems to be needed is the DME as suggested here.
>I find combining the DME approach with the Hierarchical view
>provides some additional answers, and leads to some revisions of the HIERARCHY.

I think you will find that the levels I have added (categories, sequences, programs, principles, and system concepts) contain much of what you want to put into the DME. I agree that such functions are required. I have simply broken them out into specific levels of functions, while reserving the arbitrary reorganizing part to a separate non-hierarchical system. I don't say that's right. It's just what I have done.

I have several arguments with your proposed levels, but will pass them up for just one clarification concerning your Fourth Order, Temporal Variables. For quite a long time after we parted, I considered just the sequence level in the position where you put it, above configurations. Then I realized that there are really two kinds of sequence variables, one exemplified by the second-hand of a clock, and the other by the notes of a melody. The second-hand of a clock gives rise to a perception of continuing angular motion, d/dt(angle). With angle as a configuration perception, the new perception is simply its time derivative. As such it has a value at all times, in present time, which can change in magnitude as the angular (or other) velocity increases or decreases.

This is quite different from the temporal progression in the successive notes of, say, "Taps," which can be played slowly or more quickly. In the case of the melody, there is no simple motion signal, but the sense of a specifically ordered progression of different sensations, one following the other. What matters is not so much the speed, but the ordering in time -- which note follows which.

On realizing this difference, I introduced the "transition" level, which is basically derivatives (and perhaps derivatives of one variable relative to another). This level went just above configurations, and is where stroboscopic as well as continuous motion or change is perceived. That left the sequence level to cover just the temporal ordering of lower-level variables, including transitions. I called this the "event" level, where an event was supposed to be a short familiar temporal pattern of perceptions of transitions, configurations, sensations, and intensities (you omit intensities).
Only a couple of years ago, Gary Cziko brought up some more examples of temporal variables in which ONLY the ordering was important -- in language, for example, the ordering of words. Here the temporal pattern was not evident, for an ordering is quite independent of how long it takes elements to occur or of the spacing between elements. This struck me as different from an event which which there is a stereotyped unitary pattern that forms a single package in perception. So the sequence levelended up being split once again, the event level now meaning only brief "packaged" temporal patterns recognized as a single thing like the bounce of a ball, with pure sequential ordering -- lists -- being moved to a higher level.

We can discuss the rest of your proposals for levels later -- I expect that others will have questions and comments, too. I am glad to see the subject opened up again, because I don't like the sensation of having my hypotheses converted into Gospel. I think that by trying to boil down all propositions to the basic underlying operations, and connecting them with experience, we can arrive at an agreeable set of levels for experimental test. Maybe the reason that there has been so little questioning of my definitions is that nobody saw any conflicting alternatives and thus didn't feel compelled to make a decision!

Best, Bill P.

Date: Wed Dec 09, 1992 5:59 pm PST
Subject: Science vs Religion

[From Rick Marken (921209.1230)] Ed Ford (921209:0930) --

>>Greg Williams (What about the Alis and Lis and the Krishnas in the world?)

>You are so right?

But doesn't it get you to thinkin', Ed?

Everybody seems to be making up a different story about god(s) and what they say about the meaning of life and how we should behave in it. Seems like what we've got here are variable means to achieve a higher order result -- varying across people, anyway. Wouldn't it be marvelous if we could learn to vary these means within one person -- ourselves. Then a "Serb" could see that s/he is "Bosnian" too -- and vice versa; an Isreali could see that s/he is Palestinian, a Catholic could see that s/he is Lutheren and an Atheist could see that s/he is Muslim, etc. The solution to the problem of religion (like the solution to any conflict resulting from inflexible goals) is not to eliminate the goal but to RISE ABOVE IT; PCT can help people get their consciousness to

the level that is served by controlling religious perceptions. Once you get up there you will see that religious goals are arbitrary -- but useful for satisfying the needs of that higher level. When you get up there you see that chosing a religion, ethnicity, nationality, etc is just as useful (and arbitrary) as chosing a nice book to settle in with on a rainy day; sometimes you want a romance and sometimes only a thriller will do.

Yours in Satori Rick

Date: Tue Dec 08, 1992 8:18 pm PST
TO: Gary (Ems)
Subject: Educational Researcher

Congratulations on these wonderful news.

[From Dag Forssell (92128)] - almost forgot - am typing "live".

I would sure like to have a hard copy as a reference item. How do I buy a single issue. Address? Price?

Can you remove the extraneous stuff and post an electronic version limited to the PCT aspects? Bill's listserver?

If you edit it and create a new version, you would have complete freedom copyright-wise, if you don't already.

Again, congratulations. Dag

Date: Wed Dec 09, 1992 11:16 pm PST
Subject: Science and Religion

[From Dag Forssell (921209) Rick Marken (921209.0900)

>...The crux of the difference is the way you ultimately test whether
>your imaginings are "right"; In science, the final arbiter is God --
>ie. the cause of one's perceptual experience (we call her Boss Reality).
>In religion, the final arbiter is People....

What a marvelous, lucid insight. And people can create and defend any system concept religion they want, teach it, fight for it and die for it. Witness the sorry spectacle in India. No Boss Reality arbiter there.

>To my knowledge, there is no religion that would qualify, by this
>definition, as a science.

Some years ago, I attended Religious Science, Science of mind (several times). They would take a text from the bible, another from the Koran, a third from some buddist book. They suggested that there have been many good teachers, but that none is a God any more or less than you and I.

In every affirmation, song and message, I was able to substitute the word God with "laws of nature". ... The one thing that was supernatural was "treatment". So I guess they fall down like all the others.

Religion is more than a system concept, though. It is also a social club. There is where much of the strength and value comes from. And the coercion. If you don't say you believe in what we say we believe in, you can't play in our sandbox. You may get ostracized from your family, friends and community. Better go to church on Sunday.

Best, Dag

Date: Thu Dec 10, 1992 1:36 am PST
Subject: Re: "Models and their worlds..."

> Hello Ray. After consultation with Greg Williams, the official
> CSG editor, publisher, archivist and house roofer, Bill Powers and
> I decided to release "models and their worlds" as a CSG technical
> report, available to the few people our former reviewers and
> editors believe might be interested.

Is there an ftp site or LISTSERV file server site from which we could download various PCT articles, this, the Intro article, etc.? If not, how could that come about? If I INDEX CSG-L, all I get is a listing of the log files.

Cliff Joslyn

Date: Thu Dec 10, 1992 1:39 am PST
Subject: perceptual realizations

[From Rick Marken (921209.2000) Wayne Hershberger (921208) --

Hi Wayne! Welcome back to the net. >Rick, you are missing the point!

Well, make that a mini-welcome (kidding!).

>The problem with this schematic
>(I hesitate to call it a model or to say it's yours; I recognize
>it as Thomas Reid's idea) is not so much what it lacks as what it
>presupposes, namely that there is some realization process (EV)
>that is distinct from and CAUSALLY a priori to the process of
>perception.

I'm just assuming that there is an external world (Boss Reality) called EV (external variable) that is the ultimate cause of perception. EV is itself a model -- physics. What's wrong with guessing that there is an EV that is distinct and causally a priori to the process of perception if the assumption works? My little model of perception is what we use successfully all the time to build control models; control engineers do it too -- they convert one (possibly multidimensional) variable -- like density -- into another (unidimensional) variable -- like voltage. This is what I think happens in perception (perception being the resulting, unidimensional variable). But even if it really, really doesn't happen this way, it does work in modelling -- and I haven't seen a coherent alternative proposed -- one that would work as part of a control model anyway.

>In short, you've got the PF too localized. What you
>are calling PF must permeate the whole process

Why? The model works the way it is; what demands the change? What question is begged? We percieve -- it happens. We control -- it happens. I model perception as a many to one functional transformation. It works. I bet you model perception this way too when you are not being a philosopher. What's the problem? I suppose I'm still missing the point. Maybe you are talking about stuff that's a lot deeper than what I'm talking about. If not, maybe you could show me how your ideas about perception fit into the control model. Then I might understand them better.

Best regards Rick

Date: Thu Dec 10, 1992 2:44 am PST
Subject: Re: Science vs Religion

[From Oded Maler (921210 - according to "their" claendar)]

* [From Rick Marken (921209.1230)]
*
* Then a "Serb" could see that s/he is "Bosnian" too -- and
* vice versa; an Isreali could see that s/he is Palestinian, a Catholic could
* see that s/he is Lutheran and an Atheist could see that s/he is Muslim,
* etc.

Try this one:

And an enlightened PCTer would see that he is a blind behaviorist/cognitivist..

:-) Merry Hanukka --Oded

Date: Thu Dec 10, 1992 3:38 am PST
Subject: Perceiving error

From Greg Williams (921210) >Mark Olson (921208)

>Basically, Greg Williams sent me a post which clarified
>things for me really well--its alot like what I've been saying all along,
>(but didn't quite know it--funny how that works). Whether it's perceptions
>or error that is controlled is dependent on what LEVEL from which one is
>speacking. From an individual ECS level, its percpetions,no doubt--the
>math shows that conclusively. But from the perspecive of teh whole
>organism (the organism as an organism, as I said earlier) its error.

>Basically both are equally correct from each perspective. Before I thought
>both were correct but one was more correct.

>Rick Marken (921209.0900)

>So I ask you (and Greg) to explain how "perspective"
>(or "level") influences the results of the test for controlled variables. If
>the "error" variable can be "not controlled" from one perspective but
>"controlled" from another, then I could be coming to the wrong conclusion
>when I say "this variable is not controlled" because I happen to have done
>the test from the "wrong" perspective. It also would be possible to conclude
>that, say, the difference between curson and target is "controlled" from

>one perspective and "not controlled" from another. If this is possible, then
>methodologists should know how to tell what perspective they are
>doing the test from and how it influences their results.

That will teach me to post privately. Below is what I sent to Mark last Monday:

From Greg Williams (921207 - direct) Hi Mark -

I think you're being conned by the math.

The basic PCT loop doesn't control error, but the function of such loops is "ultimately" to keep intrinsic error from getting out of hand. The loops control perceptions (not error), seen individually, but the purpose of the SYSTEM of individual loops, seen from a higher perspective, is to control error (which is what surviving in order to reproduce is all about). This is the same argument-of-perspectives which I've brought up several times: the current-time perspective (emphasized by Bill P.) vs. the historical perspective emphasized by Skinner. The loops-as-currently-structured control perceptions AND are themselves modified when necessary to control errors. "Ultimates" are too verbal for my taste, yet I can appreciate the "ultimate" nature of error control in "shaping" the control structure, vs. the "pragmatic" nature ("proximal" nature?) of the moment-to-moment workings of the loops in the structure, controlling perceptions (if they're able).

So, I think the problem is that Rick leaves out where the basic loop came from: from the need to control (intrinsic) errors.

Hope this makes at least a little sense,

[end of post to Mark]

I see the function of the entire hierarchy as doing whatever it is able at a particular time to keep intrinsic errors within a tolerable range (with the result that the organism keeps living). If, at some time, that becomes impossible, then the hierarchy changes (reorganization). So at the "systemic" level, intrinsic errors are being perceived and if any of them get too large for too long, then actions are taken until they get back within "spec" (or, failing this, the organism dies) -- that's negative feedback control of the perception "too much (of this or that intrinsic) error." Still, each loop in the hierarchy doesn't control a perception of error. So error is controlled by the reorganization process, but not by the reorganized loops. Those are the different levels for which The Test shows that different "kinds" of variables are being controlled (actually, in the former case, error is ALSO a perceptual variable; it isn't in the latter case).

Rick, do you think reorganization does NOT have to perceive error? If not, how does it know when to start and stop?

Best wishes, Greg

P.S. Ooops, I see I slightly goofed above -- should have said "that's negative feedback control of 'tolerable (this or that intrinsic) error.'" The reference signal is for acceptably low error.

Date: Thu Dec 10, 1992 8:47 am PST
Subject: Re: Evolution

[Allan Randall 921210.1020]   (ps 921207.1600)

>   [From Bill Powers (921204.1430)]

>   But I am still bothered by the idea of the environment, the stupid
>   environment, selecting anything. The problem is a lot like that of S-R
>   theory.
>   ...
> because the environment is *not* ``non-living.'' take away all the
> living parts of earth's environment, and what do you get? something
> ...
> any bioecology, or probably any modern biology, textbook has scads of
> examples of how individuals, populations, and species are intimately
> interconnected. classic examples include predator/prey populations
> ...
>   --penni

While I agree that our environment is an eco-system, I'm not sure this is the answer to Bill's concerns. Are you suggesting that a non-living environment cannot, even in theory, select for survival? That it is the environment's living properties that create selection pressure? I would say that the reason the environment selects for "good" things is sinply because there are limited resources, and all possible organisms cannot be realized (only a teensy percentage in fact). The ones "good" enough to survive are all that's left to create future generations of organisms. Natural selection is not a benevolent force that somehow "decides" what's best for an organism, nor do I think it somehow relies on the living properties of its environment. Natural selection is completely dumb. It is not an actual physical force acting in the world.

Bills asks
>   Why should the environment stimulate organisms to do just those
>   things that are good for the organism? Why should the environment select for
>   survival of a species instead of its extinction? The nonliving

Just think what it would mean to "select" for extinction. This would mean those organisms incapable of survival would be the ones to survive, while organisms capable of survival would die off. This *would* require an actual physical selection force. But natural selection is not such a force, which is why it sounds so tautological to some people:

"What survives is what survives."

Allan Randall, randall@dciem.dciem.dnd.ca

Date: Thu Dec 10, 1992 8:59 am PST
Subject: Down With Private (Direct) Posts

[from Gary Cziko 921210.1600 GMT]   Greg Williams (921210) said:

>That will teach me to post privately.

He should have said, "That will teach me NOT to post privately."

I would like to encourage CSGnetters to ALWAYS post to CSGnet when corresponding to other CSGnetters on matters in any way relevant to PCT. Don't worry about clogging up the network. Nobody is obliged to read everything that comes over CSGnet.

In contrast, private (direct) e-mail should be sent on matters such as: "Hey, Joe, can you send me a copy of that great PCT paper you wrote that has been rejected for publication 15 times."--Gary

Date: Thu Dec 10, 1992 9:25 am PST
Subject: Reaching Deficit

[from Gary Cziko 921210.1622 GMT]

Bill Powers, Greg Williams, Joe Lubin, other "brain" people:

Saw a videotape last night shown by Robert T. Knight (UC Davis) of a patient who was unable to reach small objects in front of him. The experimenter would hold a comb within the patient's reach and the patient would extend his arm about a foot shy and attempt to grasp it. The only way the patient could reach the comb was by first making contact with the experiments shoulder or arm and then following it to the comb. The patient could also not reach to objects emanating sound in a blind condition.

The patient had had a stroke resulting in bilateral lesions in the superior parietal lobe (apparently it is very rare to see this bilaterally).

Comments? Explanations? Can we give the Little Man a stroke like this?--Gary

Date: Thu Dec 10, 1992 10:20 am PST
Subject: language as grooming

[From: Bruce Nevin (Thu 921210 12:06:56)]

In _New Scientist_ for 11/21/92 (28-31) is a piece by Robin Dunbar (prof. of biological anthropology, University College London) "Why gossip is good for you", which discusses his proposal that language arose as a more efficient way of accomplishing what grooming accomplishes. A forthcoming BBS target/commentary issue will be on just this topic, with the target article by Dunbar.

There are other functions of language that must have been important in its evolution. I am thinking of the survey of these in _A theory of language and information_ (Zellig Harris, Oxford 1991), Chapter 12, The nature and development of language. This is the distinction between information and communication again. Dunbar's emphasis is on communication as a function of and constitutor of social relations.

Here is the BBS announcement:

Linguist List: Vol-3-972. Thu 10 Dec 1992. Lines: 77

----------Messages----------

1)
Date: Wed, 2 Dec 92 21:45:39 EST
From: "Stevan Harnad" <harnad@Princeton.EDU>

Subject: Brain, Language & Evolution: BBS Call for Commentators

Below is the abstract of a forthcoming target article by R. Dunbar on cortex, language and evolution. It has been accepted for publication in Behavioral and Brain Sciences (BBS), an international, interdisciplinary journal that provides Open Peer Commentary on important and controversial current research in the biobehavioral and cognitive sciences. Commentators must be current BBS Associates or nominated by a current BBS Associate. To be considered as a commentator on this article, to suggest other appropriate commentators, or for information about how to become a BBS Associate, please send email to:

harnad@clarity.princeton.edu or harnad@pucc.bitnet or write to: BBS, 20 Nassau Street, #240, Princeton NJ 08542 [tel: 609-921-7771]

To help us put together a balanced list of commentators, please give some indication of the aspects of the topic on which you would bring your areas of expertise to bear if you were selected as a commentator. An electronic draft of the full text is available for inspection by anonymous ftp according to the instructions that follow after the abstract.

CO-EVOLUTION OF NEOCORTEX SIZE, GROUP SIZE AND LANGUAGE IN HUMANS

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London WC1E 6BT

KEYWORDS: Neocortical size, group size, humans, language, Macchiavellian Intelligence

ABSTRACT: Group size is a function of relative neocortical volume in nonhuman primates. Extrapolation from this regression equation yields a predicted group size for modern humans very similar to that of certain hunter-gatherer and traditional horticulturalist societies. Groups of similar size are also found in other large-scale forms of contemporary and historical society. Among primates, the cohesion of groups is maintained by social grooming; the time devoted to social grooming is linearly related to group size among the Old World monkeys and apes. To maintain the stability of the large groups characteristic of humans by grooming alone would place intolerable demands on time budgets. It is suggested that (1) the evolution of large groups in the human lineage depended on the development of a more efficient method for time-sharing the processes of social bonding and that (2) language uniquely fulfills this requirement. Data on the size of conversational and other small interacting groups of humans are in line with the predictions for the relative efficiency of conversation compared to grooming as a bonding process. Analysis of a sample of human conversations shows that about 60% of time is spent gossiping about relationships and personal experiences. It is suggested that language evolved to allow individuals to learn about the behavioural characteristics of other group members more rapidly than is possible by direct observation alone.

Linguist List: Vol-3-972.

Bruce Nevin bn@bbn.com

Date: Thu Dec 10, 1992 10:20 am PST

Subject: Gopher for File Retrieval

[from Gary Cziko 921210.1715]

Forget UNIX. Forget FTP. Forget putting and getting files. Try Gopher instead.

Attached is an article from a local campus publication on Gopher. Using Gopher, you can easily get connected to the CSG file server. From my local Gopher server, I find the CSG directory by selecting items in this order:

Other Gopher and Information Systems
North America
Bedford Institute of Oceanography (Canada)
Public Transfers To and From BIOME
Control Systems Group

The first item may differ depending on your local Gohper server, but once you find North America (quite big, you really can't miss it), the rest of the sequence should be the same. Some programs (like TurboGopher I use on my Mac) allow one to set up "bookmarks" so you can go directly to Control Systems Group the next time you use Gopher.

Gopher allows you to read and transfer text files and transfer program files to your personal computer (although I like a Mac program called Fetch better for transferring program files since Fetch automatically unbixes and unstuffs files on the fly).

Find out about how to use Gopher from your local Internet link. If they don't have Gopher, tell them you must have it. If you use ATT Mail or MCI Mail or CompuServe, etc., tell them you must have Gopher.

With Gopher, the world is at your fingertips, not just the CSG fileserver.

Gopher it.--Gary

P.S. Using Gopher, I found an e-mail address for contacting Phil Runkel and found out his wife works for the library at the University of Oregon. Who knows what info I may be getting about YOU?
++

Gopher: The link across campus, around the world

By Melissa Mitchell

To the uninitiated, a gopher probably isnt anything more than a burrowing rodent with wide cheek pouches. But to an increasing number of computer network users, a gopher also can be a link to a world of information. Gopher is the general term applied to a type of electronic information server, or bulletin board, that is accessible through the Internet, a computer network that connects academic computer users worldwide. The original gopher software was developed by computer programmers at the University of Minnesota, who named it for you guessed it the UM mascot. Lynn Bilger, co-administrator of gopher in the UIs Computing and Communications Services Office, said the Minnesota model has been widely copied by universities throughout the world, including the UI. Minnesota still acts as the clearinghouse, but gopher servers now can be found all over the world, Bilger said, noting that about 200 organizations from Austin to Australia put information into gopher. Whats fascinating is that people

everywhere are contributing ideas and programs. The UI created its own gopher server last summer as a replacement to a previous information distribution system called GUIDE. Gopher provides fast and easy access to information ranging from campus and regional events to consumer reports and weather data. Unlike its predecessor, gopher can be used not only by those with access to a university mainframe computer, but by anyone using either a modem or a personal computer connected to the campus network. Paul Gibbs, the other co-administrator of the UIs gopher, said there are several ways to get access to gopher. If you have a personal computer on the network you can get a client program and use gopher from your desk, he said. If you dont know how to use ftp to get programs, ask your departmental computer administrator or one of the CCSO Microcomputer Consultants at 244-0608 to help you. Gopher also is installed at all the CCSO public microcomputing sites. In addition to general information about the university, lectures, entertainment and sporting events, several campus publications including Inside Illinois and the Daily Illini are now online on gopher. The Library Computer System can also be accessed via gopher, and information about course listings can now be gleaned through the online Timetable. The student/ staff directory is on line as well. Just added, Bilger said, is a listing of holiday hours for the Library, McKinley Health Center, Intramural- Physical Education Building and other campus facilities. The hours can be found in gopher under campus announcements. Bilger said gopher traffic keeps increasing as more people learn about the service. During a typical week, more than 50,000 files or menus are read in the local service alone. Why is gopher so popular? For starters, its simple to use. You do not need a manual to learn how to use it, Bilger said, adding that gophers hierarchical menu structure allows users to move back and forth quickly through files. And, gophers indexing capabilities make it easy to sort through huge amounts of data. Another attractive feature of the information server is that it is truly a one-stop information outlet. Through the UIs gopher server, users can also get access to gopher servers at other locations. That could be useful for a number of reasons, among them, to retrieve phone numbers or electronic-mail addresses of colleagues at institutions that maintain online directories, Bilger said. Still another good reason to tap into gopher is that the price is right. Its free, Bilger said. For additional information about gopher how to use it or contribute information to it contact Bilger at 333-6236, or by e-mail at c-bilger@uiuc.edu; or Gibbs at 244-5905, or by e-mail at gopher@uiuc.edu.

Gary A. Cziko

Date: Thu Dec 10, 1992 11:29 am PST
Subject: Arbitrary goals, cntrl of prcpt of error

[From Rick Marken (921210.0900)

Oded Maler (921210 - according to "their" claendar)]

And you say I'm predictable.

>Try this one: And an enlightened PCTer would see that he is a
>blind behaviorist/cognitivist.

Yes, indeed. In fact, some years ago I was a behaviorist and then a cognitivist (without the pejoratives). I think you are trying to make the point (in your typically charming and straight-forward way) that people who have adopted a particular high-level goal (like "being a PCTer" or "being an Israeli" --did I hit a nerve?) think of it as the "right" goal. And they are the "right" goals if they achieve the higher order goal. When I said that these goals are "arbitrary" I did not mean that one can select them arbitrarily. Like any goal in the hierarchy(except fot those at the highest level) , these goals are

set in order to achieve higher level goals -- which means they must prduce the intended higher level perceptions in the context of prevailing circumstances (disturbances). So a person raised in a Serbian home will probably find it easier to control perceptions like family acceptance, self image or whatever by adopting Serbian type goals. The goals I spoke of are arbitrary goals in the same sense that the position of your hand is arbitrary and arbitrary goal -- you can set it to anything you like. It is a variable. But when you have a particular higher order goal (like putting away a forehand smash in a 20 mph crosswind) the higher order goal and prevailing circimstancesdetermine what the setting of the lower order goal must be (at that time). Going up a level just lets you see that the goal (being a Serb, holding the hand in a particular way) is done in the service of the higher level goal.

I think the higher level goals that are satisfiedby being a PCTer rather than a behaviorist are of a different sort than those achieved by being a Serb rather than a Croat. But the result is the same. I am a PCTer because the model satisfies my higher order goal of understanding the kind of data that I think is important. If I were shown that the cursor is really the cause of handle movements in a tracking task, I would, indeed, return to behaviorism or cognitivism. But, I admit, the only reason I am a PCTer is because it works for me (ie. satisfies some higher order goal) which, I think, I would call "scientific value". Unlike Serbs and Israeli's, I'm willing to change if disturbances (Boss Reality) make it so that PCT no longer satisfies that higher level goal (whatever the hell it really is). You can test this, if you like. Just do an experiment that demonstrates tha stimuli (or a cognitions) are the cause of a purposefully produced (controlled) result.

Greg Williams (921210) to Mark Olson --

>So, I think the problem is that Rick leaves out where the basic loop came
> from: from the need to control (intrinsic) errors.

I left it out only because this is still control of perception -- the error signal (or some aggregate measure thereof in the hierarchy) can become a perceptual input to a control system (the reorganization system) -- and this is, indeed, a very important possibility. In this case, the control system controls a perceptual variable (which represents error) by affecting the parameters of the control systems in which the error exists. In this case there is control of error -- but it is still control of a perception of error.

>Rick, do you think reorganization does NOT have to perceive error? If not,
>how does it know when to start and stop?

Again, error is indeed one variable that the reorganization system might control. And, of course, in order to control it, it must perceive it. But the error in the control system that is controlling the perception of error is still not controlled.

Best   Rick

Date: Thu Dec 10, 1992 1:26 pm PST
Subject: molecular computing

[From: Bruce Nevin (Thu 921210 13:38:04)]

The November 1992 issue of IEEE Computer has articles on the theme of molecular paradigms for computing. Looks absolutely fascinating and relevant to evolution and foundation

issues in PCT. Don't know when I'll have time to read it, but thought I'd pass the ref along.

Bruce bn@bbn.com

Date: Thu Dec 10, 1992 4:06 pm PST
Subject: RE: Down With Private (Direct) Posts

[FROM: Dennis Delprato]

A public big congrats to Gary on the two papers. Please send hardcopy reprints if available to:

Dennis Delprato
Dept. of Psychology
Eastern Mich. Univ.
Ypsilanti, MI 48197

Date: Thu Dec 10, 1992 5:38 pm PST
Subject: Re: Evolution

(ps 921210.1400)

> [Allan Randall 921210.1020]
>
> >
> > (ps 921207.1600)
> >
> > [From Bill Powers (921204.1430)]
> >
> > But I am still bothered by the idea of the environment, the stupid
> > environment, selecting anything. The problem is a lot like that of S-R
> > theory.
> > ...
> > because the environment is *not* ``non-living.'' take away all the
> > living parts of earth's environment, and what do you get? something
> > ...
> > any bioecology, or probably any modern biology, textbook has scads of
> > examples of how individuals, populations, and species are intimately
> > interconnected. classic examples include predator/prey populations
> > ...
> > --penni
>
> While I agree that our environment is an eco-system, I'm not sure this
> is the answer to Bill's concerns. Are you suggesting that a non-living
> environment cannot, even in theory, select for survival? That it is the
> environment's living properties that create selection pressure?
> [...]
> Natural selection is not a benevolent force that somehow
> "decides" what's best for an organism, nor do I think it somehow relies
> on the living properties of its environment. Natural selection is completely
> dumb. It is not an actual physical force acting in the world.

i wasn't addressing the issue of ``natural selection'' _per se_. (i don't believe in a reified/deified Natural Selection any more than i believe in Plans or Sentences.) i took bill's original msg. to be arguing that selection must happen Inside the organism rather than Outside it. he offered as support for this argument the claim that the organism is living (and therefore has Purpose) and the environment is not (and therefore has no Purpose). i find this claim weak, because *every* organism's environment contains some to many other organisms, all of which have Purpose as much as our original organism does.

so, regardless of the interactions amoung these Purposes (of which i am sure there would be many many), they're Out there in the organism's environment. the Environment may or may not have a monolithic Purpose (and may or may not be stupid), but clearly many aspects of the Environment do have Purpose. i think this is a much more interesting and fruitful premise from which to consider selection pressures and evolution than a premise based on a strict Inside/Outside division.

(btw, i don't really believe in Purpose, either; i was stipulating it for the sake of this discussion.)

cheers. --penni

Date: Thu Dec 10, 1992 7:34 pm PST
Subject: science and religion

from Ed Ford (921209:1955)

The purpose of my posting ((921208) was to share with friends a delightful and amusing interchange between two innocent children. I then added my own holiday best wishes. What I enjoyed the most about their conversation was how different their perceptions were and how they tolerated that difference. It was meant to be light and amusing and not a serious comment on or about religion.

Years ago, the forward to a movie with a religious theme read as follows: "To those who believe, no explanation is necessary; to those who don't, no explanation is possible."

>Rick Marken (921209) But doesn't it get you thinkin', Ed?

It certainly does, and has, all my life. It has absorbed my thinking more than anything else (PCT and its applications coming in a close second during the last 13 years; economics, third).

>The solution to the problem of religion (like the solution to any
>conflict resulting from inflexible goals) is not to eliminate the
>goal but to RISE ABOVE IT; PCT can help people get their consciousness
>to the level that is served by controlling religious perceptions.

It depends on whether, for a particular individual, his/her religion presents a conflict. For me, it doesn't. It is highly compatible with everything else that goes on in my head, including and especially PCT. Secondly, I see it at my highest level. It does give me satisfaction at the very highest (in terms of priorities) systems concept I have, which, for me, is to be one with my maker. Obviously, there are standards that flow from that system and choices I make based on those standards.

>Once you get up there you will see that religious goals are

>arbitrary - but useful to satisfying the needs of that higher
>level.

I think it would depend on an individual's own perceptions of "when he was up there" and how he perceived his "religious goals and the other needs at the higher level." It is interesting when someone comes on the net and claims to understand PCT, and then says things that are obviously different from what we all have experienced through our own individual work. Even among those in the CSG, we all understand PCT according to how we have created it in our perceptual system, from what we have done, read, observed around us, perceived as useful, experienced in creating ideas from it, perceived from building models based on it, etc. Many of us have understandings that others will never have. My wife's understanding of what it is like to have a child is quite different from mine, and, obviously, I'll never understand her experiencial knowledge.

>When you get up there you will see that chosing a religion,
>ethnicity, nationality, etc is just as useful (and arbitrary) as
>chosing a nice book to settle in with on a rainy day; sometimes
>you want a romance and sometimes only a thriller will do.

Again, "you will see" refers to what you perceive not what everyone will perceive "when you get up there." I think you presume a lot when you state you understand everyone's knowledge of how they will perceive an experience you've only had in terms of your own individual perception of your own created experience. Your knowledge of religion is limited to what you presently perceive just like my knowledge of PCT (or anything else) is limited to only what I have built into my own perceptual system.

>Dag Forssell (921209) Some years ago, I attended Religious
>Science...They suggested that there have been many good teachers,
>but that none is a God any more or less than you and I.

I'm happy to learn "they suggested that there have been many good teachers, but that none is a God..." That was their perception and you accepted that as yours. And I respect that. However, I don't happen to agree with that statement. That's my perception.

>Religion is more than a system concept, though. It is also a social club.

Again, that is your perception. And again, I don't happen to agree with that statement. My own particular religion is based on fact, not fiction. It is also based on 50 years of thought, study, research, and lots of reading. More importantly, the CSGnet is NOT the time nor place to discuss such matters. As I said above, the purpose of my post was to reflect on what I thought to be a delightful and amusing interchange between grandchildren. And, at the same time, to wish everyone an enjoyable holiday season. Next time, I'll choose my words more carefully.

Have A Nice Holiday, Ed

Date: Thu Dec 10, 1992 7:35 pm PST
Subject: Congrats to Gary

[From Rick Marken (921210.1850)]

Well, I'd better join in the outpouring of congratulations to Gary Cziko or be conspicuous by my absence. So congratulations, Gary, for successfully publishing not one but TWO papers on perceptual control theory.

I, of course, don't really care about publishing -- those journals are probably sour anyway.

Very good work Gary Regards The Fox

Date: Thu Dec 10, 1992 8:01 pm PST
Subject: Misc subjects

[From Bill Powers (921210.1900)] Rick Marken (921209.2000)--

You made a good point to Wayne:

>What's wrong with guessing that there is an EV that is distinct and
>causally a priori to the process of perception if the assumption
>works? My little model of perception is what we use successfully
>all the time to build control models; control engineers do it too
>-- they convert one (possibly multidimensional) variable -- like
>density -- into another (unidimensional) variable -- like voltage.
>This is what I think happens in perception (perception being the
>resulting, unidimensional variable).

This is what I've been trying to get across, too. We can set up a model environment in which there are several variables -- "v's." The input function can produce a perceptual signal that is any weighted sum of these variables, arbitrarily weighted. Then we can set up the output so that it affects these variables either positively or negatively as required for negative feedback, but all by the same amount, proportional to the integral of the error signal. This control system will proceed to control its perceptual signal just as if there were a single external variable corresponding to it. If any or all of the v's are disturbed, singly or in any combinations, the perceptual signal will be maintained close to the given reference signal by an appropriate change in the output effects on all the variables..

This certainly takes care of Wayne's objection to "representation." The perceptual signal is in no way a "representation" of the collection of environmental variables. There is no one variable in the environment that corresponds to the perceptual signal. Yet as the external variables change, the perceptual signal will change. As far as the control system is concerned, it is perceiving something "out there" -- but that something is shaped as much by the form of the input function as by the behavior of the external variables.

So this control system experiences an entity in the environment that does not in fact exist there. It is creating the entity. Nevertheless, the behavior of the environment is lawfully and reliably related to the perceptual signal, as long as the form of the input function remains the same.

If higher systems have access to the environment only in the form of perceptual signals from control systems like this (or similar perceptual functions not involved in control systems), they can know nothing of the individual variables outside the lower systems. They can never experience anything of the external variables except a certain weighted sum of their sensory effects. And this weighted sum can be changed by changing the weightings on a continuous scale. EVERY weighting will produce a controllable perception.

This is my basis for concluding that while perception does depend on variables outside the sensory organs, it does not correspond to entities outside the nervous system. The environment has far more degrees of freedom than our perceptions of it have. Our perceptual systems literally "make sense" of the environment -- simplify it, and present a function of it to us in the form of experience.

Woldgang Zocher --

I'll get back to you in a day or two. Too much to do right now.

Greg Williams (921210) --

RE: control of error.

It still grates for me to hear of error signals being controlled, or for that matter, perceived. Error signals exist, but I associate perception strictly with the output of an input function: a report on the current state of affairs, as sensed. By extension, this applies to the reorganizing system as well.

In this discussion, the importance of reference signals keeps getting lost. If it were not for reference signals, error would be undefined. Intrinsic or critical variables do not have reference levels of zero. Body temperature, body weight, blood glucose, blood pH, and numerous other basic variables are controlled with respect to NON-ZERO reference levels. The inherited reference signals determine what level of the sensed variable will be considered "zero error." To say that these systems "control intrinsic error" ignores the crucial information about what level of the monitored variable constitutes zero error. It is not the zero-error state that is important to the organism: it's the state of the critical variable when error is zero.

All control systems PRODUCE zero error, near enough. But that tells you almost nothing about what they DO. What's important to the organism is not the error, but the level of the controlled variable, as perceived. An organism with a zero reference level for food would produce zero error just as readily as an organism with a reference level of 100 grams of intake per day. But the first organism would die.

So you see, I can't buy your statement:

>I see the function of the entire hierarchy as doing whatever it is
>able at a particular time to keep intrinsic errors within a
>tolerable range (with the result that the organism keeps living).

Yes, it keeps intrinsic errors as close to zero as possible. But what matters about that is not the error, but the consequent maintenance of the milieu interieur in a pattern matching intrinsic reference levels. The error signal itself is just a signal -- it has no importance of its own in the bodily ecology.

Allan Randall (921210.1020)--

My musings about evolution reflect my inner confusion on the subject. I don't mean that natural selection doesn't work. As you say, "What survives is what survives." I just don't think that that process is efficient enough to produce complex organisms. This is why I was complaining about "external criteria" in Genetic Algorithm experiments.

Real evolution gives no credit for a partial solution to a problem of acquiring a complex behavior, yet the (few) experiments I have heard of all used external criteria, allowing simulated organisms to survive if they simply made a change in the right direction.

I read about some "maze-running" experiments recently, in which the organisms had to learn a very complex path to a final goal. I'll bet you that the initial survivors were allowed to survive without actually getting to the goal. This implies that each stage of progress had some selective influence. In order to get all these intermediate selective influences to select for progress toward the goal, somebody had to know where the goal was -- which part of the maze was the right part to get to. Somebody had to specify intermediate selectors that would lead in the right direction. In effect, it was a put-up job: somebody built a statistical funnel and dropped the organisms into it, and naturally they came out where the spout was pointed -- by the modeler.

This is what I claim that the natural world doesn't do. It doesn't care that there is food in one part of the maze but not in another. It doesn't care whether an organism is selected to do something useless or something useful to the organism. This kind of selection, I think, has to be based on INTERNAL criteria -- that is, reference levels inside the organism that define the right states of critical variables. When the critical variables begin to depart from the right state, the organism commences randomly changing itself: it mutates. As we know from E. coli, these mutations can be totally random, yet produce a systematic effect. By selectively turning mututation on and off the organism can keep control of the most critical variables. We know that this can be an astonishingly efficient method of control despite the completely random element in it. And there is no reason to think that this method of control can't exist at the level of DNA.

Of course such control systems must themselves have arisen from ordinary natural selection.

Oded Maler (921210) --

I agree with Rick. If you want to understand PCT, you must also be able to be a behaviorist and a cognitivist, to see how the world makes sense from those points of view. This is especially true if you want to show what is wrong with the other views: you first have to understand how they could seem right.

Bruce Nevin (921210) --

OK on the neural net chips. My phone is (303) 247-7986.

I'm unimpressed by Dunbar's thesis: why isn't neocortical volume a function of group size? And who sez that this is a real relationship at all? This is just another of those useless statistical facts, suitable for the foundations of castles in the air. So there.

Best to all, Bill P.

Date: Thu Dec 10, 1992 10:27 pm PST
Subject: Do you believe in purpose?

[From Rick Marken (921210.2200)]

(ps 921210.1400)

>(btw, i don't really believe in Purpose, either; i was stipulating it
>for the sake of this discussion.)

It's pretty late for me but this comment woke me up.

What is this Purpose that you don't beleive in, Penni?

In PCT, "purpose" is virtually synonymous with "control". Actually, the common sense of the word "purpose", as in "behaving with a purpose" or "his purpose was to" is synonymous with PCT's "reference level of a controlled variable". The physical realization of this purpose is the setting of the "reference signal" that determines the particular level to which the controlled (perceptual) variable will be brought and maintined.

Why don't you believe in purpose?

It's certainly OK if you don't -- but then I can see why PCT would be a pretty uninteresting theory to you.

Best regards Rick

Date: Fri Dec 11, 1992 1:26 am PST
Subject: Re: Arbitrary goals, cntrl of prcpt of error

[From Oded Maler (921212)]

* [Rick Marken (921210.0900)
*
* Oded Maler (921210 - according to "their" claendar)]
*
* So a person
* raised in a Serbian home will probably find it easier to control perceptions
* like family acceptance, self image or whatever by adopting Serbian
* type goals. The goals I spoke of are arbitrary goals in the same sense that
* the position of your hand is arbitrary and arbitrary goal -- you can set it
* to anything you like. It is a variable.

* I think the higher level goals that are satisfiedby being a PCTer rather
* than a behaviorist are of a different sort than those achieved by being
* a Serb rather than a Croat. But the result is the same. I am a PCTer
* because the model satisfies my higher order goal of understanding
* the kind of data that I think is important. If I were shown that the
* cursor is really the cause of handle movements in a tracking task, I
* would, indeed, return to behaviorism or cognitivism. But, I admit,
* the only reason I am a PCTer is because it works for me (ie. satisfies
* some higher order goal) which, I think, I would call "scientific value".
* Unlike Serbs and Israeli's, I'm willing to change if disturbances (Boss
* Reality) make it so that PCT no longer satisfies that higher level goal
* (whatever the hell it really is). You can test this, if you like. Just do an
* experiment that demonstrates tha stimuli (or a cognitions) are the
* cause of a purposefully produced (controlled) result.

I'm not really sure if there is an absolute objective sense for saying that one higher order goal is less arbitrary than another. People, being mortal, tend to adopt

higher-order goals that transcend beyond their limited time and space. Being associated with a group (of whichever size) or a nation is one obvious solution. A more sophisticated one is being associated with an idea (religion, ideology, scientific "truth").

Why, for instance, is it important at all to have a theory of human behavior? Even if TRUTH is accepted as as a goal (which is something rather problematic) why in this area? Why not try to solve some open problem in mathematics or chemistry? Your predictable (:-) answeres might be 1) to have a better world [which I don't buy; I'm sure that there are more efficient ways to do it than sending papers to be rejected from Alchemistry journals], and 2) It is related to what you already learned and know - which is a very fair answer, but not at all non-arbitrary.

I also think there are other factors (beside "objective" truth) that might lead someone to be PCTer. The romantic image of a prophet rejected (and even prosecuted) at his time (biblical, Jesus, Galileo) and then recognized by History, is very appealing to certain personalities. At a lower-level it is sometimes more enjoyable to start the morning by reading mail, learning a word of wisdom from Bill P., etc., instead of doing what you are paid for...

I also think that people "should" have more than once in their life the experience of realizing that their previous system of beliefs is wrong. If it happens only once, they might become "locked" in the second system in the sense that they refuse to see the realtive arbitrariness in it. (It relates also to moral relativism). I could give examples from the development of my own views on the Israeli-Arab conflict, liberalism/conservatism, etc. but not this time.

Arbitrarily yours, --Oded

Date: Fri Dec 11, 1992 4:16 am PST
Subject: Gopher it?

From Greg Williams (921211) >Gary Cziko 921210.1715

>Forget UNIX. Forget FTP. Forget putting and getting files. Try Gopher
>instead.
>...
>Find out about how to use Gopher from your local Internet link. If they
>don't have Gopher, tell them you must have it. If you use ATT Mail or MCI
>Mail or CompuServe, etc., tell them you must have Gopher.

>With Gopher, the world is at your fingertips, not just the CSG fileserver.

>Gopher it.--Gary

"Must have it," eh? First, convince me that I want the world at my fingertips. You sound like an BellSouth commercial. Another case of presuming others' reference signals match one's own?

Wishes (you choose if you want them best or not),

Greg

P.S. Note that I posted this to the net. I tried private-posting to Mark because I didn't have time to get into a big conversation on semantics. I still don't have time, so I'll just say I appreciate Rick's agreement on the subject of error-control and note that I think Bill's comment on that subject IS a semantic argument.

Date: Fri Dec 11, 1992 8:04 am PST
Subject: ER Papers

[from Gary Cziko 921211.0500 GMT] Rick "The Fox" Marken (921210.1850) says:

>Well, I'd better join in the outpouring of congratulations to
>Gary Cziko or be conspicuous by my absence.

Yes, I was wondering when you were going to unconspicualize yourself. Now that Marken has joined in, I don't need to hear any more about this from anybody else (please).

>So congratulations,
>Gary, for successfully publishing not one but TWO papers on
>perceptual control theory.

Well, really one article and a short rejoinder to my critics. I should add that there is really not much of interest in the articles for anyone who already has a basic understanding of PCT. It's basically a rehash job drawing mostly from Powers and Runkel and using McClelland's cruise control example and diagram of the ECS.

But I am nonetheless very pleased to see this published in a quite prominent journal, at least among educational researchers and theorists. Already some of my colleagues here said they have read it and found it interesting. But I hope they don't stop thinking I'm crazy and start giving me too much respect--that would take away all the fun (unless, of course, the respect comes with a salary increase).

>I, of course, don't really care about publishing -- those
>journals are probably sour anyway.

Yes, journals of wrath.--Gary

Date: Fri Dec 11, 1992 9:52 am PST
Subject: various

Rick,
First, I just want to mention that a week ago I thought that one could speak in terms of controlling errror at the individual ECS level--now I realize that is not correct. So the issue is wether it is relevant at a whole system level. Tou say that some system has error as its perceptual input. Well, that makes sense to me--I can go along with that--an empirical claim. But if you say that some system is controlling its perception of error, THEN isn't it the case that error is controlled, not the error of that system, but the error of the system(S) from which it recieves its input? On the one hand I went "Ah, OK, sure, perceptions of error--I see what you mean" but on the other hand its still error control. Concerning the error of the system controlling the error, I don't know what to think right now. There is a problem of infinite regress that I don't know how to get around.

Wayne,

Your dialogue with Rick has made me wonder whether we control the perceptions of what is conscious or perceptions of what is not conscious--not stated very well. What I mean is, on one level "vertical" and "horizontal" may be represented, but the EXPERIENCE of it is "diagonal." Which is what is controlled. If I give 5 quick taps on the wrist followed by 3 at the elbow and two near the shoulder, you will experience 10 equally distant spaced taps up the arm at equal intervals. S if we get inot a control example, which perception would the organism control?

Mark

Date: Fri Dec 11, 1992 10:11 am PST
Subject: csg and the brain

I knew there was somethig I forgot to say;
Concerning Gary's post about the man who couldn't reach for an object, I think this is a perfect example of how PCT'ers should give more consideration to where these various control processes occur in the brain. The superior parietal region is evidently involved in representing external space (body centered) and prefrontal areas seem to be involved in tasks which sound very much like tasks at the Sequence and Program levels.

I think Bill did a great job of constructing these levels in the manner that he did, but the hierarchy is not going to improve much anymore by thinking about it--we should look at the brain. I said 2 weeks ago that some levels seem to be invarious regions (Relationship seems to be in both Parietal and Temporal). Maybe that is OK but it seems that this indicates that some mixing and matching needs to be done. If anyone has tried to work out where some of the levels "ARE" in the brain and which levels are more "secure" than others, I would be very interested in knowing about it. Does anyone know of people with lesions which spare al abilities except the ability to perceive beauty or value or goodness (Principle)?

Mark

Date: Fri Dec 11, 1992 10:44 am PST
Subject: Re: "Models and their worlds..."

From: Tom Bourbon (921211 09:22)

Cliff Joslyn (921210 00:45:16 EST) Re: "Models and theor worlds..."

* Is there an ftp site or LISTSERV file server site from which we
* could download various PCT articles, this (TB: Models and their
* worlds), the Intro article, etc.? If not, how could that come
* about? If I INDEX CSG-L, all I get is a listing of the log files.

There is not. We only recently made the decision about letting this paper out as a tech report, after a third rejection. When Greg Willliams finishes with it, there will be figures included -- plots of data from people and simulations. I don't know of a way to include those in files distributed via e-mail. Is that possible?

Best wishes, Tom Bourbon

Date: Fri Dec 11, 1992 10:47 am PST

Subject: help and bio

[len lansky 921211 1143]]   a little bio and request for help.

I am a social psychologist on faculty at University of Cincinnati where I have been for past 30 --count 'em-- years. Am still a stranger in town. Have many interests in the field: early work was in personality theory/measurement and developmental, especially sex roles and defense mechanisms; then worked on educational issues--participative learning with new curricula of post sputnik days; was in NTL, have done sensitivity training, od (and have written about them as well as other items above and below); some work on creativity in architects (with architect John Peterson); and then work on handedness (also with Peterson)- -might send questionnaire to persons on net some day--am curious about handedess and demography; and more recently body image-- back to sex roles and measurement, eating disorders, etc. Maintain strong interest in teaching, systems as well.

Phil Runkel and I are friends from grad school days(I have chapter in his book on college teaching; teach from his book with McGrath on methods, have done od with him)) so I read, with great joy and interest, his "Casting Nets..." which got me interested in control theory. I wrote him via email, he called me back and then I joined the net last year, have "joined the group"-- incidentally have not got most recent copy of "Closed Loop" although I did send in my annual dues--and that brings me to the next topic.

Help!!!! I have agreed to teach a seminar on control theory next quarter--begins in January--for ten weeks. I have the books and have been trying to understand and to classify the traffic on the net. My own understanding is still very limited; thus I plan to learn along with the other participants--probably 3-10 graduate students in social, clinical, maybe experimental psychology and maybe some from elsewhere--we have sent out some publicity.

BUT I have no sense of a sequence of readings or activities. Has anyone worked out such, other than the intro text which seems too elementary for the group I am working with? I plan to xerox or copy via computer the introductory statements by Dag. But where do I go from there? Does anyone have a worked out sequence for presenting the theory. I am also trying to figure out how to get copies of the various documents in the listing that Dag has done to introduce the field. I will solve that one. Since we do have access to computers, I plan to have the students sign onto the CSG net to bug some of you on one or another topics--indeed that might be a feasible assignment to enhance that student's learning and those of us s/he then tells about it--and all of us would have seen the traffic too. But I do not have any feel for a sequence.

All offerings will be gratefully accepted. I suspect a start might be with a demonstration or two. I do not know enough, as of this writing, to ask more intelligent questions, but hope to be able to do so by early next week. You will be hearing from me. And I thank you all in advance.

If you want to send me hard copy, address is

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direct to me on computer is Len.Lansky@uc.edu

Thanks again.

Len Lansky

Date: Fri Dec 11, 1992 11:34 am PST
Subject: Re: Science vs Religion

From Tom Bourbon (921211 11:28)
Oded Maler (921210 - according to "their" calendar)

* And an enlightened PCTer would see that he is a blind behaviorist/cognitivist ...

Of course! And then the PCTer would realize there are no "real" PCTers, behaviorists, cognitivists ... not in Boss Reality.

Felicitationes, Tom Bourbon

Date: Fri Dec 11, 1992 1:30 pm PST
Subject: Perception

[from Gary Cziko 921211.1544 GMT]

Bill Powers (921210.1900), thanks so much for explaining how:

>We can set up a model
>environment in which there are several variables -- "v's." The input
>function can produce a perceptual signal that is any weighted sum of
>these variables, arbitrarily weighted. Then we can set up the output so
>that it affects these variables either positively or negatively as
>required for negative feedback, but all by the same amount, proportional
>to the integral of the error signal. This control system will proceed to
>control its perceptual signal just as if there were a single external
>variable corresponding to it. If any or all of the v's are disturbed,
>singly or in any combinations, the perceptual signal will be maintained
>close to the given reference signal by an appropriate change in the
>output effects on all the variables. . . .

>So this control system experiences an entity in the environment that
>does not in fact exist there. It is creating the entity. Nevertheless,
>the behavior of the environment is lawfully and reliably related to the
>perceptual signal, as long as the form of the input function remains the
>same.

Bill, I can't believe that this was staring me in the face all the time and I never realized it. Have you made this point explicitly before? If so, how did I miss it?

This will go a long way to help me start sorting out my problems in understanding how perception works, particularly concerning its bottom-up vs. top-down nature and the issue of whether perceptions are "transmitted" from the environment to us via our sense organs or "constructed" deep in the brain (I think the answer is both "neither" and "both").

Rick (Marken), do you remember how at the Indiana PA meeting in 1990 you were wondering how Ed Ford could say that we create our perceptual world? I believed you grabbed a chair

and made comments about how you couldn't arbitrarily create or "uncreate" its perception. Isn't Bill's comment directly related to this issue.--Gary

Date: Fri Dec 11, 1992 1:57 pm PST
Subject: Science & Religion

[From Rick Marken (921211.1000)]

Oded Maler (921212) ^^ Really?

>Why, for instance, is it important at all to have a theory of human behavior?

The right question! Asking it implies that this goal exists to satisfy some higher order goal. Once you can see the goal "have a theory of human behavior" FROM THE PERSPECTIVE of the system that has that higher order goal, then you can see that it is just one of many different goals that you might have selected to achieve it -- even if you never actually realize what that higher order goal is. That's what I mean by arbitrary -- just knowing that there are options. From the point of view of the system controlling for the theory itself, the goal "have a theory of human behavior" is not arbitrary; that's all the system cares about. Same with more personal goals; eg. from the point of view of the system that has the goal "be a christian" is not an arbitrary goal-- it MUST be achieved; it is the only "right" thing to do. You have to go up a level to see that this is just one of many goals that might have been selected. That still doesn't mean that you can go and arbitrarily select goals -- just as you cannot arbitrarily select the force that is used to produce the perception "lifted suitcase"; it depends on how much the suitcase weights. "Be a christian" might be the only goal that works in the context of one's existing hierarchy of goals and currect circumstances to satisfy the higher order goal. But, if you can "go up a level" you can at least see that "be a christian" is something you select -- not really something that "must be true". As Bill P. says, its a matter of changing the "point of view" of one's consciousness. Read his chapter about it in "Living Control Systems II". This "point of view" change can be done (at least, I can do it -- though not always easily). And its worth the experience -- trust me, I'm a doctor.

Ed Ford (121210) --

> "To those who believe, no explanation is necessary; to
> those who don't, no explanation is possible."

Apparently, that's true. What I want to understand is why this is true. I want an explanation of believing itself, whatever the beliefs themselves might be. By the way, Hitler believed that it was a great idea to kill every Jew, communist and homosexual -- and, indeed, no explanation was necessary. Many of the people that Hitler killed believed in a God who considered them special and "chosen" -- and, indeed, for them, no explanation is now possible.

>It depends on whether, for a particular individual, his/her
>religion presents a conflict.

I didn't mean that religion is a problem because it creates intrapersonal conflict. I'm sure most devout people are quite unconflicted about their religious beliefs. The problem with religion (and other high level goals of the same sort that become fixed -- ethnicities, nationalities, etc) is INTERPERSONAL CONFLICT. I don't know if you've looked at your paper lately but mine is FILLED with violent, interpersonal conflicts over religions (india, balkans, mid-east) nationality (mid east, masadonia, balkans),

ethnicity (somalia), etc. So people are fighting their brains out to defend perfectly arbitrary goals; I consider this a problem -- and one that is so unnecessary that it is unbelievable. And the solution, of course, is for each person to be able to see that their own ethnic, religious, national, etc goals, though important to themselves, are perfectly arbitrary; that it's like arguing over whether cars should be driven on the left or right.

Geeez.

I said

>>When you get up there you will see that chosing a religion,
>>ethnicity, nationality, etc is just as useful (and arbitrary) as
>>chosing a nice book to settle in with on a rainy day; sometimes
>>you want a romance and sometimes only a thriller will do.

Ed says:

>Again, "you will see" refers to what you perceive not what everyone
>will perceive "when you get up there."

I'm seeing of you can go up a level -- not change your religious goals. Try this. Imagine that you were born Ed Greenberg instead of Ed Ford into an Orthodox Jewish family in flatbush. You have a loving family and warm relations with your friends. Really try to imagine it.

Now, do you think you would be sitting here now, convinced that Jesus Christ is God become Man (leaving aside, for the moment, the question of whether or not he is). Since you say:

>My own particular religion is based on fact, not fiction. It is also
>based on 50 years of thought, study, research, and lots of reading.

I have to wonder whether your religious conclusions would have been that same if you had done this study and research as Ed Greenberg.

>More importantly, the CSGnet is NOT the time nor place to discuss such matters.

I disagree. I sense resistence to disturbance here. But you can just avoid reading the religious posts. To me, religion is (as I said before) just something that people do -- like being a control theorist. PCT is trying to understand ALL of human behavior -- and religion is certainly one of the most important (and troublesome) things that people do. I think it should not only NOT be off limits for the net -- it should be something we in PCT try desperately to understand.

Best Rick

Date: Fri Dec 11, 1992 2:40 pm PST
Subject: Re: "Models and their worlds..."

> When
> Greg Willliams finishes with it, there will be figures included --
> plots of data from people and simulations. I don't know of a way
> to include those in files distributed via e-mail. Is that

> possible?

Well, more or less. I publish my technical papers with simple diagrams in the LaTeX language, all ASCII. Postscript can be emailed, and anything (e.g. WordPerfect w/embedded GIF files, or whatever graphics WP uses) can be ASCII encoded and stored and emailed. There are lots of other ad-hoc methods out there, but as yet no elegant, complete solutions.

Cliff Joslyn,

Date: Fri Dec 11, 1992 4:01 pm PST
Subject: Re: My First PCT Publication Out

[From Dick Robertson] (12.11.92)
Gary, congratulations on seeing your PCT article in print. I've just finished using it in a History of Psychology course as the last example of what Kuhn called a paradigm revolution.

All the best, Dick

Date: Fri Dec 11, 1992 5:51 pm PST
Subject: Re: Do you believe in purpose?

(ps 921211) [From Rick Marken (921210.2200)]

> (ps 921210.1400)
> >(btw, i don't really believe in Purpose, either; i was stipulating it
> >for the sake of this discussion.)
>
> It's pretty late for me but this comment woke me up.

i'm flattered you read me so closely ;-}.
> In PCT, "purpose" is virtually synonymous with "control". Actually,
> the common sense of the word "purpose", as in "behaving with a purpose"
> or "his purpose was to" is synonymous with PCT's "reference level of
> a controlled variable".

i think this ``virtual synonymy'' is a rhetorical move of pct's that irritates people w/ a scientific bent (like me). i'm quite willing to accept yr control models of concrete and reasonably well-understood phenomena. i find yr generalization to everything else, including the kinds of things implied by ``purpose,'' completely unwarranted. so i wish you'd stick to making claims you can justify....

cheers. --penni

Date: Fri Dec 11, 1992 5:54 pm PST
Subject: conceptual and perceptual EVs

[from Wayne Hershberger] Rick and Bill:

(Bill Powers 921210) You (Rick Marken 921209) made a good point to Wayne: "What's wrong with guessing that there is an

> EV that is distinct and causally a priori to the process of
> perception if the assumption works?"

What is wrong is supposing that the conceptual EVs are not part of the model of the perceptual process! The light-ray tracings in Bill's little stick man simulation comprise an integral part of what works! And, if this screen were not luminous you wouldn't be perceiving this.

This is not to say that our perceptual EVs (what you refer to as the things perceived to be "out there") need correspond to the conceptual EVs in our scientific models, but only that the perceptual EVs and the conceptual EVs are lawfully related to each other (I gather we agree fully on this point). That relationship is the basis of empiricism. It is this lawful relationship that allows us to use our perceptual EVs to test our conceptual EVs in scientific experiments. Science does not address the question of whether our conceptual EVs (making sense of the immanent order) correspond to those of some divine conceiver (God or Boss). Not only does the immanent order have many more degrees of freedom than we can perceive, it may have more degrees of freedom than we can conceive.

Mark Olson (re: 921210)

If you are asking whether we control subjective or objective variables, I would say both: perceptual EVs, considered immediately, are subjective, but they are also, potentially, objective. Further, if the control of an objective physical variable (i.e., a conceptual EV) by the simultaneous control of two subjective variables involves conflict between the latter subsystems, the subsystem controlling the "more objective" subjective variable will tend to dominate and/or reorganize the other one.

If you are looking at something through a wedge prism, your gaze will be directed toward a displaced virtual image of the object. And if you reach out slowly to touch it, you will start reaching out toward the virtual location, but only until you can see your hand--thereafter you will begin to align the retinal images of your hand and the object like Bill's little stick-man. Further, if we do this repeatedly, the "error" during the initial part of the reach will diminish--and when the wedge prisms are removed a negative aftereffect is observed.

Warm regards, Wayne

Date: Sat Dec 12, 1992 12:42 am PST
Subject: Re: neuropsych and PCT

From: Tom Bourbon (921212 02:13 CST)
Concerning Nerves, Brains and PCT

The continuing discussion on "controlling error" spawned a subtheme on the idea that PCTers should pay more attention to neurophysiology, neuroscience and related areas.

Mark Olson (921129 19:06) posted on "neuropsych and PCT"

>TO Bill or anyone,

>... I have noticed a few things which bother me and maybe you can
>iron out or explain. My basic observation/complaint is that we
>ought to be doing some more bottom-up thinking in terms of what

>the neurophysiological data suggest. I say this because it seems
>to me that we have Sensation (color) before Configuration
>followed by Transition (motion) when the neuropsych data suggest
>that color and motion are processed before "objects" are
>discerned (in area TE, or IT).

>It seems that some levels are concerned with temporal
>relationships while others are concerned with spatial
>relationships. Some levels (Sequence) seem devoted to procedural
>tasks while others seem devoted to declarative tasks (Category).

Through your readings, you have (inevitably?) picked up the style used most often in the neuropsych-neuroscience literature. Writers employ language that supports certain impressions and inferences in readers, but says nothing definite or meaningful. One result is that innocent readers believe more is known about brain structure and function than is the case. I am not attacking you, Mark, just stating that words and styles in the neuroscience literature can be tricky. You have probably seen the phrase "the neuro... data suggest" countless times. But data do not suggest. People look at data and people decide or believe that the data mean something -- people suggest that data mean certain things. I am not trying to be picky, but honest writers in the neurophysiology literature -- and most of them are -- are telling you that they don't really know -- the data merely suggest. If the data are no better than that, then they can also be seen to "suggest" other conclusions to other people. That is usually what happens.

When applied to brains, words like "processed", "discerned", "concerned", and "devoted" have no specific referents. Researchers see that activity differs across locations or times or some combination of locations and times, and they read into the data the language of communication theory and computer science. Or they attribute to cells and "regions" in the brain the achievements and the attributes of the entire person, or of the species or the culture. That is common practice, but it is not good science.

> ... We should take what we know about brain processing into
>consideration. I agree that we do not want to just go from brain
>data to theory--both are needed. But now it seems that the
>hierarchy does not jive with neuropsych data. For instance, it
>doesn't seem to me that processing in the parietal lobe (telling
>us WHERE the "object" is) is in any sort of hierarchical
>relationship with processing in the temporal lobe (telling is
>WHAT the "object" is).

>How, for instance, is the Relationship level instantiated in the
>brain? If that is the level devoted to noting (temporal)
>relationships like causation and (spatial) relationships like
>above/below, left/right, etc, then does this level exist in
>multiple brain regions? Higher levels seem to reside in frontal
>regions.

Bill Powers (921130.0730) had some good things to say on those topics:

>Unfortunately, neurophysiology is too vague about most perceptions
>to do us much good. While the neurological observations are
>probably all right (identifying activity or lesions in various
>parts of the brain), the identification of the CORRELATES of these
>activities depends on informal subjective observation of the

>world. It isn't the "subjective" aspect I question, but the
>"informal" aspect.

I think by "informal" Bill means something like what I mean when I speak of neuroscientists' careless use of language. Bill's discussion a little farther along, about regions that allegedly process visual information and tell us where or what, goes directly to the issue of the incredible sloppiness in much of the neurophysiological literature.

Later, Bill wrote:

>In HPCT there are at least some principles involved in identifying
>correlates of brain activity. For B to be a higher level of
>perception than A, the existence of B must depend on the existence
>of A but not vice versa. When B is controlled, it must be
>controlled by varying A, but not vice versa. And it must be
>possible to control A without necessarily controlling any B, but
>not vice versa.

An extremely important point. The word "levels" can be applied to nervous systems in many ways, not all of them compatible or equivalent -- it can refer to "altitudes" or distances above or below an arbitrary reference point; to presumed complexity, abstraction or sophistication of "processing"; and so on. Bill went on with a nice discussion of this problem and of the categories of your own perceptions that you were imposing on brain function and architecture.

>But what we know about brain processing is based on how we
>organize our concepts of perceptions. All we know directly about
>brain processes is that different areas of the brain are active
>under difference circumstances. The whole problem is to
>characterize what is different about those different
>circumstances. So we have to organize our understanding of
>subjective perception before we can use that understanding to give
>meaning to activities in various parts of the brain. Without an
>understanding of how perceptions are related to each other, we
>can't understand what it means when different parts of the brain
>are active.

This topic cannot be emphasized too strongly. Taken alone, anatomical and physiological data have no psychological meaning. Meaning, importance, and significance flow FROM the facts of subjective experience TO anatomical and physiological data, not the other way around. For example, we do not perceive the world more clearly, or less so, as a consequence of neurophysiological data; but we "see" neurophysiological data in the light of, or as ways of explaining, perception. It has always been so.

The problem is even deeper. Most of the neurophysiological literature derives from research in classic cause-->effect designs: researchers apply stimuli and observe responses, or if they are up to date, they administer inputs and observe outputs. If you believe organisms, or their parts, are C-->E systems, you will study them as though they are and you will interpret your data as though they are. But what if they aren't? What if they exercise control and you don't even consider that possibility, let alone test for it? In that case, you will repeat most of the failures in the neurophysiological literature.

Even at the level of a putatively simple reflex, where the model of C-->E seems safest -- where S-->R with certainty and regularity, or so it seems, appearances can be deceiving. Virtually all such reflexes are examples of control -- the alleged reflexive response just happens to eliminate the effect a disturbance, mistakenly identified as an independent stimulus, has on a controlled variable -- the existence of which is not dreamed of by the researcher. There are exceptions to this portrayal of neurophysiology-neuroscience, but not many.

From the apparently simple level of reflexes, on through the allegedly most sophisticated and complex neurocognitive feats, the story is the same: Researchers treat the living systems with which they work as though they were input-output devices. The resulting data have little to do with what organisms do and how they do it, therefore, when we use them in our attempts to understand brains, they (the data) cast more darkness than they shed light. I wish that were not the case, but it is -- no matter how impressive the technology for producing lesions, for imaging, or for creating colorful cartoons of changing patterns of activity, research in which organisms are treated as though they were what they are not tells us very little about how brains figure in the bigger picture.

And that is why I disagree with Mark's sense of urgency when he posts:

Mark Olson (921211 10:35:57)

>Concerning Gary's post about the man who couldn't reach for an
>object, I think this is a perfect example of how PCT'ers should
>give more consideration to where these various control processes
>occur in the brain. The superior parietal region is evidently
>involved in representing external space (body centered) and
>prefrontal areas seem to be involved in tasks which sound very
>much like tasks at the Sequence and Program levels.
>I think Bill did a great job of constructing these levels in the
>manner that he did, but the hierarchy is not going to improve
>much anymore by thinking about it--we should look at the brain.

Mark, you will need to go beyond Gary's example, and beyond the literature on "involvement" of brain areas, before I see that PCT is in danger if we do not "look at the brain." My reading of the present scene is exactly the opposite: Without insights into the organization of perception and behavior like those offered by PCT, much of (cognitive) neuroscience is seriously at risk.

Until later,

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Date: Sat Dec 12, 1992 3:59 am PST
Subject: Many subjects

[From Bill Powers (921212.0300)]

Might as well get the mail and write about it. What else can you
do when you're awakened at such an hour to be told you have a new
grandson (my daughter Barbara's second)?

Len Lansky (921211.1143) --

You raise an excellent set of questions. At a more propitious time I will start putting together an outline for a one-quarter (13-week) course in PCT. Unless someone else beats me to it. I envision a group project here, in which people rewrite the outline and re-rewrite it until we have something that looks good.

Rick Marken(921211) and Ed Ford (921210) --

RE: Science and religion

It will not be possible for science and religion to get together until both realize that neither is Revealed Truth, and that both are human ideas. Of course that is precisely what both sides have been rejecting since the start of science. One side points the finger at Nature, the other at God. Neither side, apparently, notices whose finger is doing the pointing.

Gary Cziko (921210.1622) --

The Little Man starts out (in the mapped mode) with a "bilateral lesion" that makes it consistently point short of (and to one side of) distant targets. As it builds up the map it gets closer and closer until it points exactly to the target on the first try. I suppose you could put a "lesion" in the map that selectively eliminates the correction in any dimension -- horizontal, vertical, or depth.

Bruce Nevin (921210.1206) --

Come to think of it, language IS like grooming. Sometimes you pick up something nice and salty, but sometimes it wiggles and squirms until you crunch down on it.

Oded Maler (921212) --
RE: why is a theory of behavior necessary?

Why is it necessary to have a theory of gravitation, or electronics, or numbers, or anything else? I suppose "necessary" is the wrong word here -- after all, other species have got along fine without any such theories. One can certainly jump and fall down without a theory of gravity, and behave without a theory of behavior, or even with a wrong theory of behavior. But theories in general seem to make sense of things in a way we can't achieve by any other means.

Or were you specifically asking why we need a theory of behavior, in addition to all the other theories?

Penni Sibun (921211) --

RE: Not "believing in" purpose.

I was about to respond in a way almost identical to Rick's, by saying that purpose is the essence of control. But you then went on:

>i think this ``virtual synonymy'' is a rhetorical move of pct's

>that irritates people w/ a scientific bent (like me).

If you think that the PCT interpretation of the meaning of purpose, or intention, or desiring, or wishing is a "rhetorical move" then you simply haven't caught on to the basic architecture of control.

>i find yr generalization to everything else, including the
>kinds of things implied by ``purpose,'' completely unwarranted.
>so i wish you'd stick to making claims you can justify....

What kinds of things do you think are implied by "purpose?" The PCT claim is that purpose is a perfectly real phenomenon, explainable by a proper model of control behavior. Purposive behavior is behavior that results in bringing some aspect of the environment or oneself or the relationship between them (as perceived) to a predetermined state, and maintaining it there for some time despite disturbances that tend to change it. This is the phenomenon that science mistakenly rejected in the early decades of this century, claiming that it required the future to affect the present and a number of other foolish things. The problem really was that science, at that time, lacked any explanation for purposive behavior, and at the same time assumed that what science could not explain (in, for instance, 1920) was mysticism, superstition, or illusion. That opinion prevails today, especially among scientists who still don't understand how control theory explains purpose. There are always scientists who think that what they can't explain in terms of the science of their time (the part they know about) can't be explained at all.

Since you're the main person around here with a scientific bent, how about giving us the really scientific explanation of how a person carries out a purpose?

Wayne Hershberger (921211) --

>What is wrong is supposing that the conceptual EVs are not part
>of the model of the perceptual process! The light-ray tracings
>in Bill's little stick man simulation comprise an integral part
>of what works!

There are others who would argue on your side -- some cyberneticists, in fact, claim that not only are the light-ray tracings part of the model, but that the outputs that alter the light-ray tracings are part of it, too. Basically they argue that any separation of the organism from its environment is a conceptual mistake. It's all just one big system, so there's no point in trying to take it apart into components in order to understand it. You can only understand, like, the WHOLE THING.

I for one have never found that contemplating the WHOLE THING leads to anything but bafflement. I think that many people confuse this sense of bafflement with a sense of knowing.

The light-rays are not only part of the perceptual processes, they are part of the output processes. We could just as well say that the actions of the Little Man consist of altering retinal images, or to go further that they consist of alterations in neural signals in the brain. In other words, it's all output, not perception. When you trace out the signal paths, you find that what we mistakenly call perceptual signals are really just part of the whole process of output.

When you go far enough with this, you must finally decide that the outputs really consist of outputs, because when you start with the motor output forces and keep adding all the things directly related to and dependent on them, you end up back with the output forces

after one trip around the loop. Making all the substitutions to eliminate intermediate terms, you end up with output = f(output). Or perception = f(perception), or error = f(error). It all depends on where you start.

In our modeling efforts, we have found it definitely useful to distinguish the organism from its environment. When we do so, we find that the organism can know only what its senses tell it. And we find that what its senses tell it about the environment depends critically on how the sensory processes are organized. Two organisms in the same environment experience two different environments. Yet the mere fact that we can say "the same environment" means that we suppose that something does exist there independently of the perceiver. We can't prove that, but we can build models on the assumption that it's true.

>... perceptual EVs and the conceptual EVs are lawfully related
>to each other (I gather we agree fully on this point).

Yes, but I doubt that you will agree with my agreement. I consider conceptual EVs to be higher-level perceptions of lower- level perceptual EVs, and that the lower-level perceptual EV's are in turn functions of variables in the Boss Reality. The higher-level "conceptual" EVs consist of such things as relationships, categories, sequences, logical and syntactical propositions, principles, and system concepts. Each successive level consists of perceptions that are functions of perceptions of lower level. The "lawful relationship" of which you speak is simply the relationship in which higher-level perceptions depend on lower ones according to the form of the higher-level perceptual function. If you look at the content of any "conception," you will find elements that correspond to the levels I propose -- and, I claim, nothing else. If you do find something else there, please let me know.

I disagree, therefore, with the distinction between perception and conception as being too crude. I use the term "perception" to mean any afferent process at any level -- that is, anything we can experience. In place of the simple dichotomy perception- conception I substitute specific levels of apprehension of the world, and make the claim that these levels are hierarchically dependent. The general achitecture of the brain, and the effect of lesions and ablations in various parts of the brain, supports the view that the "conceptual" levels have access to the world only through lower levels of input processing. The highest levels have no direct access to the world -- there is no uninterrupted input path from the sensory organs.

>It is this lawful relationship that allows us to use our
>perceptual EVs to test our conceptual EVs in scientific
>experiments. Science does not address the question of whether
>our conceptual EVs (making sense of the immanent order)
>correspond to those of some divine conceiver (God or Boss).

You are fighting a straw man here. To hypothesize that there is a Boss Reality is not the same as saying that there is a Divine Conceiver. I'm surprised that you keep bringing this up -- surely you don't think that this is my explanation of the Boss Reality! You seem to be ruling out the possibility that there could simply be a universe that is independent of our experiences but on which our experiences depend. Or you seem to be saying that the only way such a universe could exist would be in the mind of a supernatural being.

This truly puzzles me. I don't see how you arrive at just those possibilities. The only possible clue I can get is in this:

>That relationship [between concept and percept] is the basis of empiricism.

and this:

>Not only does the immanent order have many more degrees of
>freedom than we can perceive, it may have more degrees of
>freedom than we can conceive.

It seems clear to me that what you are calling the "immanent order" is what I call the "Boss Reality." Models are a way of conjecturing about what might exist in the immanent order that we have not yet perceived. We imagine, in other words. We provide for ourselves lower-level perceptions synthesized from memory, and view them from higher levels as if they exist in real time. We categorize and characterize them, we reason about them, we form principles on their basis, we fit them into a systematic concept of the world. And we test them: we create outputs calculated to produce certain perceptions after passage through that external world as we model it, and check to see if the result is what we expected.

Of course we must always cast these conjectures in the form of human perceptions. We have no other way of experiencing the boss reality.

Perhaps all of this comes down to the same concept that you call "empiricism." But I think you may have more faith in empiricism than I do, as a source of true knowledge.

Best to all, Bill P.

Date: Sat Dec 12, 1992 11:04 am PST
Subject: Sciligion?

From Greg Williams (921212) Bill Powers (921212.0300)

>Might as well get the mail and write about it. What else can you
>do when you're awakened at such an hour to be told you have a new
>grandson (my daughter Barbara's second)?

Excellent! Sounds like a wonderful holiday present (namely, you, Mary, Ed, Barbara, and other family members as a present to the new arrival)!!

>RE: Science and religion

>It will not be possible for science and religion to get together
>until both realize that neither is Revealed Truth, and that both
>are human ideas. Of course that is precisely what both sides have
>been rejecting since the start of science. One side points the
>finger at Nature, the other at God. Neither side, apparently,
>notices whose finger is doing the pointing.

Well said. I'd like to add one additional observation regarding the possibility of the "sides" getting together. In several forms of religion, and some (at least historical) forms of science, accepting authority and having faith have been/are now valued more (sometimes MUCH more) than adjusting beliefs in the light of new evidence. Modern science at least gives lip service to the idea that one's OWN finger should be doing the pointing, unencumbered by pleas or threats from others. But that is anathema to some modern religions. One reason that a discussion of "science vs. religion" is appropriate

on the net, in my opinion, is that the issue of self- vs. other- determination is right at the heart of what control theory has to say about the chances of an individual successfully coping in a disturbance-filled world. On the other hand, high-level reference signals (within a broad spectrum) appear to be very loosely coupled to day-to-day survival (assuming you aren't in a holy war, of course), so I don't feel much missionary zeal for going around begging folks to recant what they accept on authority. And if I did, I wouldn't rail against the beliefs themselves so much as against why they are held. As a general principle (based on PCT ideas), it would appear that breaking correcting loops (e.g., accepting dogma uncritically) is dysfunctional. Yet people do it all the time and seem none the worse for it. Of course, their neighbors might be MUCH worse for it!

May your neighbors not be EXTREMELY dogmatic this holiday season,

Greg

Date: Sat Dec 12, 1992 11:59 am PST
Subject: Misc Replies

[From Rick Marken (921212.1100)] Penni Sibum (921211)--

>i think this ``virtual synonymy'' is a rhetorical move of pct's that
>irritates people w/ a scientific bent (like me).

Rhetorical move or not, PCT definitiely does irritate people (especially psychologists and cognitive scientists) with a scientific bent.

> i find yr generalization to everything else, including the
>kinds of things implied by ``purpose,'' completely unwarranted. so i
>wish you'd stick to making claims you can justify....

You could help us out enormously by pointing out the unwarranted things implied by "purpose" that we generalize to in PCT. This would be especially helpful coming from someone with a scientific bent. Once we knew what we shouldn't say about purpose, then we might be able to stop irritating those with a scientific bent -- and who wouldn't want to stop doing that? ;-)

Bill Powers (921212.0300) --

>Might as well get the mail and write about it. What else can you
>do when you're awakened at such an hour to be told you have a new
>grandson (my daughter Barbara's second)?

Mazel tov.

>RE: Science and religion

>It will not be possible for science and religion to get together
>until both realize that neither is Revealed Truth, and that both
>are human ideas. Of course that is precisely what both sides have
>been rejecting since the start of science. One side points the
>finger at Nature, the other at God. Neither side, apparently,
>notices whose finger is doing the pointing.

I partly disagree with this. I think there are many scientists (the good ones) who understand that their models are human ideas and that "nature" -- the cauldron in which these ideas are tested -- is just their own perception. I think there are also religionists who understand that religious models (myths) are human ideas and that "spiritual experience" -- the cauldron in which these ideas are tested -- is just their own perception (human experience). I would venture to guess that there are far more scientists like the above than there are religionists. And I would argue that the reason for this is that implicit (or explicit) in most religions is the idea that you MUST believe that these ideas are Revealed Truth or else you , your people or the human race are in deep doo doo. I don't think this latter assumption is part of science -- although I agree that many scientists act AS THOUGH such an idea were part of the game; and that is where science and religion become one -- as you say, when their ideas (models, myths) are treated as revealed truth rather than human invention -- invented for a PURPOSE (oops, I think I heard a gasp from another person with a scientific bent).

[While reviewing this I saw Greg Williams post which basically said exactly the same thing -- science at least pays lip service to the idea that its ideas are not Revealed Truth while few religions do this -- and, of course, I heartily agree with him (and, by implication, with myself)]

Gary Cziko (921211.1544 GMT)

>Rick (Marken), do you remember how at the Indiana PA meeting in 1990 you
>were wondering how Ed Ford could say that we create our perceptual world?
>I believed you grabbed a chair and made comments about how you couldn't
>arbitrarily create or "uncreate" its perception. Isn't Bill's comment
>directly related to this issue.--Gary

I didn't mean to say that perceptions are not "creations" of perceptual functions in the nervous system. I was suggesting that one cannot willfully and arbitrarily change the functions so that you see the world in a different way. I think these functions can be (and are) changed by reorganization --and this process may be willful sometimes; actually, I can think of instances where it seems that it is. Learning to do surgery, for example, might involve learning that what was perceived as a diffuse mass of gunk is actually distinct sets of arteries and nerves -- some of which should be cut, others not. So I'm prepared to admit that I may have misspoke -- in part. Another thing I was thinking of was the fact that you cannot arbitrarily change a perception and expect to maintain control of the perceptual variable. I think of it in terms of the spreadsheet model (of course). Suppose that system 1, level 2 is controlling a perception p = x+ y (perceptions at this level are just linear combinations of lower level perceptions). I think of an arbitrary change as one in which the perception becomes something like p = x-y. Now the perception, p, corresponds to a whole new function of lower level experience; its as though the combination of sound intensities that coresponded to the perception of a minor third now sound like a major fifth. Besides the puzzling experiential result, such an arbitrary change in a perceptual function would very likely lead to conflict between control systems -- espectially if another system at the same level of the hierarchy had already been controlling x-y. So that is the phenomenon I was thinking of when I said one could not arbitrarily change perceptions; I really meant that if we really could will such changes (and maybe we can), doing so would (I think --from my experience doing this with the model) be most likely to produce control problems. Of course, if you already have a control problem (conflict) then changing perception ("creating a new perception from the same raw material") might be one reasonable possibility -- and I think this is what does happen -- in therapy, education and everyday problem solving; it's just one aspect of reorganization.

Best regards Rick

Date: Sat Dec 12, 1992 12:03 pm PST
Subject: A Long Post

[From Gabriel 921212 12:44CST]

This is a long post, being the concatenation of three in an offline correspondence. I and Bill C. have noted Gary's concern about offline discussion depriving net mbrs of necessary context. We'd like to share our whole discussion, but it is complicated by being in part telephone conversations, in part subject to non-disclosure requirements, and part very technical and full of alphabet soup.

Some background on the participants. One group is the (by now) gang of five interested in data-fusion/information-fusion, why the two are so often confused, and uses in better Government and Defense. All of the gang of five work in more or less defense related institutions, and are to varying extents "thinking the unthinkable" so as to keep it from happening.

There is also a "gang of three", less active in the discussion, one being also a member of the gang of five. The gang of three is mainly interested in making better Management Infomation Systems organisations, because the present breed work with great expense, low speed, and terrible cost effectiveness.

The thing we all have in common with PCT is feedback paths, the information that moves along them and the way people behave when the information is perceived. The gang of five is concerned with military matters mainly because there is more experimental data and recorded detailed history, so that we can test theories best in the military context. The long term agenda is beating swords into plowshares, even though we still each keep a sword hung by the door just in case. We feel there is compelling evidence that CEOs and military commanders have rather largely the same problems except that being laid off is preferable to being killed or badly wounded. But each is a likely consequence of utter defeat of your unit.

The gang of three are concerned with people and information. There is no need to cut and weld pipe, or to shoot guns. This makes all the theory simpler. Our experimental evidence is about a quarter century of collective experience in the software business. This has advantages and disadvantges. Having grown up with the industry we can see the evidence of evolution in the embryology. But three is a small sample.

[Gabriel to Bill Cunningham at some ungodly hour of the morning]

This continues our afternoon TELCON about the problem of search/alert modes in PCT, and also scratches my itch about discovery being a collective phenomenon of populations of ECSs.

For those readers who did not share the earlier discussion, Bill and I have been discussing the question of how a single control system can ever "discover" anything, as distinct from "tracking" something already perceived, and whether this is a fundamentally absent issue in PCT, so we need to add it to the theory of advice to Cdrs.

I think the following suggests an interesting line of thought.

An N'th order control system is exactly mimiced by and exactly mimics, i.e. it is precisely represented by a set of N coupled first order ordinary differential equations (ODEs) - Bill Gear's lectures 101. (By the way, Bill is now directing a research consortium in Tokyo, funded by the Japanese Govt.)

A set of N coupled first order ODEs for the kind of N we usually can think about - say N <= 50 has nothing to do with algorithms to examine search spaces, that is to say, PCT seems to have little to tell us intuitively about discovery unless there is some unknown mathematics, which seems unlikely. N~10**4, N~10**7, or N~10**9 are far enough outside ordianry experience to make an intuitive argument straight out of PCT unlikely to carry any weight.

How to make a connection?? Well, if you uncouple the ODE's a bit, you can make them essentially all the same (good neuroanatomy), and have each one generate a path through state space determined by initial conditions. Different conditions, different paths.

Now, suppose we take a leap of faith, and guess that a collective set of ECSs (i.e. first order ODEs) are given a set of different initial conditions such that for any point P in the search space there exists at least one path passing within distance epsilon of P, and we can make epsilon as small as we want by taking enough ECSs (concentrating enough attention on the problem).

If as an ECS traverses a path, it notes the minimum value of an objective function on that path, a vote at the end of the collective behaviour can tell us which set of initial conditions led to the closest approach to the global minimum of the objective function in the search space defined by the set of all sets of initial conditions tried, one for each ECS in the ECS's assigned to the task of discovery by focussing attention. This vote is good neuroanatomy - it's done by a voting tree, and is not very far from the Kanerva neuroanatomical model for memory.

This model also explains some of Joe's and Bill's observations.

If you don't have a good set of Bayesian priors (initial conditions) Joe's observation that you can't cope with soemthing new on short notice follows, because you have not built the setup to explore that part of the search space.

The thing about false lock, only local minima, and misguided prosecution at law, is a case where you made a first pass through the space, decided that you could abandon a large part of the initial condition sets you tried and concentrate on the promising ones, which turned out not to go anywhere near the global minimum. This is one Bill and I have debated, ever since I raised the case of the possible terrorist found through a pistol permit. Perhaps we have a resolution.

Well, that's my 4AM insight for today. Try it on Stark/Vincennes, and the other litany of INTEL failures. Note by the way that it's a devil and deep sea problem. If you don't focus in on the problem you don't have the resources to solve it. If you focus in too early you have a high probability of being on the wrong track. Is this a type 1 vs type 2 error issue too. I have a strong feeling that the type1/type2 tradeoff in Sequential Statistics a la Abraham Wald is at the bottom of a lot of things we see.

This is the spot incidentally where the exceptionally able strike team of software developers always beats the Mongolian horde of average ones, and it's why most software these days is so very bad.

It also seems to me to bear on the distinction that Bill and Tom make between managers and leaders, and what Bill keeps repeating is different about a 4*.

John 921211 04:32 CST

Adopting CSGL date/time convention since this discussion likely to expand.

:From Bill Cunningham 921211.0830 EST:

(John Gabriel 921211.0432 CST)

John, Ifind your suggestion "not implausible" and very attractive, on the surface. The idea is certainly convenient, but I haven't a clue whether it's just a convenient model or a DEEP explanation of what actually happens.

My first thought is "What do I tell a mathematical illiterate?" I have to do that sooner or later.

ANS: "Keep your options open as long as you possibly can, knowing that as soon as you exclude new ideas you are committed to one/few. Actively seek as many working hypotheses as possible. Use brainstorming. Be very slow to throw out old data."

I think Joe would say play ALL the data (especially negative) against ALL the hypotheses, and if selection of a promising minimum doesn't lead to satisfactory closure--reopen the search by generating new hypotheses.

I think an action oriented commander would argue that he didn't have time for that b------t, that most of the time available had to be spent planning the execution.

My response would be, remember your METT-T doctrine. Mission, enemy, tactics, terrain--and time available. Start with mission and time available and identify drop dead points when you MUST make a decision.

I'd remind of Stark/Vincennes and say make your drop dead decision to defend the ship if you can't find contrary evidence by t=tcrit. Put that order into motion and search like hell for the disproof. But don't fire til you see the whites of their eyes.

I also think I'd drag in the fratricide problem. If I expect no friendlies, my range of hypotheses is limited to one and my pucker factor reduces my search time accordingly. But if I expect an ambivalent situation and my mission is say hostage rescue, then I have to allow more decision time--even at the risk of getting shot at. I can only justify that if the mission warrants. So the critical search is one for those prior constraints that can't be changed. And that also restricts the search space.

Or I might cite yesterday's XXX report and say you have to ask the right question.Shaping the debate so it starts with wide range and narrows "efficiently/effectively" is the ART of command. I'll bet I can find that in Druzhinin & Kontorov.It certainly fits with my telephonic comments that the great commanders seem to be in a very wide search mode, and then focus like a hawk on some issue. They are bimodal to the max.

Along the same lines, how about the restraint of the Marines in Somalia? I'm referring to the incident where the blonde female ABC reporter found herself face down in the sand with a rifle in her back. The reporters had all their lights on the Marines, and a few

shots were fired. My first reaction, if illuminated and under fire, would be to eliminate the illuminator whilst my buddy sprayed the area to discourage any aimed fire that might occur until the lights went out. Those guys done good.

With respect to the strike team of software writers vs the horde, I'd comment that, intellect aside, the strike team is deliberately in a search mode and the horde is deliberately in track mode. Once you have a horde, the problem becomes one of work breakdown structure and you've already constrained the search space. Now, it certainly helps if the strike team is made up of associative thinkers with sufficiently different backgrounds to enrich the search space and sufficiently common background/ focus to communicate efficiently and place SOME constraint on the search. Rosabeth Moss Kantor (sp?) writes that innovative organizations have very good lateral communication whilst the stagnant ones are highly vertical.

Now the question for Martin. How does this square with your dual channel models? Particularly the fast association/slow detail?

Bill C

Subject: Reply to Bill C's note
Status: R

[From Gabriel 921211 11:12CST]
A wondrous bright light Bill, and nobody shoving an M16 in your back either. Let me respond mathematically, but without formality - I don't get to do that often which is why you shone such a great light on the subject. Usually it's not possible to set out the idea without the formalism. And I truly enjoy talking about my real research to somebody other than myself.

The reason why my 04:30 note is appealing but not proven is that it's an imagined neuroanatomical implementation of the mathematical abstractions of search spaces, ordinary differential equations, optimal decision theory, and Hamiltonian system dynamics.

There are lots of other possible implementations, and one can only tell between them by experiment outside the realm of the common mathematical abstraction. That is to say, the mathematics has degrees of freedom which are lost once you go a physical system of any kind. I can implement the procedure in neuroanatomical wetware (perhaps), organisational wetware (certainly but I have serious constraints about who I choose to put in the organisation if I want it to be cost effective) silicon (at great capital expense, but practically zero cost of replication) software (same as silicon except easier to change when I find a mistake).

BUT since the mathematical abstraction has lots of properties we can observe to be approximately true outside the neuroanatomical black box, and actually observe in operations like YYY, it's useful independent of the details of wetware or software or silicon or organisations. Its usefulness lies in the observed fact that it's a detailed abstract implementation of the phenomena we observe, or a "model." It's predictive, it elicits strong recognition reflexes from those who know the physical implementations, and so on. That is to say, it has the properties of PCT. The basis for PCT in neuroanatomy is not bad, but certainly not conclusive for anything much bigger than the Moths and the Bats. The real justification for PCT is that Bill P. can build the Little Man and Little

Arm models in software, and they have lots of the properties of their analogues in wetware, but they are built from the piece parts of the PCT premise.

Now, there are some other universal abstractions that have been very useful, and it's worth giving them a passing glance because they are meta-meta.....meta theories. Hard to use, but very powerful. And they have to do with constricting degrees of freedom which is one of the things that interest us.

If you look at planetary dynamics for instance, you find there are some transformations of coordinates that don't change the equations of motion. For example those that arise because gravitation is a central force, and so, although orbits are not circular, one ellipse is as good an orbit as another of the same size but rotated in 3 space, and two ellipses with the same T**2/A**3 are both equally good. And that (r**2)*(d theta/dt) is the same at all points in the orbit.

These facts, observed by Kepler, can be made to yield some astonishing results. That acceleration is directed towards the sun, that it is inversely proportional to r**2, and that the constant of proportionality is the same for all the planets.

Now we make the great gedanken experiment. Introduce an imaginary new planet, and assume the same things are true. We can at once conclude the usual statements of Newton's Laws and Gravitation are true for this imaginary new planet, and so on .... BUT the conclusion depends on two things, the Bayesian priors, and the assumption that they are true for the new planet - which was OK until Einstein, and still good enough for Govt. work.

Now, when we do the same kind of gedanken experiment for the next level up in the hierarchy, we arrive at the idea of symmetry operators for the system, and the theorem that initial conditions and the symmetry operators alone determine an orbit.

This has a counterpart in psychology, it's Gestalt theory, and invariants, and although I still don't really know what reorganisation is, I suspect it has to do with throwing away some invariants, as distinct from simply changing initial conditions.

Now back to software, which can mimic any physical system, so it's potentially a useful abstraction too. If we have a program

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y = f(x)
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if it's going to be useful it had better yield reproducible results, so that for each input x, there is only one possible output y. That is to say, f is a many:1 mapping - several x may each give the same answer, but a particular x better not give different answers Mon Tue Wed, from Thur Fri Sat - on the seventh day f takes a rest.

This divides the set of all possible x into subsets, such that for every x in a subset, y=f(x) is the same.

You can see there are all kinds of symmetry lollygagging around - the "brotherhood" of all the x giving the same y is just a restriction of necessary varieties from the set of all x to the set of all y, i.e. a Ross Ashby necessary variety. If you leave a few brotherhoods out of your consideration of possible inputs, you have left out some y values, i.e. possible outcomes of running the dynamical system (campaign, TACWAR model....) represented by f().

Better get off my soapbox before it breaks. Merry Christmas to All.

This gets mathematical at about the same rate as the deterioration of enemy war capacity in Bill's example from strategic bombing, where B29s mined the Straits of Tsushima and damaged the Japanese prosecution of the war in the Pacific.

But the idea of symmetries, brotherhoods of scenarios all leading to (nearly enough) the same outcome, and Gestalt, and human perception of same, and search amongst them is very much our business.

John

PS I think I just did Ross Ashby wrong. The set of all brotherhoods is the necessary variety, and we need just one representative from each to get all the possible outcomes. But, if a brotherhood ain't really a brotherhood, i.e. we've put two different phenomena in the same class (f(x) can be a classifier) we may get an unpleasant surprise if the actual scenario we face is not the one belonging with the representative we chose for the "brotherhood". For the mathematician, there is less error in the abstraction than in the implementation - that's how I did Ross A. wrong, and why there are bugs in programs.

[Cunningham to Gabriel]

Ref my 0900 response to John's 0432 post

Any military commander will jump down your throat to tell you that a halfway right solution boldly and fully executed is far more likely to succeed that the best solution implemented 30 milliseconds later. This is so well ingrained that search beyond the the first local minimum isn't likely.

This takes us right back to the information campaign, and I suspect there is a commercial world counterpart. Given the fog and friction of war, the above approach is more likely to catch the adversary unable to perceive and respond to the action boldly taken. The friction problem places a premium on early decision. The guy who first perceives more or less the right situation and who acts immediately upon that perception wins every meeting engagement.

That also applies to predator/prey or breeding competition in nature. So good old Ma Nature fosters evolution of a two-channel fusion system. One is fast acting, finding approximate solution AND IMMEDIATELY ALERTING THE PROPER EFFECTORS/OVERRIDING ONGOING CONTROL. The other is the slower, more precise tracker. Rather obviously, you can't survive without both qualities-- and neither can our commander.

I guess that argues nature hasn't selected for processing negative information. Wonder why. I'll bet because the system is optimized for real time response to own sensors. We can certainly point to species alerted by negative info of "no birds singing==danger"

One more great argument for the information campaign is that we can process much more, and more quickly. A stated goal should be to extend search time to permit selection of a global minumum in time to execute fully. We're not actually extending the search time, but exploring more possibilities in less time. Now, that's going to take a change in commander's mindset!!!! And that's a training issue, once rest of system is in place.

Now, I feel more comfortably aligned with Martin's two channels.

Bill C.

Date: Sat Dec 12, 1992 3:56 pm PST
Subject: comments on arm2 docco

[Avery Andrews 921212.1010] (Bill Powers (921212.0300))

Congratulations!

3 comments on the arm2 documentation -

- I think it would be good to give some basic references for the neural wiring, especially the covariance of alpha & gamma reference signals.

- pg 9, mid, don't you mean `Fig 3 above is drawn ...'

- cogitating on the spinal control system, it's my impression that it is rigged to protect the muscle from damage due to extreme events caused by unpredicted variations in the external resistence to effort. Thus, if the (alpha-gamma) reference signal increases by a modest amount, this increase will call forth a modest change in position if there is no resistance, or a modest increase in tension if there is (effectively) infinite resistence. This presumably means that the muscle won't destroy itself, regardless of what idiotic commands come down from on hi. If this is more or less on the right track, it might be a good idea to put it in towards the beginning of the discussion of spinal cord systems.

Avery.Andrews@anu.edu.au

Date: Sat Dec 12, 1992 6:53 pm PST
Subject: "Blind men"'s rebuff, Another PCT Tech Report (Long!)

[From Rick Marken (921212.1400)]

Well, as I predicted, the "Blind men and the elephant" paper has been rejected by the scientifically inclined folks at Psycholoquy. So I can't even publish in an e-mail journal. It's pretty depressing; not one reviewer understood what the paper was about. Virtually every one thought it was a theoretical paper about control theory. It was not. Every reviewer thought the ideas were in one way or another "old hat" or too elementary. There were six reviewers; only one recommended publication.

I suppose I can either continue as a lunatic fringe PCTer or take a clue from these reviewers (whose opinions represent the majority of real scientific psycholgoists) and accept the fact that my work is horse dung (old horse dung at that) and that psychology has moved well past whatever PCT might have to contribute (I'll probably opt for lunatic fringe, of course).

I am attaching the reviews to this post. I know it's kind of a long document (27K) but probabaly no longer than one of Bill Powers' better efforts. For those who think that PCT is silly, old fashioned, unscientific garbage, these reviews should prove quite entertaining. For those who understand PCT and think it is the most important scientific insight of the 20th (or any other) century these reviews should prove quite entertaining.

Even though PCTers (well, at least one -- ME) are not allowed to have a dialog with the real psychologists on Psycholoquy, at least we can discuss these reviews amongst ourselves. Then maybe we can see what we (or they) are missing.

Greg -- Do you think we could put "Blind men" out as a PCT Tech Report? Along with "Models and their worlds"? Maybe this is a better idea than trying to publish a journal (of unpublishable results).

Here they are, with a prelude by S. Harnad.

Dear Rick,

Below are the 6 referee reports on your ms. "The Blind Men and the Elephant: Three Perspectives on the Phenomenon of Control."

The referees make some useful comments and suggestions for your future development of these ideas, but the reports unfortunately do not give me a basis for accepting your paper for publication in PSYCOLOQUY or for recommending revision and resubmission. The principal problem is that the ideas are not sufficiently new or specific to form a basis for peer commentary that would be useful for the cognitive/biobehavioral field as a whole.

I hope you will find the 6 thoughtful referee reports useful challenges for fleshing out the predictive, empirical aspects of these ideas.

Thank you for allowing PSYCOLOQUY to consider your ms.

Best wishes, Stevan

Stevan Harnad
Editor, Behavioral & Brain Sciences, PSYCOLOQUY

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REPORT #1: Tom Zentall (ZENTALL@UKCC.uky.edu)

I have now had a chance to look over Richard Marken's ms. "The blind men and the elephant: Three perspectives on the phenomenon of control". On the one hand, it is far enough outside of my area of specialization that I that I DON'T THINK I CAN GIVE IT A CRITICAL EXAMINATION WITHOUT READING A NUMBER OF THE REFERENCES ON WHICH THE MODEL IS BASED. On the other hand, I tend to look favorably on any paper that tries to integrate disparate areas of research. This paper presents a model that purports to integrate S-R, R-Srf, and cognitive theories of behavior. This is the kind of paper that asks to be published in an open commentary format. It is provocative, relatively simple, and tries to be inclusive. Even if others find it flawed in some way that I cannot see, I think it will generate interest in the reader. For this reason, I would recommend its acceptance.

As a general comment, there are many theories that are based on the principle of an optimal level of stimulation that an organism tries to maintain (the author might want to reference, e.g., Fiske & Maddi, 1961; Zentall & Zentall, 1983; Zuckerman, 1979).

More specifically, it is probably reasonable to assume, as the author has, in deriving equation (5) that s* = 0. It seems less reasonable to assume that "system amplification" approaches infinity as it needs to to derive equation (4). This may just show my lack of understanding of control theory, but other readers may need to have this explained.

Finally, although I feel that this submission makes an important contribution to the journal, I don't feel that my thoughts are sufficiently developed to write a publishable commentary on this piece.

REPORT #2: ANONYMOUS

I'm afraid I am UNABLE TO SEE HOW THE PAPER REPRESENTS A NEW CONTRIBUTION AND CONSEQUENTLY CANNOT RECOMMEND IT FOR PUBLICATION NOR CAN i OFFER ANY SUGGESTIONS FOR SUITABLE REVISIONS. The paper raises some interesting questions which are probably worth discussing but I think that the author NEEDS TO OFFER A MUCH MORE GENERAL AND INTELLIGENT TREATMENT OF THEM. I regret that I cannot be more positive.

The central thesis of this manuscript appears to be that an animal's behaviour is generated in order to keep certain sensory input variables at (or at least as near as possible to) preset values (set-points). This thesis is attributed by the author to Powers (e.g., 1978) and perhaps this particular way of putting it is peculiar to him though it is clear that it is JUST A REPHRASING OF THE CYBERNETIC VIEW OF BEHAVIOUR INTRODUCED A LONG TIME AGO BY NORBERT WIENER AMONGST OTHERS AND A RATHER SIMPLE VERSION AT THAT since it restricts the type of control system used to conceptualize behaviour to simple servo-mechanisms and homeostats (NO MENTION IS MADE OF OPTIMAL CONTROL SYSTEMS, ADAPTIVE CONTROL, MULTIVARIATE CONTROL, AND SO ON WHICH MAKE UP THE TOOLBOX OF THE MODERN CONTROL THEORIST). In any case, as far as I can see the author is not putting forward an original thesis and it hardly represents a "new conception of behaviour" - the idea of behaviour being part of a homeostatic mechanism is older even than Wiener's cybernetics.

In my opinion, the manuscript's central thesis is a rather technical way of expressing the "fact" that behaviour is (typically) goal-directed and that talk of behavioural homeostasis and cybernetics were, in the past, attempts to deal with goal directedness without mentioning any teleological terms which were considered dirty words before about 1970. In effect though, it says that goals are represented by an animal as sensory set-points - which is typically the way goal states are represented in servo-mechanisms and homeostats (though we wouldn't want to say that servo-mechanisms have goal-states, merely that a servo-mechanism is a tool for achieving a goal state). It is obvious that an animal engaged in a goal directed activity must have some way of determining whether the goal has been achieved and it will typically do this by observing/sensing the state of it's relationship to the environment or simply the state of this environment (because goals are typically desired states of this kind). Indeed, I think that the author's strict adherence to his talk of sensory set points and the accompanying notation unnecessarily complicates matters since it distracts one's attention from the simple fact that he is talking about goal states which in our enlightened times he is perfectly at liberty to do. It would be very much easier to talk about states of the environment (which are represented by "sensory input variables") and goal states (represented by sensory set points) - I'm not convinced that the author's formalism goes far enough beyond this to warrant its introduction. Consequently, I'm unconvinced that discovering

what an animal's sensory set-points are is any different from discovering what its goals are. The latter is none other than a functional analysis of behaviour which people have been trying to do for a long time.

It seems to me that the author's formalism not only fails to add anything to a functional analysis in terms of goal states but actually OVERSIMPLIFIES MATTERS TO A CONSIDERABLE DEGREE. For example, equation (1) strongly suggests that the actions which tend to reduce the difference between the desired state and the current state are of a single type which differ only in magnitude depending on the size of the error signal. This is CLEARLY UNTRUE OF animal and human behaviour in which the action taken to minimize such an error can vary widely - there are typically lots of means to an end. Note further that the assumption that the functions in equations (1) and (2) are linear is ENORMOUSLY RESTRICTIVE; it allows the author to treat k.e, k.f and k.o as numbers permitting the derivation of all the other equations which appear in the manuscript. It is completely unclear what would happen if the assumption of linearity was dropped (as it surely needs to be) since it is vitally important for equations (3) to (7) which would then no longer hold. For example, equation (5) cannot be said to establish the "behavioural illusion" since it relies on the linearity assumption.

I failed to understand what the author was getting at in section 3.3.1 the "stimulus-response" view of control. The pupillary reflex, for example, was one of the first types of behaviour to be subjected to a control theoretic treatment that the author is advocating and this treatment is widely accepted - the pupillary reflex is a servo-mechanism and similar treatments were offered a very long time ago for other reflexes e.g., the muscle stretch reflex - the idea that active muscle force, F, is related to the difference between its stretched length, x, and its unstretched length x* is described by an equation of the form, F = k(x*-x) is basic stuff. The discussion of Warren et al. (1986) is very curious - their treatment of the control of running is basically control theoretic: they argue that the optic variable tau determines the input to the muscles necessary to achieve the goal of placing the feet correctly.

I don't really follow section 3.4.1 because the author does not discuss learning which seems to me to be a central part of reinforcement theory.

I think that the three types of "view" that the author discusses represent ways of attacking the problem of understanding goal directed behaviour - we need to understand how goals are selected and how methods for achieving these goals are arrived at (part of cognitive psychology); we need to understand how perceptual information is used to control on- goingbehaviour once that behaviour has been selected as suitable for achieving the desired goal (the "stimulus-response" style of the Warren et al. paper discussed by the author); and we need to understand how the organisms learns what to do in order to achieve its goals (the domain of reinforcement learning theory discussed by the author). I FAIL TO SEE WHAT THE AUTHOR'S ANALYSIS ADDS IN THE WAY OF CLARIFICATION OR METHODOLOGY AND DO NOT SEE IN WHAT WAY IT REPRESENTS AN ORIGINAL CONTRIBUTION. In short, I cannot recommend that this manuscript be published.

REPORT #3 Eliot Shimoff (shimoff@umbc4.umbc.edu)

Marken's "The Blind Men and the Elephant" seems like an attempt at a grand unification theory, showing that S-R relations, reinforcement theory, and cognitive psychology are all aspects a single process (a la Powers [1978]).

Such attempts are not unprecedented; radical behaviorists have, for example, often argued that cognitive phenomena can be best viewed from an operant perspective, and cognitive

psychologists have argued that operant phenomena reflect underlying cognitive processes. Some of these attempts have proven fruitful, in the sense that they have suggested experiments or clarified muddy conceptual issues.

A serious attempt at unification must (in my opinion) (a) lead to interesting NEW EXPERIMENTS, or (b) force CLARIFICATION of some muddy concepts, or (c) make a SURPRISING PREDICTION (e.g., "If this theory is correct, you should observe phenomenon X which is not predicted by any other system").

Part of the problem may be that the VARIABLES ARE NOT PRECISELY DEFINED. The precise measurement methodology for determining d (the environmental variable) and r (the response variable) are far from trivial. One might make a convincing case that the major distinction between cognitive and radical-behavioral approaches is in the definition of what counts as behavior (and how it is measured).

Marken suggests (I think) that the proper task of psychology is to determine k.f, k.o, and k.e. But wouldn't those depend, for example, on what r is and how it is measured?

Regrettably, the present paper doesn't seem to suggest new experiments, clarify concepts, or predict any unexpected phenomena. I am not sure what kinds of comments would be occasioned by its publication. Perhaps it is all _too_ theoretical (in the pejorative sense); specifying new experiments, clarifying old concepts, or making novel predictions might make the paper more suitable for Psycholoquy.

REPORT #4: Ed Fasse (edfasse@Athena.MIT.EDU)

Marken is suggesting that behavior is control. Engineering control theory has been applied to study of behavior before, particularly in the study of motor behavior. The idea that certain aspects of behavior can be understood in terms of control is thus NOT NEW. The proposition that behavior IS control may be new. In any case the following question is interesting: To what extent is behavior control. Those are not Marken's words, but that is the question he is asking as I understand it. I would add (1) can all human behavior be understood in terms of control? and (2) even if it can is this useful, i.e. will it lead to new understanding about function of the nervous system? I have my doubts about the first question, but even if it is possible to describe every possible human behavior as a control problem this reformulation will not always shed insight on processes in the brain. For example, it may be possible to describe playing chess as a process of controlling some "sensed aspect of the game" as Marken puts it. I don't think so, but even if that's the case I doubt that that is an accurate description of processes in the game. It's not a good description of how computers play chess.

I think the question "to what extent can human behavior be described in terms of control" (Not Marken's words, but I think his intent) is worthy of consideration. I personally feel that to say that behavior IS control is again saying that an elephant is a snake with a nostril that eats peanuts.

I am not comfortable with all of the presentation however. Some of what follows is criticism, the rest is comment. I will try to make the distinction between the two clear.

1.0 Introduction

1.1 I think that it is valid to say that the goal of control is to produce consistent results, but I DISAGREE that this always takes place in the context of an unpredictably changing environment. It is my understanding that most computer hard disk drives use

stepping motors which are well enough behaved that feedback control is unnecessary. They operate open loop, and if there is a significant disturbance the drive will fail unrecoverably. Similarly, the human speech production system does not have to deal with the amount of environmental disturbances that the limbs do as the acoustic properties of air don't change much. That is my opinion. I think it is valid to say that the goal of control is to produce consistent results in an unpredictably changing environment. I don't have any problem with section 1.1 as is.

2.0 Closed-Loop Control

2.1 The first sentence, "The basic requirement for control is that an organism exist in a negative feedback situation with respect to its environment" is FALSE. Marken later implies that stability requires negative feedback. This is not true in general. The range of feedback gains that correspond to stable behavior depend on the underlying dynamics of the system that is to be controlled. The system to be controlled may be inherently stable, in which case a little negative feedback will not destabilize it, and may enhance performance. For multidimensional, linear feedback it is not the sign of the indivi- dual feedback gains that count, but the properties of the resultant feedback matrix. Anyway, it is not true that negative feedback = stable, although that is often the case.

2.2 The definitions in this section are too VAGUE. Are sensory input variables raw, unprocessed sensory information? Is this something I might find in the primary sensory cortex? Or are these perceived qualities or quantities? Let's say an individual sees a piece of cheese, and that they decide to pick it up and eat it. Is the sensory input the light coming from the cheese, the retinal image of the cheese, the percept that it is an object, the percept that it is a piece of cheese, or what? Are these quantities defined instan- taneously or over time? Do they change continuously or discretely? Are they indeed quantities, i.e. can we assign them numbers? Is a piece of cheese s = 6 and a piece of cake s = 7? The definitions as is are so vague that I can see applying them in any number of ways. If they cannot be made more precise I don't see the utility, and I feel it will not be possible to make them suffi- ciently precise while describing all human behavior.

I have similar questions about the response variables. Is a response something like a set of motor outputs, something I could measure from a pool of motor neurons? Is it more complex like "man eats cheese"? I think it is meant to be more complex, but if that's so, how is it going to be quantified given the mathematical tools that are applied later on? "Man eats cheese" is r = 3 and "Man throws cheese out window" is r = 5? It's not clear that the mathematical tools that are used later are applicable. What is an environmental variable? Is it truly extrinsic to the human, or is it sensed. In section 3.1 Marken says that "The environmental variable, d, is seen as a stimulus, such as a light or a sound." I would think that the human only has knowledge of the environment via the sensory input, s.

I'll pass on k.o, k.e and k.f for now.

2.3. and 2.4: Equation (1) is a purely static model, as are the other equations. These equations are not dynamic, that is they cannot make predictions about evolution of the system with time. In section 2.1 Marken comments on stability, yet these models have no concept of stable or unstable behavior. It might be useful to assume this for sake of argument for equation (1) at times, but not for equation (2). Equation (2) is a description of how sensory input changes depending on responses and the environment. This relation will in general be dynamic. r,s and d will be related by differential equations in the continuous case, not by static, algebraic equations such as (2).

2.5-2.6: Equations (3) and (4) are static constraint relations. They are not causal statements for r. Equation (1) is the causal statement for r. Marken speaks of sensory energy and response energy. Does he really mean thermodynamic energy, or does he mean energy-like quantities as is often done in engineering? For example, the square of magnitude is often related to energy, so it is sometimes loosely referred to as energy. Saying that a little s lead to a big r does not necessarily imply that a small sensory energy leads to a large response energy. It depends on the energy functions. Also, saying that k.o can be "quite a large number" is not in general meaningful. Here one can argue that it is okay since Marken required that s and r be in the same units, but in engineering control one usually refers to the loop gain rather than the feedback gain as the relevant quantity. After all, what is a large number? 0.1 metric tons/mm? 1000 Newtons/light year?

The energy discussion seems irrelevant. Marken wants justification to let k.o approach infinity. His point is that as feedback gains increase the steady state (static) behavior of the controlled system becomes less dependent on the dynamics of the system which is being controlled. This makes the controlled system behavior less susceptible to changing dynamics. Fine. I would just say that and forget about the energy argument, which I feel is misleading. If it is important I would develop it better.

I do not like the last paragraph of section 2.6. Equation (4) is not a causal statement, and thus cannot be used to show that the organism acts to keep its sensory input constant by varying responses to compensate for variations in the environment. The equation is correct given Marken's assumptions, but the inferrence about behavior is not valid. r = k.o (s*-s) is the causal statement. One could say "IT IS AS IF the organism had access to information about the environment, d, and that it was changing its responses based on this information. However, information about the environment is only available via sensory information, so the actual control law is r = function of (s*,s)."

I am not happy with section 2. Control theory has been applied to understanding behavior before, notably in motor control. I do not feel that this presentation is particularly adept. It did not teach me anything I didn't know before. Again, what I see as interesting is not "Can aspects of behavior be understood in terms of control?". Yes, they can. It's been done before. What I see as interesting is "Can ALL behavior be understood in terms of control?".

3.0 Three Views of Control

3.1-3.2: Again, equation (4) is not causal and should not be used for making inferences. Equation (1) says stimulus causes response. Equation (4) says that the response is compatible with the environmental variable d. What does Marken mean to say that the environmental variable is seen as a stimulus. How is the human aware of the environment except via the sensory information s? Or is d sensory information too?

"The value of s* can be constant or variable, it's value at any instant being secularly determined by properties of the organism itself." A change in s* has an observable change in behavior. How is it changing? What processes are changing it? These processes are affecting behavior, and since we are trying to model behavior, are we not then concerned with modelling the change of s*? Is s* the response of some other control process? Some planning process? s* affects behavior as much as anything else, and it seems that it is being swept under the rug. How can we model the process that generates s*?

3.3.1: I cannot really comment on this section as I am not familiar with the references.

3.3.2: I think of a stimulus as being a sensory or perhaps perceptual event. Marken seems to think of a stimulus as an external event. If this is true, then it makes sense to speak of d as a stimulus. From my perspective to say that d is a stimulus is nonsensical. From my perspective the control law, equation (1) implied stimulus-response, not equation (5).

3.3.3: I think the statement of this section is too strong and misleading. Feedback makes the particular dynamics of the system that is being controlled less visible. It does not make them invisible. I would change "negative feedback" to "feedback", as the fact that the feedback is negative is irrelevant. I have no idea what Marken means by a "behavioral illusion" as I am not familiar with the reference. I would like more explanation.

3.4.1: I can't comment on this section.

3.4.2: "The sensory effect of a reinforcement can be assumed to be directly proportional to its size and weight making k.e = 1." That's just plain wrong. s = k.f (r) + k.e (d) is already linear with respect to d. Assuming d is some parameter of size this expression already says that the sensory effect is directly proportional to d. That constant of proportionality is k.e, it certainly does not have to equal 1. Or else I'm missing something here in a big way, but I don't think so.

3.4.3: Well, that's logically consistent, but don't think that's actually what's going on.

3.5.1: Okay, but what's the point. Is this an introduction for 3.5.2?

3.5.2: But what is generating s*? Presumably some cognitive process or another controller. The process that is generating s* has as much influence on behavior as the controller. What is this process? One cannot say control explains behavior until one explains the generation of s*, which affects behavior.

4.0 Looking at the Whole Elephant

This section is fine. It is logically consistent with the rest of the paper. The statements in it are contestable. "Organisms behave in order to keep sensory inputs at these reference values (Powers, 1989)." That is debatable, but that's the kind of statement that would be good to discuss by the community at large.

5.0 Appendix

I think the derivation in this appendix is sufficiently obvious that it does not warrent inclusion. It is correct.

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So, I think the question "can ALL behavior be described as the result of a control process" (not a quote, but I don't have italics) is interesting. I feel the theoretical, mathematical analysis of the paper is WEAK, and NOT PARTICULARLY ORIGINAL. I think it would be best to get rid of most of it and focus on formulating definitions of the concepts introduced in section 2.2 that are as precise as possible. I also think it would be useful to spend more time on the process that generates s*, rather than the VAGUE statement that "it's value is at any instant being secularly determined by properties of the organism itself". That statement doesn't tell me anything.

REPORT #5: ANONYMOUS

I've taken several long looks at [Marken's] paper on control and feedback and don't feel I have anything helpful to the author. It reminds me of the sort of off-the-wall discussing we used to do late in the evening after studying for comps. There's been so much specific research since then, I would have expected such speculations to take a new tack.

But that's just my reaction. Other readers, who didn't have the benefits of my fellow graduate students might profit from being stimulated by such a paper.

REPORT #6: Vasant Honavar (honavar@iastate.edu)

The author raises a number of interesting issues concerning the the central role of control as a determinant of behavior. I am not in a position to evaluate the author's suggestion that psychological studies of behavior ignore issues of control for the most part. Perhaps other reviewers will address this issue. I will restrict my comments to some general suggestions on how the paper could be strengthened by a more thorough consideration of the principle thesis put forth by the author.

Cybernetics (the science of control and communication in humans and machines), starting with the pioneering work of Wiener in US and Lyapunov in the former USSR during the late fifties very much recognized the importance of studying control. Indeed Lyapunov characterized the evolution of living nature (and hence one might argue, intelligent behavior) as a process of proliferation of, and selection among, control systems of various types (see Honavar & Uhr, 1990 -- Coordination and Control Structures and Processes: Possibilities for Connectionist Networks; Journal of Experimental and Theoretical Artificial Intelligence 2 277-302 for a discussion of this and a number of related iss> ues in the context of artificial and natural intelligent systems). Similar considerations have motivated contemporary work on models of intelligent behavior within the dynamical systems framework.

I am sympathetic to the author's view that the phenomenon of control needs to be studied in its totality. However, I would like to see a STRONGER AND MORE COMPLETE ARGUMENT developed -- perhaps using specific examples of psychological phenomena that can be better understood and explained (than is permitted by current approaches in psychology) by taking a holistic approach to the study of control. This is just one suggestion and I am sure there are other ways to strengthen the paper. I recommend that the paper be resubmitted for publication in a revised form. If the paper is eventually accepted for publication in Psycoloquy, I will be more than happy to write a commentary.

Date: Sat Dec 12, 1992 6:54 pm PST
Subject: Re: Sciligion; gang of 3 to 5

[From Bill Powers (921212.1400)] Greg Williams (921212)

RE: Science and religion

There are two sides to religion. One of them, the good side, consists of the attempt to adopt and live out principles that make civilization possible. As most people never think

about such things except in the context of a religion, one wonders what the world would be like without such formalized social systems of belief.

The bad side shows up because people have different religions. If those living under principles of love and tolerance could actually live up to those principles, all would be well. But aside from the fact that not all religions preach universal brotherhood, it doesn't seem possible for people to live up to their religious principles when those principles disagree with someone else's.

The basic reason, I think, is the assumption of supernatural origin of the religious principles. When you believe that you are in receipt of the word of God, directly or through an authorized dealer, there can be no tolerance for deviations. The word of God is absolute. This means that if a different group claims to have heard a different word, or a different interpretation of words, the other group must simply be wrong. Every religious group must feel this way about every other group, no matter what they say. Very quickly this comes down to the choice of converting the other group to the true belief ("saving" them), isolating from the other group, or eliminating the other group.

Each group, of course, must resist all attempts by the other group to evangelize, because succumbing would be going against the word of God. The loop gain, with respect to adhering to the word of the Infinite, must be infinite. This means that even minor differences of doctrine can lead to maximum conflict.

All that saves us from continuous violent confrontation between religions is that very few people are actually as religious as they think they are, or claim to be.

John Gabriel (921212 12:44CST) --

Maybe that principle should be "Thinking the unthinkable so we don't have to try to do the undoable."

So far, as near as I can tell, thinking the unthinkable usually seems to result in doing the unthinkable. We just keep redefining what is unthinkable. Burning living people to death was once quite thinkable, then became unthinkable, and then became thinkable again. Anything you can imagine is a potential reference signal.

I have to confess that you guys are way over my head. The one sentence that stood out for me was

>That is to say, the mathematics has degrees of freedom which
>are lost once you go [to] a physical system of any kind.

This is a nice way of putting my objections to an overmathematicized approach to anything. Mathematics deals with abstract relations, which usually means that mathematical systems apply to a great many worlds that do not actually exist.

And it's not only the degrees of freedom that collapse when you try to apply a mathematical system to physical systems. Very often the premises collapse, too. What if the brain's operation does not consist of a large set of ordinary differential equations? What if the ECSs of the brain are not a multitude of tiny independent systems, but a much coarser organization with systems designed for special purposes? No doubt, everything you say about systems that are composed of multitudes of tiny systems each acting like an ODE can be proven to be true, but that is irrelevant if the real system isn't made that way. It seems to me that the first order of business would be to find out what kind of system we are actually dealing with.

This is all undoubtedly old stuff to you. But whenever I encounter a mathematical system that's far beyond me, I try to salvage some self-respect by wondering if it's really necessary.

Best to all, Bill P.

Date: Sat Dec 12, 1992 7:16 pm PST
Subject: Harnad's reviews

[From Bill Powers (921212.2000)] Rick Marken (921212) --

Incredible, ain't it? If you write a basic paper on PCT, they'll say it's irrelevant to psychology. If you try to make it relevant to psychology, they'll say it isn't basic. Looks like there's no way to get there from here.

Those reviews you posted brought to my mind the whole dreary stream of trivial reviews we have all seen over the years. This really seems to be the best that established science can do, at least in the journals we have seen (and that includes the major ones, I think). The awful truth is that these scientists do not want to learn anything new. They want their own scientific endeavors and those of their colleagues to be validated. There's no way to sneak PCT past the guardians of the gates, because they all recognize that there is something strange about it, something unfamiliar, and therefore something wrong. The whole system is organized to protect itself against being upset.

It seems to me that we already have the forum where PCT is understood and can be developed. Let's forget about the rest of the scientific world and just do our modeling and talk about the results here. People who are interested will stick around and learn and contribute. People who aren't will go their own ways as usual. Most of the people who subscribe to this List have been here for over a year, most for almost two years. That says that something of interest to them is going on. Maybe if we start treating this list as a place to suggest and report on research, and to trade methods and ideas for research, the list of subscribers will become even larger. Maybe if we keep it up, most of the people who are actually interested in exploring a new idea will end up on this net, while the rest of the scientific world reads the journals like comforting bedtime stories.

I think that psychology is old and tired, and ready to be bypassed.

Best, Bill P.

Date: Sat Dec 12, 1992 9:05 pm PST
Subject: RE: "Blind men"'s rebuff, Another PCT Tech Report (Long!)

From Tom Bourbon (921212 22:55)

Rick,

Too bad your fate was in the hands of people with scientific bents -- they don't have much use for old fashioned fuzzy heads like PCT modelers. Maybe we should just accept the fact that they already know everything we try to say -- and then some. They just don't talk about it -- ever -- until they review a PCT manuscript, that is.

The message is clear for those of us in psychology: If you want to publish PCT in "real" journals, water it down and turn it into another "perspective" or "framework," cite everybody they think is important, and leave all of the serious issues alone. I am happy that sociology and now education are fields in which at least some of the reviewers are not so scientific.

Maybe we can put out a special holiday set of Technical Reports -- two for the price of one, but suitable only for nonscientists.

Too bad, buddy. Tom Bourbon

Date: Sat Dec 12, 1992 10:06 pm PST
Subject: arm2 docco remarks II

[Avery Andrews 921213.1700]

On the basis that the arm documentation ought to be reasonably self-contained, it would be good to add a sentence or two c. pg. 16 explaining what the integration factors is 1,2,3 are doing. Is there any actual evidence that there are such integrators in the arm control circuitry?

Avery.Andrews@anu.edu.au

Date: Sun Dec 13, 1992 6:33 am PST
Subject: One Whole Minute

[From Bill Powers (921213.0700)]

John Gabriel, Bill Cunningham, Tom Baines, Martin Taylor, et. al.

At the risk of alienating friends, I have to make some further comments on John Gabriel's post of 921211 11:12CST.

My first impression was that we are seeing hints of a great and complex work by a group of people who have vast mathematical expertise and deep experience with the world. After a night's sleep, I awoke with the different impression. If this work is so fundamental and important, why don't I understand it? And why, beneath this work, does there seen to be such an unthinking acceptance of the premises of military philosophers? Would a scientific advisor to a torturer find it just as easy to become immersed in the technical problems, and to ignore the underlying repulsiveness of the whole undertaking?

I'm perfectly aware of my limitations and my ignorance, but the fact is that when, as a teenager, I read Einstein's explanation of relativity, I may not have known what a tensor was, but I understood the idea perfectly well. When I read Norbert Wiener's account of control processes in living organisms, I may not have seen the relevance of stationary time series or followed his arguments about Newtonian and Bergsonian Time, but I understood how a control process worked and what it had to do with behavior. When people describe a phenomenon of nature and offer a clean and simple explanation of it, I usually understand what they are getting at. So, I think, do most people.

When I read your post, John, I could not figure out what you and the others were getting at. Beneath my surface reaction, I was wondering "Why are they making it so complicated? What are all these theorems supposed to be about? What makes them think that any of this

has anything to do with reality? Are these people really of such a different order of human intelligence that they can see simplicity and order in such seemingly vague and abstract conjectures?"

Frankly, I don't think that the answer to the last question is "yes" for ANYBODY who works in the field of human behavior. What I see in morning's light is mathematics leading people around by the nose, while they struggle to find real applications that will give the resulting abstractions some shred of meaning. I see a search for understanding of the mysteries of human behavior misdirected, so it becomes a search for mathematical truths in an imaginary universe.

What bothers me most is that this project of yours seems to accept that the solution to human problems like warfare is to figure out better ways to defeat the enemy. This is a total misapprehension of the nature of the enemy and of the problem. It bespeaks an acceptance of warfare as a fact of nature, something we simply have to live or die with and about which we can do nothing at all except to try to come out the winners. To apply PCT in this way is like using Galileo's telescope to hammer a nail.

In PCT there is the potential for understanding human conflict and its resolution. That kind of understanding, not the endless improvement of physical and organizational might, is what will finally make the inthinkable undoable.

I wish you all would do the simple tracking experiments in Demo 1 and Demo 2, and ponder the outcome of the experiments at the end of Demo 2. Here we have a model that will explain a simple motor behavior carried out by a human being for one minute. It explains it by predictively simulating the detailed movements by which a person counteracts a random disturbance. If you fit the model carefully, you can then predict the person's movements for any new pattern of random disturbances, for a whole minute, with an accuracy of about 3 percent of the peak-to-peak excursion of the control handle. As Tom Bourbon has showed, you can predict with this sort of accuracy how the person will behave a year later; you can get the same predictively accuracy for 100 people picked at random. Rick Marken has done the similar experiments in two dimensions and with more complex controlled variables. If you didn't look very carefully at the traces of real and simulated handle movements, you wouldn't know which one represented the human being's behavior and which one represented the model's behavior.

Think about it. We can predict human behavior with this accuracy for a WHOLE MINUTE. Tom Bourbon has shown that this can be done with two people interacting in the presence of unpredictable disturbances; two models will interact in the same way, with the same accuracy, for ONE WHOLE MINUTE. Rick Marken has shown that you can predict motions in two dimensions, with two-dimensional disturbances and with a coupling between the dimensions, for ONE WHOLE MINUTE.

There has never been this kind of ability to predict any human behavior whatsoever in the whole previous history of the life sciences -- not even the simplest of behaviors. Most psychologists, cyberneticists, physiologists, and so on who have seen this demonstration or who have reviewed articles about some version of it have completely missed the point: they have never seen anything remotely resembling this kind of accuracy of prediction of behavior; as a consequence, they miss seeing it altogether. They don't understand what the demonstration is showing. You can say to them, ten minutes later, "Control theory has the potential of explaining behavior with the precision of a physical theory," and they will immediately launch into counterarguments about variability and population statistics and individual differences and environmental influences and such like, proving conclusively that they didn't understand what was in front of their eyes.

One whole minute, with one person or two persons, in one dimension or two dimensions. That's the best anyone in the world can do right now. That's where the true science of behavior stands. The next step is not to solve the problems of military organization or the causes of crime or the behavioral effects of chemical concoctions or the complexities of adaptive control in a network of 1E10 neurons with 1E14 connections. It is to try for two minutes, three people, three dimensions, five different kinds of controlled variables, always demanding that the prediction be precise and work for every individual. This is how a science of human behavior will arrive on this planet for the first time.

The vast complexities of human interactions will still be there when this science of behavior has become able to handle them. They will also still be unsolved by any of the present approaches, even those that are trying to leapfrog all of the hard and detailed work that is actually needed to bring PCT to maturity. I think that any researcher who is trying to get lucky with applying PCT to vast systems is wasting time. We are still crouched beside Galileo, watching balls roll down an inclined plane and timing them with our pulses. Nobody can imagine what future will follow from these first primitive experiments. Of course that future will never arrive if everyone insists on trying to make a laser with the materials and knowledge available to Galileo.

Best, Bill P.

Date: Sun Dec 13, 1992 6:57 am PST
Subject: Neural integrators; protecting muscles

[From Bill Powers (921213.0730)] Avery Andrews (921213.11700) --

Yes, there are actual neural integrators. Given the sudden appearance of an input signal of constant frequency, some neurons will produce an output frequency that slowly rises, eventually reaching an asymptote (a leaky integrator). When the input signal is removed, the output frequency will slowly decline. I believe these are well-known, but I'll leave it to others to supply exact references. I don't have one.

As to your question yesterday: one could say that the combined tendon and stretch reflexes are designed to prevent excessive efforts from developing when obstacles are encountered, except that this would be a result of their operation, not a cause. Evolution designed them, so presumably the design does tend to prevent injury to the organism.

There is, however, nothing that monitors for "excessive force" and prevents it from happening. It is perfectly possible for driving signals to the muscles to become so large that muscles are pulled from their attachments or bones are broken. Did you happen to see that grisly episode of some sports program on TV where, during an arm-wrestling match, a contestant's forearm snapped? Bob Beamon, when he broke the world record for the long jump at the Mexico City Olympics, pulled a muscle so severely that he was out of action for a long time -- more than a year, I think. Sports figures are always injuring their muscles simply by using them too hard.

I don't think that looking for some possible advantage of a behavioral organization goes a long way toward explaining it. Such advantages are guesses anyway, and they're side-effects of what really makes the system work. We could say that organisms have control systems because control systems can survive better than any other kind, but that doesn't explain what makes control systems behave the way they do. Rather, it's the way control systems behave that explains why organisms organized that way have survived.

Best Bill P.

Date: Sun Dec 13, 1992 8:36 am PST
Subject: Camma and alpha coactivation

[From Bill Powers (921213.0845)] Avery Andrews (921213) --

Avery, you also asked for a source on the combining of gamma and alpha reference signals for the Little Man model.

A reference is McMahon, Thomas A.; _Muscles, Reflexes, and Locomotion_ (Princeton, NJ; Princeton University Press, 1984). See Chapter 6, Reflexes and Motor Control, subhead Coactivation of Alpha and Gamma Motor Neurons (p. 148). " ... so the experiments are in harmony with the idea that both alpha and gamma motor neurons are excited by higher motor centers at the same time."

I think that gamma outputs are also involved in autonomic reflexes, but don't want to get into that kind of detail.

Best, Bill P.

Date: Sun Dec 13, 1992 9:41 am PST
Subject: RE: One Whole Minute

From Tom Bourbon (921213 11:36 CST) Bill Powers (921213 08:33)

Elegant, Bill. Simply elegant.

Date: Sun Dec 13, 1992 9:58 am PST
Subject: Re: Sciligion; gang of 3 to 5

[From Gabriel 921213 11:40CST, reply to Powers of same date]

I don't know quite how to respond Bill. I am both in agreement, and in disagreement with you.

Agreement that if the mathematics has a set of axioms that omit an essential part of the the physical phenomenon, then the mathematical model may describe something, but it doesn't describe the physical system being discussed. But this will be found when the mathematics and the reality are perceived to be disconsonant.

Disagreement with the implied conclusion that mathematics is not a useful model. If the mathematics and the sense data agree, this is evidence that the mathematics is a reasonable predictor of things we already know, and so we CAN, but don't have to, use it to attempt to predict things we DON't already know, and THEN go and see if they are observed.

It seems to me that that's what "models of reality" are all about, and that's what your really insightful book deals with.

Two other comments:-

Perhaps what you are saying is "Only a topologist could believe that the donut and the coffe cup are the same thing." That depends on what you mean by "same." If by same you something like "If represented in three dimensions by bounding surfaces within some small distance from the boundaries between air and material for the donut and coffee cup on the breakfast table before you, treating the breakfast table as if it were air, THEN the bounding surfaces may be continuously transformed one into the other without tearing." the topologist is speaking the truth.

All this gets you into Avery Andrews' "aforesaid" problem, and it only gets worse the more you try to say, because there are more aforesaids.

There is a good mathematical model of this issue, but it's not very accessible, so perhaps it's not very good after all. But as my father was fond of pointing out, people only directly perceived acceleration and deceleration regularly without coming to harm (like falling off a cliff) after automobiles were invented. Before that Newton's Law of Acceleration was just about as academic an experience as topology is today.

About burning people to death. Let's debate that off line. Perhaps we may find we agree more than you at present perceive. My choice of words was unfortunate - but I am short of time, and not as good as you and Martin at expressing myself. Sorry if I hit a sore spot. That's one of the reasons why the "gang of five" converse mainly off line. You and I have both experienced war - I was bombed in Britain, and you were in the Navy, and we probably neither of us liked it very much, in spite of the adrenalin rush we likely both know.

One thing I do believe - the Chicago Stockyards used to use everything but the squeal. If we have experiences, we ought to use them to illuminate our world. Better to light a candle than to curse the dark.

I don't want ever to shoot anybody, but I still practice with an M1911A1, and if I have to choose between dying myself, and using lethal force to stop somebody who was trying to kill me (or anybody else not deserving to be shot at), I don't intend to hesitate very long.

Nor I think would you, if if your new granddaughter were in mortal danger.

Well, let me get down off my soapbox. I'm not sure the net is the proper place for the above discussion. Let's continue off line if we feel so inclined. I don't think Gary will object.

With Very Best Regards to All, Malice to None, and wishes for a Joyful, Peaceful, Holiday Season.

John (gabriel@athens.eid.anl.gov)

Date: Sun Dec 13, 1992 12:29 pm PST
Subject: Developing Curricula on PCT

Bill P --- Congratulations, and best wishes to you and your familly
on the new arrival!!!!!!

[from Ray Jackson (921213.1230 MST)]

len lansky (921211.1143) >a little bio and request for help.

Bill Powers (921212.0300)
>At a more propitious time I will start putting together an outline
>for a one-quarter (13-week) course in PCT. Unless someone else beats me to it.

At the same time, I would like to express interest for a similar lesson plan; I am going to be developing some PCT material for an MBA Organizational Behavior class I will teach next fall. Of course, I see the PCT ideas as critical to a true understanding of the subject. However, I will probably only be able to spend 2-3 weeks on it before moving into the rest of the course content (but, then I can reiterate the theory through examples during the rest of the semester).

I will also be teaching an undergrad class in Organizational Development, where I will emphasize PCT as well, but probably not as extensively. I won't have as much freedom with that material.

For the past 18 mos, I've taught what I know of PCT to managers and supervisors in the workplace, and in various grad and undergrad classes as sort of a guest lecturer. The problem I've found in teaching PCT to whoever is listening is that although there are students who cling to every word, they often interpret everything with such a strong s/r filter that what you're saying only makes sense (or not) in THEIR world. But, of course no one on the Net has had to deal with that before...

It seems the initial task we face is to challenge the students MINDSET early on -- with such precious little time to spend with them (2-16 weeks), we can not afford to have them view the biggest part of the instruction through behavioristic eyes. From that standpoint, my experience has been to use the rubberband and other demos fully explained with as many real life situations as possible. For an overview of the basic mechanics of the theory, I use an over-simplified version of the feedback loop (i would send a e-copy, but it's a mac graphic; I'll send a hard copy if you like). And, of course, question and question them to see what's important in their lives, and then try to come up with examples based on that (education, industrial, sales, etc.). At that point, most have acquired the basic framework, and then you can help them to fill in the blanks.

By the way, I am sure that Dag Forssell will have some interesting input from his experiences teaching in the corporate setting. From looking at some of his materials, he's way ahead of most of us in this area (even though he touts his program as one for managers who think; I tend to work with ones who cant...).

Should there be a difference between the approach for educating individuals on PCT in the academic and corporate settings? From a global perspective, I tend to think not, other than keeping in mind their motivation to learn (error signal), as well as other minor differences based on the setting.

>(Bill P)...I envision a group project here, in which people
>rewrite the outline and re-rewrite it until we have something that
>looks good.

Not to mention teach, (evaluate), and re-teach, which is often the only way to tell the value of any educational piece. But, no doubt this will be something that looks (and is) good. I will continue to follow this thread closely, and contribute what I can. With everyone on the Net having a chance to join in here, we can develop the foundation of something to reach many people in diverse fields.

Best Regards, Ray

Date: Sun Dec 13, 1992 1:00 pm PST
Subject: Blind Men; Leadership

[from Gary Cziko 921213.1900 GMT] Rick Marken (921212.1400):

Rick, thanks for posting the psycholoquy reiviews on the Blind Men paper. I suppose one advantage of electronic submission and review is that it is easier to share the reviews with CSGnet.

I must say the reviews were real eye-openers for me. I see much more clearly now how much education it would take of "normal" psychologists before they could provide valid reviews of PCT-inspired papers. This was indicated by questions such as "where does s* come from" and "is a piece of cheese s = 6 and a piece of cake s = 7" (My goodness, isn't it obvious s = 6 is a medium piece of cheese and s = 7 is a big piece of cheese and when s = 8 you have a fondue?).

The problem is that trying to put in the necessary background would make the paper too long for a journal article.

Maybe Bill is right in that psychology has become irrelevant. Wouldn't it be neat if education and sociology and linguistics and other more "specialized" social sciences passed by psychology and left it in the dust (this wouldn't be the first time for linguistics). Maybe what the "real" PCT modelers like you, Bill and Tom ought to do is lend your skills and understanding to people in these fields which are less resistant to PCT.

Speaking of which, I don't know if I mentioned this before, but I did a little PCT show last spring for some of the staff of the federally funded National Center for School Leadership which is located on my campus. The director, Paul Thurston, found it quite interesting and has asked me to follow this up next semester with regular meetings with his staff and writing a paper with him on the applications of PCT to school leadership issues and problems (I will even get an extra month of summer salary, so you see PCT can pay after all!).

I hope to make heavy use of CSGnet to help me with this, especially people like Dag Forssell and Ed Ford and the management types out there. It will be interesting to see what type of underlying "model" current theories of effective school leadership are based on and how PCT gives a new perspective.--Gary

Date: Sun Dec 13, 1992 1:01 pm PST
Subject: Teaching PCT

[from Gary Cziko 921213.2036 GMT] len lansky (921211.1143) said:

> Help!!!! I have agreed to teach a seminar on control
>theory next quarter--begins in January--for ten weeks. I have
>the books and have been trying to understand and to classify the
>traffic on the net?

>Does anyone have a worked out sequence for presenting the theory.

I gave a graduate course focussing on PCT last spring and will do it again hopefully this coming spring (Mark Olson took the course last spring) under the disguise of "Psychological Theories Applied to Education."

Last year I used quite a bit from the Robertson and Powers's _Introduction to Modern Psychology_ as well as Powers's _Living Control Systems (I)_. This year I plan to use stick with LCS and focus on the Runkel's book with Ford's _Freedom from Stress_ thrown in for good measure. I also use lots of demos (Powers's and Marken's computer ones plus many of the manual, portable ones I've started to catalogue--"keep pushing those eyeballs until you fall down!"), show some of the videos that Ed Ford made of Powers's and will add in some new "trendy" developments in psychology (connectionism, new developments in education, maybe some AI stuff).

I haven't worked out a complete plan for next semester yet; that is what semester breaks are for. I also rely heavily on my good looks, charm, grace and wit to help sell PCT to the students on this campus (I wonder why it's taking so long).

Maybe Clark McPhail can give you some tips as well since he sneaks PCT into courses that are required of all sociology grad students on this campus (don't ask me how he gets away with it). He has less in the way of good looks, charm, wit and grace to fall back on (remember, he's a sociologist), so he probably has things better organized than I do.--Gary

Date: Sun Dec 13, 1992 1:01 pm PST
Subject: Hebb,"feedback" & cognitive psychology

[from Gary Cziko 921213.1945 GMT]

The January 1993 Scientific American has an article by Peter Milner entitled "The Mind and Donald O. Hebb." I particularly like the paragraph where Milner describes Hebb's use of feedback:

"Feedback was not entirely new in learning theory. Almost all models assumed that the output of the organism influences the input in some way, for instance, by enabling the animal to receive a reinforcing stimulus. Unfortunately, feedback proceeding in this way, through a single path, would operate slowly and often unreliably. But with millions of internally connected feedback paths, it would clearly be possible to establish internal models of the environment that might predict the effects of possible responses without having to move a muscle."

So for Hebb, feedback has to do with internal loops, not with a loop working through the environment. This allowed him to move easily from S-R psychology to cognitive psychology (S-O-R) without changing the overall perspective of stimulus and response.

I was very lucky to be a part of the last group of psychology graduate students at McGill University that participated in Hebb's graduate seminar. My first teaching assistantship involved teaching from Hebb's _Textbook of Psychology_. I was quite sympathetic to behaviorism when I started graduate school and Hebb made it easy for me to consider his brand of neuro/cognitive psychology as an improvement. I suppose it was this background that also made it easier for me to see the problems of both S-R and cognitive psychology after being introduced to Powers's work.

I highly recommend Hebb's _Textbook_ to anyone wanting to see how neuro/cognitive psychology can be understood as a straightforward modification of S-R theory.--Gary

P.S. One insight of Hebb's which I continue to value his is interactionist perspective on the nature-nurture issue in psychology. Anyone tempted to say things such as "70% of trait X is innate" or "65% of characteristic Y is due to learning" would do well to read Hebb's 1953 article in the first issue of the _British Journal of Animal Behaviour_.

Date: Sun Dec 13, 1992 2:59 pm PST
Subject: Re: Neural integrators; protecting muscles

[Avery Andrews 921214.0955]   (Bill Powers (921213.0730))

>Yes, there are actual neural integrators. Given the sudden
>appearance of an input signal of constant frequency, some neurons
>will produce an output frequency that slowly rises, eventually
>reaching an asymptote (a leaky integrator).

Okay. But is it actually known that real arm control works this way, rather than, for example, controlling perceived angular velocities.

Avery.Andrews@anu.edu.au

Date: Sun Dec 13, 1992 3:14 pm PST
Subject: Olive Branch

[gabriel to powers 921213 16:52 CST]   Dear Bill

On reflection perhaps I came on a bit strong. If you want to know why look at the history of ANZAC in WWI and NZEF in Crete in 1940. Also a couple of school friends. One had spent from age 10-14 in Japanese prison camp because parents were missionaries in Hong Kong. Another's father had been shot for being a member of the resistance in France, and the young man and his mother had had to take to the hills with his father's friends. That young chap had killed his first enemy sentry at the age of 12 in order to acquire a firearm. I suppose I didn't mind being bombed that much, I had other friends who fared far worse. I also think the move to denigrate Harris and the memorial to Bomber Command, was in poor taste, even though I feel strongly that Harris and Co. made a strategic mistake that prolonged the war. See Col. Harry Summers' book on VietNam. I also feel between the devil and the deep sea about Somalia, but there should have been the kind of debate Summers talks about. Difficult though, with people starving and the Administration changing.

But on lethal force etc. there are interesting issues in animal behaviour - see the Altmans on Baboon Society, and Rolf Peterson and others on Wolves. Escalation is always a problem, witness what is happening in the cities. Just exactly why I want to have better decision making in Govt AND in Defense.

If you want, let's debate off line, but perhaps just let it drop. By the way, in general I disagree with Strategic Bombing, and true history of the Japanese efforts to sue for peace in 1944 is also interesting, as is the Straits of Tsushima story. But both those belong to Bill C. I'm sure he'll tell you if asked.

Happy Christmas, and much delight in all the good things of life, including the grandkids.

John

Date: Sun Dec 13, 1992 4:08 pm PST
Subject: Re: Olive Branch

[Ray Allis - 921213.1530]

> [gabriel to powers 921213 16:52 CST]

> If you want, let's debate off line, but perhaps just let it drop.

I'd like very much to see this sort of conversation continue. Of course I understand that this may be impossible because such 'tangential' traffic may strain many people's budget for e-mail storage and cost. But I'm interested in the differences in philosophies (as in "How does the world work?") developed by people with differing life experiences, and it seems to me this insight should help illuminate your (our) attempts to build actual real-world models. After all, it's _minds_ that we're trying to understand isn't it?

> Happy Christmas, and much delight in all the good things of life,
> including the grandkids.

Indeed.

Date: Sun Dec 13, 1992 4:58 pm PST
Subject: Harnad's reviews; replies

[From Rick Marken (921213.1000)] Bill Powers (921212.2000) --

>It seems to me that we already have the forum where PCT is
>understood and can be developed. Let's forget about the rest of
>the scientific world and just do our modeling and talk about the
>results here.

>I think that psychology is old and tired, and ready to be bypassed.

Tom Bourbon (921212 22:55) --

> Too bad your fate was in the hands of people with scientific bents --
>they don't have much use for old fashioned fuzzy heads like PCT modelers.
>Maybe we should just accept the fact that they already know everything we
>try to say -- and then some. They just don't talk about it -- ever --
>until they review a PCT manuscript, that is.

Thank you, my friends.

Your counsul is, as always, right on target. I thought that it would at least be possible to start an electronic dialog about the issues in "Blind men" (which you guys dealt with experimentally in "Models..."); I didn't think they would be guarding the gates so to this relatively informal medium so ardently; boy, was I wrong.

So I'll just get back to the balls and the planes (as Bill put it so well in his wonderful post this morning to John Gabriel et al) and forget about talking to the "real"

scientists (of course, Galileo was also a fairly abrasive SOB -- and he got noticed [though not always in ways he would like] -- Gregor Mendel was more polite and look what happened to him).

Nevertheless, in the spirit of Galileo, I will post a testy reply to the reviews here. For those who have read these reviews and who also understand PCT, publishing these replies is a bit like explaining a joke. For those who don't understand PCT and want to, perhaps these replies can help you understand some of the fundemental differences between PCT and other approaches to understanding living systems. (For those of you who don't understand PCT and don't want to, just read the reviews and not my replies -- the latter will just be irritating.

Here goes:

REPORT #1: Tom Zentall (ZENTALL@UKCC.uky.edu)

This is the only reviewer who recommended publication and for all the right reasons:

>This is the kind of paper that asks to be published in an open
>commentary format. It is provocative, relatively simple, and tries to
>be inclusive. Even if others find it flawed in some way that I cannot
>see, I think it will generate interest in the reader. For this reason,
>I would recommend its acceptance.

Hooray for Zentall!!

REPORT #2: ANONYMOUS

>the author NEEDS TO OFFER A MUCH MORE GENERAL AND
>INTELLIGENT TREATMENT OF THEM. I regret that I cannot be more
>positive.

And I regret that I cannot be more intelligent.

>The central thesis of this manuscript appears to be that an animal's
>behaviour is generated in order to keep certain sensory input variables
>at (or at least as near as possible to) preset values (set-points).

By George, I think he's got it!!

>(NO MENTION IS MADE OF OPTIMAL CONTROL
>SYSTEMS, ADAPTIVE CONTROL, MULTIVARIATE CONTROL, AND SO ON WHICH MAKE
>UP THE TOOLBOX OF THE MODERN CONTROL THEORIST).

Nor was any mention made of ANY kind of control system!! But forgetting all that trendy stuff was really a big mistake -- how could I do that?

>In my opinion, the manuscript's central thesis is a rather technical
>way of expressing the "fact" that behaviour is (typically) goal-directed

By George, I think he missed it! This is not even close to the central thesis -- reading the paper might have helped this reviewer. The central thesis was:

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

I think I said that in the paper. My goal was to encourage the opposition to explain why they think that organisms are actually NOT in such a negative feedback situation -- so that what they think they are dealing with -- SR, reinforcement and cognitve behavior -- is what they are actually dealing with. The paper showed that if organisms are in a negative feedback situation with respect to the environment, then behavior is NOT what it APPEARS to be (that's the whole point of the title, fer chrissakes -- an elephant is NOT a snake, wall or rope -- though it might appear to be to a person who cannot see the whole phenomenon -- elephant).

>Consequently, I'm unconvinced that discovering what an
>animal's sensory set-points are is any different from discovering what
>its goals are.

I AGREE -- sensory set points ARE an animal's goals and discovering what these set point are set for (what sensory variables are controlled) is what PCT is all about. That is the methodology I suggested in the paper (if organisms are in a negative feedback situation this is the only kind of research that makes sense) -- the test for controlled variables.

>The latter is none other than a functional analysis of
>behaviour which people have been trying to do for a long time.

Oh, so I should look to the resarch on "functional analysis of behavior" for examples of research based on the test for controlled variables? Thanks for the reference. How could I have missed all that in my training as a psychologist; Gee, and I studied the "functional analysis of behavior" with David Premack and he never told us about testing for controlled variables. Must not have been listening that day -- got an A in the course anyway, though.

> For example, equation
>(1) strongly suggests that the actions which tend to reduce the
>difference between the desired state and the current state are of a
>single type which differ only in magnitude depending on the size of the
>error signal.

I can't parse this too well but apparently this reviewer disdains simplifying assumptions. I guess he thinks that I'm only interested in organisms with a single sensor and a single output variable. No wonder non-linear systems crap is the hot topic -- the goal in behavioral science seems to be to move as far from the simple case as possible -- sort of like Galileo starting his work in physics with a cyclotron.

>Note further that the
>assumption that the functions in equations (1) and (2) are linear is
>ENORMOUSLY RESTRICTIVE; it allows the author to treat k.e, k.f and k.o
>as numbers permitting the derivation of all the other equations which
>appear in the manuscript.

More "simple is bad" stuff. But HOW might that simplifying assumption influence the premise of the paper -- that negative feedback control will LOOK LIKE SR, reinforcement and cognitive behavior? Not a peep. Just too much simplifying. Geez -- don't people take elementary science classes anymore?

>For example, equation (5) cannot be said to establish
>the "behavioural illusion" since it relies on the linearity assumption.

Maybe this is something that could have been discussed if the paper were accepted?? In fact, the "behavioral illusion" does not depend on linearity -- but it certainly works in the linear case. These are the kinds of things that should be debated AFTER the paper appears.

>I failed to understand what the author was getting at in section 3.3.1
>the "stimulus-response" view of control.

Quite true -- as revealed in the next sentences:

> The pupillary reflex, for
>example, was one of the first types of behaviour to be subjected to a
>control theoretic treatment that the author is advocating and this
>treatment is widely accepted - the pupillary reflex is a
>servo-mechanism and similar treatments were offered a very long time
>ago for other reflexes e.g., the muscle stretch reflex - the idea that
>active muscle force, F, is related to the difference between its
>stretched length, x, and its unstretched length x* is described by an
>equation of the form, F = k(x*-x) is basic stuff. The discussion of
>Warren et al. (1986) is very curious - their treatment of the control
>of running is basically control theoretic: they argue that the optic
>variable tau determines the input to the muscles necessary to achieve
>the goal of placing the feet correctly.

This is suitable for framing. The reviewer says that the pupillary reflex and running are understood as control processes. Then, he explains both in SR TERMS!!! F = k(x*-x) is an SR equation (the x* is unstretched length, not a reference force) and tau determins the input to the muscles necessary to achieve running-- S (tau) causes (R) muscle outputs. Gee, he forget that R causes S also --AT THE SAME TIME!!! Control appears (to this reviewer) as an SR process -- the very point I was making in the section he was reviewing. Of course, the point that the reviewer doesn't "get" is that SR IS AN ILLUSION (when there is control). Of course, getting that would mean getting the idea that psychology has got it COMPLETELY wrong about how control (goal oriented behavior) works.

>I think that the three types of "view" that the author discusses
>represent ways of attacking the problem of understanding goal directed
>behaviour

BZZZZT! Completely wrong (and completely consistent with conventional psychological thinking). The point of the paper was that these are APPEARANCES -- noticeable side effects of the process of controlling sensory input. Focusing on these appearances is precisely the way to miss the point (and not do what is necessary -- research aimed at discovering WHAT organisms control and HOW they do it). I said all this in the paper --quite clearly I thought -- maybe a bit too clearly?

>I FAIL TO SEE WHAT THE AUTHOR'S ANALYSIS ADDS IN THE WAY OF
>CLARIFICATION OR METHODOLOGY AND DO NOT SEE IN WHAT WAY IT REPRESENTS
>AN ORIGINAL CONTRIBUTION.

Indeed, he did fail to see. If he had seen, he would have understood that I was suggesting a completely new approach to methodology -- one aimed at the discovery of controlled input variables -- and one that is completely incosistent with current

approaches to the study of behavior. Good job of failing to see -- control in action -- by this reviewer.

REPORT #3 Eliot Shimoff (shimoff@umbc4.umbc.edu)

>Marken's "The Blind Men and the Elephant" seems like an attempt at a
>grand unification theory, showing that S-R relations, reinforcement
>theory, and cognitive psychology are all aspects a single process (a la
>Powers [1978]).

Yep! See, Gary, it looks like your approach would not have worked either.

>A serious attempt at unification must (in my opinion) (a) lead to
>interesting NEW EXPERIMENTS, or (b) force CLARIFICATION of some muddy
>concepts, or (c) make a SURPRISING PREDICTION (e.g., "If this theory is
>correct, you should observe phenomenon X which is not predicted by any
>other system").

We've a) got the new experiments. I b) tried to clarify what might be considered a muddy concept -- at least in conventional psychology -- the concept of control and I c) think I made some surprising predictions -- in a control loop, S doesn't cause R, though it appears to; reinforcement doesn't select behavior, though it appears to and thoughts don't control responses, though they appear to.

>Marken suggests (I think) that the proper task of psychology is to
>determine k.f, k.o, and k.e.

BZZT! Nope, the goal is to discover s* -- I said it in the paper -- but it was buried there in all the english sentences.

REPORT #4: Ed Fasse (edfasse@Athena.MIT.EDU)

>Marken is suggesting that behavior is control.

CORRECT!

>The idea that certain aspects of behavior
>can be understood in terms of control is thus NOT NEW.

This is one of PCT's main problems -- not trendy. The new way to do science seems to be to get the newest trendy methodology (non-linear attractors, neural networks, you know the litany) and apply it -- then it's worth considering -- never mind if if actually accurately accounts for some data; tools are in, observation of phenomena is out.

>In any case the following question
>is interesting: To what extent is behavior control.

Yes, indeed. That would have been something worth discussing in the NOW IMPOSSIBLE discussions in Psycholoquy!

>Those are not Marken's words, but that is the question he is asking
>as I understand it.

Not really. The question is -- what if organisms are in a negative feedback situation with respect to their environment -- what would we see? Well, I'll be darned, we'll see what looks like SR, reinforcement and cognitive behavior -- BUT ISN'T.

>1.1 I think that it is valid to say that the goal of control is to
>produce consistent results, but I DISAGREE that this always takes
>place in the context of an unpredictably changing environment. It is my
>understanding that most computer hard disk drives use stepping motors
>which are well enough behaved that feedback control is unnecessary.

Then it's not control. What a non-sequiter!!

>2.1 The first sentence, "The basic requirement for control is that an
>organism exist in a negative feedback situation with respect to its
>environment" is FALSE. Marken later implies that stability requires
>negative feedback. This is not true in general.

A low gain positive feedback system can be stabilized -- but the loop gain must be so low that there really is no "control" being exerted. I never implied that stability required negative feedback.

Nevertheless, both of these comments are irrelevant to the point of the paper; I mentioned stability briefly just to satisfy those who knew that there were dynnamic considerations that must be satisfied for control. I just meant to say; don't worry; the solutions work dynamically -- so the algebra is a good representation of the functional relationships that actually occur in a negative feedback situation.

< I will now try to skip a lot of stuff which is basically
irrelevant to the point of the paper >

>I am not happy with section 2. Control theory has been applied to
>understanding behavior before, notably in motor control. I do not
>feel that this presentation is particularly adept. It did not teach me
>anything I didn't know before.

Obviously!!

>Again, what I see as interesting is not
>"Can aspects of behavior be understood in terms of control?". Yes, they
>can. It's been done before.

Well, gee, if it's been done before, why isn't anyone worried about the possibility that they are using TONS of grant money, resources, time and energy studying AN ILLUSION!! and wasting testbook space explaining these illusions as saying something important about the nature of behavior?

>What I see as interesting is "Can ALL behavior be understood in terms of
>control?".

Great. We could have discussed that (and formulated research strategies for approaching an answer) if this paper were a target article in Psycholoquy. Now you'll just have to wonder about it all be yourself.

> What does Marken mean to say that the environmental
>variable is seen as a stimulus. How is the human aware of the

>environment except via the sensory information s? Or is d sensory
>information too?

The OBSERVER of the behaving system sees it has a stimulus -- it's the DISTAL STIMULUS to psychologists. That was obvious from context.

>How can we model the process that generates s*?

Good question -- and we have a model that does just that; and it's a working model called HPCT. But the paper was not about modelling. It was about what behavior LOOKS LIKE if in FACT organisms are in a negative feedback SITUATION with respect to their environment; there was no theory in the paper AT ALL. Apparently when people see the word "control" they just assume you are dealing with control theory. Well, this little paper was about the phenomenon of control; this is what psychologists should learn about first -- well, that is, if they want to understand what they are doing; if they just want tenure and fame maybe they'd just better treat it as a theory -- and an old, out of date one at that.

>One cannot say control explains behavior until one explains the generation
>of s*, which affects behavior.

When did I ever claim that control (I assume theory) explains behavior? I said behavior MIGHT be control -- and if it is then one can derive from equation 4 what the relationship between observable variables will look like -- SR, reinforcement and cognitive -- when an organism is controlling.

>"Organisms behave in order
>to keep sensory inputs at these reference values (Powers, 1989)." That
>is debatable, but that's the kind of statement that would be good to
>discuss by the community at large.

Too bad -- there will now be no discussion.

>I feel the theoretical, mathematical analysis of the paper
>is WEAK, and NOT PARTICULARLY ORIGINAL.

Feelings. Oooooh. Feeeeelings. Actually, he's right. Bill P. did basically this analysis in his Psych Review articles. So maybe it's true -- the reviewer might already not understand this weak analysis sufficiently.

REPORT #5: ANONYMOUS

>It reminds me of the sort of off-the-wall discussing we used to do late in
>the evening after studying for comps. There's been so much specific
>research since then, I would have expected such speculations to take a
>new tack.

That's what I get for not going to MIT or Harvard. I missed the discussion where they explained that behavior is control so studies of the external and internal causes of dependent variables reveal little more than well-known laws of the environment.

REPORT #6: Vasant Honavar (honavar@iastate.edu)

>I recommend that the paper be resubmitted for publication in a revised form.

Well, thanks. I suppose I could change all that stuff about SR, reinforcement and cognitive behavior being APPEARANCES; maybe if I explained how important it is to study SR, reinforcement and cognitive behavior as a way to understand control-- bet that would help. I could show how all those great studies I referred to in the paper have contributed to our understanding of control; like that wonderful Warren et al materpiece that shows conclusively that a visual variable (tau) can tell the muscles precisely waht to do to keep people walking upright -- what an incredible variable to discover.

OK. I feel better now. Fear not, I will NOT send this back to Harnad (or the reviewers). I just can't repond to this crap non-sarcastically anymore.

Back to the lab.

Best regards Rick

Date: Sun Dec 13, 1992 5:28 pm PST
Subject: PCT and war

[From Bill Powers (921213.1730)] John Gabriel (921213 16:52 CST) --

I'm not against trying to cope with the world as it is and doing what's necessary to survive, within limits (survival is not necessarily the most important goal, as generations of heroes and martyrs have shown). But when we go to war, I think we should just go to war and do our best to win. Forget the justifications. They're all hogwash. If someone's trying to kill you you try to kill him first. Why try to make more of it than that? As long as we find some nobility in lethal contests, we'll be reluctant to let go of that kind of social interaction. We have to remember that everyone fighting in a war has gone insane. We wouldn't let people run loose in the streets if they acted that way when they had a dispute with the neighbors.

So in answer to your question, if someone tried to harm my new grandson I would do my best to prevent it. I wouldn't stop to theorize. But I wouldn't try to pretty it up afterward. Nor would I then devote all my efforts to erecting an impregnable wall to keep the bad guys away from him -- and him inside.

What I would do afterward is what I am doing now: trying to work toward a real science of human behavior. A real science of human behavior is the ultimate protection against bullets.

>But on lethal force etc. there are interesting issues in animal
>behaviour - see the Altmans on Baboon Society, and Rolf
>Peterson and others on Wolves.

I don't aspire to be either a baboon or a wolf. All violent confrontations, I suspect, are simply a matter of incompatible goals; it's not that we inherit aggression, but simply that we are all control systems. We do what works. If it doesn't work, we try harder or try something else.

>Escalation is always a problem, witness what is happening in the cities.
>Just exactly why I want to have better decision making in Govt AND in Defense.

Yes. Control theory shows why conflicts tend to escalate. It is the nature of conflicting control systems to raise their opposing outputs to the maximum possible level. The only permanent solution is to resolve, not win, the conflicts.

>If you want, let's debate off line, but perhaps just let it drop.

No need for either. Control theory has a lot to say about conflict. But it won't be of much use until we get away from statistical generalizations and start getting real data with which to improve the theory. PCT can be effective if we decide to make it effective. The way to make it effective is to demand that it work every time, and accept nothing less. No more trends and tendencies and indications and probabilities and scenarios and situations. That approach doesn't work worth a hoot. If we're going to apply PCT to the problem of war, let's find out what brings people into conflict and what can get them out of it again. And I mean REALLY find out, not just do a bunch of statistical studies. Give me one one-hundredth of a percent of the military budget and we'll get started. This is a solvable problem. If anybody really wants to solve it.

A Happy Christmas back to you, and even happier ones ahead.

Best. Bill P.

Date: Sun Dec 13, 1992 7:23 pm PST
Subject: Gangs of 1

|Bill Cunningham 921213.2030|

The gang of 3/5, just one minute, olive branch discussion motivates this gang of 1 to throw in his own comments. You may or may not here from Martin as another gang of 1 when his e-mail system is fixed. I just logged on in a brief stop between trips and find returned mail from down system. He's perfectly capable of speaking for himself. Don't misinterpret any short term silence on his part (or mine either, since I leave in morning).

Let me throw the brick first: I make a major distinction between militarism-- which I regard to be both stupid and dangerous, and the military--which has some very bright people (these days), does some stupid things collectively (but no more so than other groups of humans (eg psychologists who review papers) and who has to live with danger (usually provided by the militarist). So, Bill P, I'm offended when you paint anybody/thing who works with/for the military with the same tar brush. More important, that kind of labeling does more to perpetuate problems than to solve them.

For the most part, the military are just like real humans. They have some cultural foibles and tribal jealousies that are important to them and absurd to an outsider. So does every large organization of individual control systems--nations, religions, you name it. For both good and ill, man is a social (or antisocial) animal. The only escape short of becoming an agnostic hermit is either catatonia or death. So we have a need for police, clinical psychologists, and even the military.

There, I feel better.

Now, on the brighter side. The process of boiling down a plethora of noisy data into something intelligible, and the process of making wise decisions based on the "right info" apply across our modern society. Every attempt I've ever seen to drain these

swamps have come from the purveyors of fine swamp water. So we are off on a noble quest. The military environment happens to be a very good place to work on the problem, since the issues are pretty clear and the grownups are willing to seek answers.

To end on the brightest note, the complaints of the math being too complicated are absolutely valid. Thank you, thank you, thank you. As I remarked before, in my dealings with John, it don't exist if I can't understand it. This I could understand. Step two is that it doesn't pass the so what test unless I can explain it on a non mathematical basis. What you saw was an extract of my struggling with that. So we ain't there yet. We still have a major unexplained problem to deal with. But we wouldn't be this far without PCT. Nor would the other folks on the net. So we are grateful for that.

I'll be out of town for 3 days. Hope to return and find all the world's problems solved, including the one we are groping at.

Warm regards (really) and affection.

Bill C.

Date: Mon Dec 14, 1992 5:04 am PST
Subject: Error; Brains; Lesions

From: Tom Bourbon (921214 01:30 CST)

IMPORT THIS DOCUMENT WITH HARD CARRIAGE RETURNS INCLUDED

This is a discussion of simulations I ran to check my understanding of issues raised in several recent threads on CSG-L: control systems control error; PCT must "look at the brain" (i.e., neuroscience) or die; and deficits in reaching as sequelae of brain lesions pose a challenge to PCT. I used the simulations to clarify my thinking on those issues taken together, not separately. I do not refer to specific posts by the various people who contributed to those threads. I cannot post graphic output of actual results of the simulations, each of which includes 1800 data points for each variable, but I include a stylized rendering in "ASCII graphics" that in no way misrepresents the results.

In the simulations I used an old (circa 1985) program in which a single PCT loop models a person who uses a control handle (H) to keep a cursor (C) and target (T) in a preselected relationship on a computer screen, the kind of performance PCT modelers refer to as intentional or purposive action by the person. Although I fully comprehend the logic and algebra of the relationships, as they were posted by Rick Marken, for example, I am more comfortable when I see them run in simulation. In all of the simulations I describe here, the reference signal calls for the cursor to remain even with the target which moves uniformly up and down on the screen tracing a triangular wave of vertical position vs time. In the program, ref. sig. = [C - T = 0].

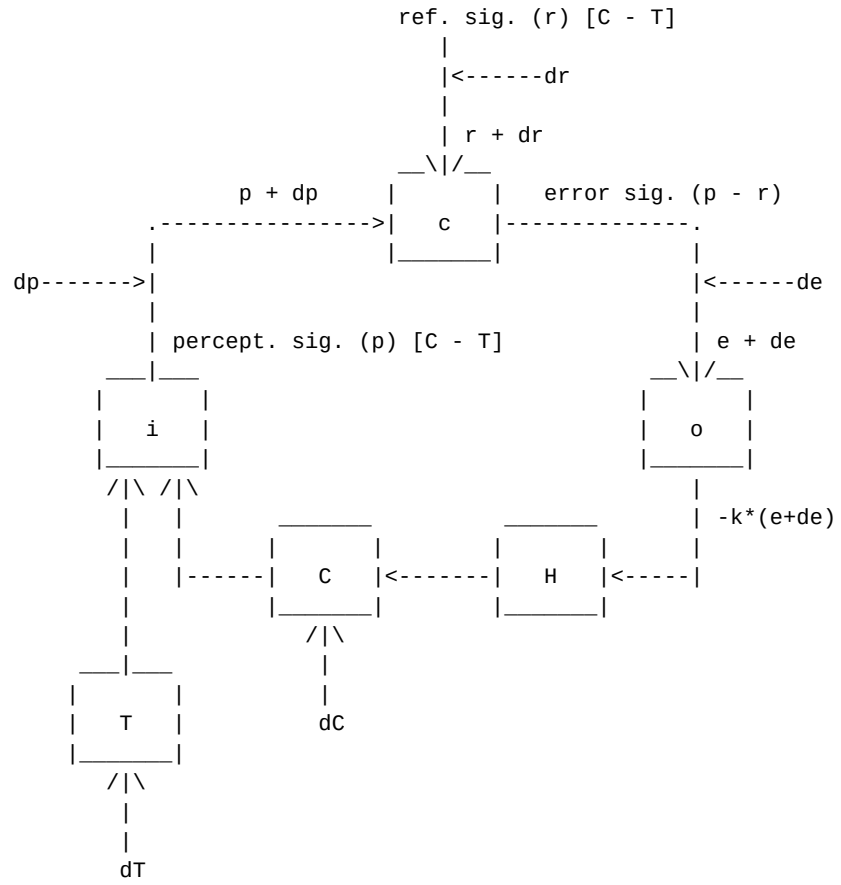
I modified the old program so that half way through any given run, I could introduce a disturbance (d) directly to one of the three signals in the model: dr for the reference signal, dp for the perceptual signal, or de for the error signal. At the beginning of all simulations, each d was set to 0. During a given simulation, half way through the run, one d assumed a value of +3.

Figure 1 (in ASCII, margins 1", hard returns included) is a diagram of connections in the model and its simulated environment. All coefficients are assumed to be 1, except

for the "integration factor," k, which was set to a value estimated from one of my actual runs on the tracking task. (Incidentally, the unaltered model had recreated my run at the familiar correlation > .99) The person is modeled as a single control loop, controlling the perceived spatial relationship between C and T.

****

Figure 1: A person modeled as a relationship-level control system, using a single control handle (H) to affect the position of a single cursor (C) relative to a target (T) during pursuit tracking. In the model are three functions (c = comparator, i = input function, o = output function) and three signals (r = reference signal, p = perceptual signal, e = error signal). Also in the model are three possible disturbances, one for each signal (dr = disturbance to r, de = disturbance to e, dp = disturbance to p). Each d adds to its respective signal and the sum conducts downstream to the next function. In the simulated data that accompany this text, all three disturbances are initialized to 0. In the environment are two independent disturbances, one acting on the cursor (dC), one on the target (dT). For the present examples, dC = 0, and dT is a triangular function of time shown in Figure 2.



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In the computer program (written in Turbo Pascal 3.01) the following two steps implement the unmodified model used in the first half of each simulation:

```
H := H - [k * (p - r)]
C := H + dC

where

p := C - T
r := [(C - T) = 0]
e := p - r.
```

Half way through the simulation, a disturbance is added to one of the signals as a constant. The d remains in effect through the remainder of the run. The following program steps implemented the disturbances:

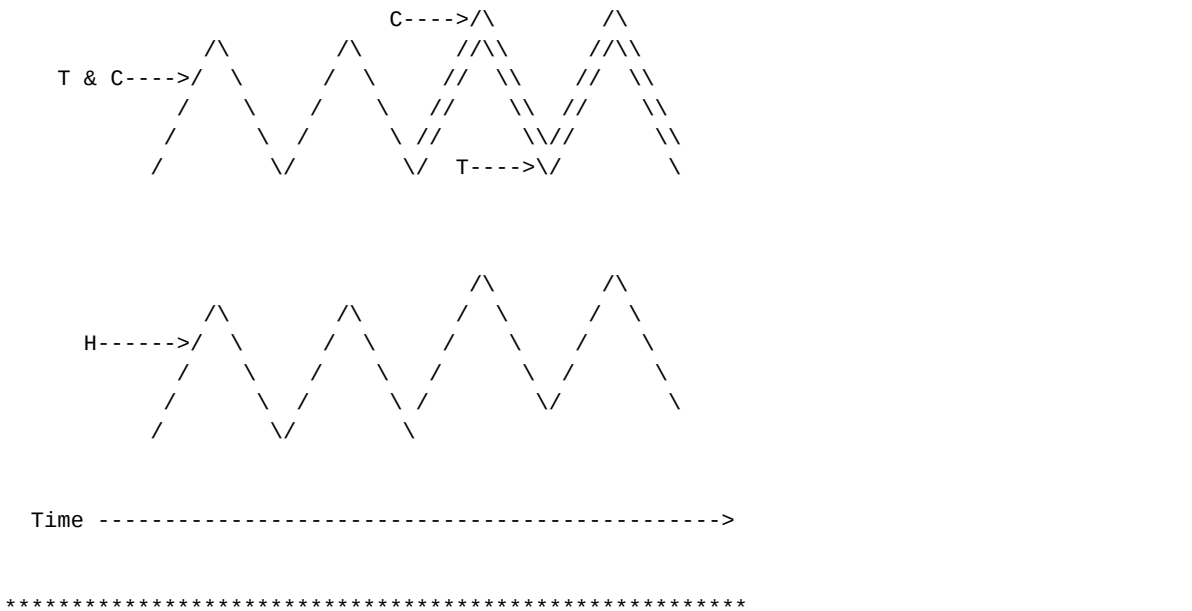
For the reference signal: r := r + dr.
For the error signal: e := e + de.
For the perceptual signal: p := p + dp.

In every case, d = + 3.

CASE 1: Disturbance to the reference signal. The stylized results are shown in Figure 2.

****

Figure 2. Stylized ASCII representation of simulation of pursuit tracking by a single PCT model when the reference signal was disturbed during the second half of the simulation. The top half of the figure shows vertical positions of the target (T) and cursor (C) on the computer screen at successive times (up = toward the top of the screen); the bottom half, displacements of the control handle (H) at successive times (up = away from the simulated person). During the first half of the simulation, C = T; during the second half, C = T +3, or C - T = +3. The position of C relative to T is explained in the accompanying text.



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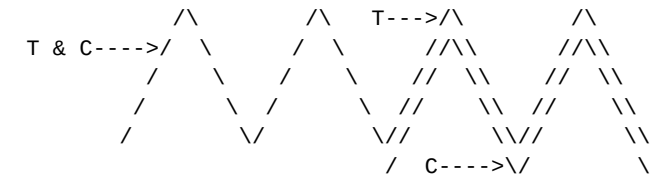
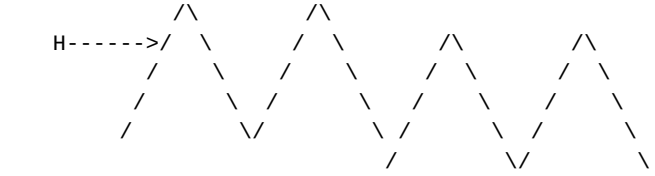
For the first half of the simulation, r = [(C - T) = 0], and the model maintained that relationship. At mid-run, dr = +3. The effective reference signal at the comparator became (C - T = +3). The model achieved that result -- the handle shifted to a range of movement centered three equivalent screen units above the middle of its range and the cursor moved to and remained at a position three screen units above the moving target: C = T + 3, or C - T = +3.

CASE 2: Disturbance to the error signal. The stylized results are shown in Figure 3.

****

Figure 3. Stylized ASCII representation of simulations of Case 2 and Case 3, pursuit tracking by a single PCT model when either the error signal, or the perceptual signal, respectively, was disturbed during the second half of the simulation. The top half of the figure shows vertical positions of the target (T) and cursor (C) on the computer screen at successive times (up = toward the top of the screen); the bottom half, displacements of the control handle (H) at successive times (up = away from the simulated person). During the first half of the simulation, C = T; during the second half, C = T - 3. The position of C relative to T is explained in the accompanying text.





Time ---------->

****

With de = +3, the effective error signal is e + 3. When the product of k*(e + de) is subtracted from the former position of the handle, the handle moves three units lower in its range of movement and the cursor is at C = T - 3, or C - T = -3. The perceptual signal, p, becomes -3. (This model treats the entire person as a relationship controller, so the output of the input function is the perceived relationship, [C - T].) The reference signal remains [C - T = 0], so coming out of the comparator, the error signal is, p - r = -3 - 0, or, e = -3. When de is added to e, farther down stream, the effective error signal into the output function becomes: e + de = -3 + 3 = 0, and the

system no longer changes the relationship between C and T. In this case, e + de has become a virtual reference signal that leads to exactly the same results in the ENVIRONMENT as would be produced by changing the reference signal to [C - T = -3], an effect that would also be produced by disturbing the reference signal with dr = -3. However, with regard to the perceptual signal INSIDE the model, dr and de lead to different results. When dr is applied, p = r; when de is applied, p = r -de, and the perceived relationship [C - T] is NOT as specified in the reference signal. More on this later.

CASE 3: Disturbance to the perceptual signal. The stylized results are also shown in Figure 3.

The effective perceptual signal into the comparator is p + 3, therefore, e = p - r = +3 - 0 = +3. When the product of k*e is subtracted from the former position of the handle, the handle moves three units lower in its range and the cursor is at C = T - 3, or C - T = -3. Coming out of the input function, the perceptual signal, p, becomes -3. The disturbed perceptual signal is: p + dp = -3 + 3 = 0. Now p = r, or (p - r = 0), the relationship specified in the reference signal. The system has eliminated the effect of dp on p, going into the comparator, but the relationship between C and T on the screen is not the same as it would be were there no disturbance on the perceptual signal.

A BRIEF DISCUSSION

I knew the three disturbances should have DIFFERENT effects on signals and their relations INSIDE the model, but I did not expect that all three would have either SIMILAR or IDENTICAL effects on variables OUTSIDE the control system. An observer, builder, or user of control systems could easily observe results in the environment and conclude there were no differences between the "treatments" or the effects of the three disturbances. Inside the model, that is not true.

Another surprise was that, if de = +3, then p - r = -3. The system never brings the perceived relationship to the value specified in the "real" reference signal. Would that happen in anything but a single-loop system? Andy Papanicolaou and I discussed this result and concluded that any system that performed this way with regard to any critical variable would not survive. In hierarchical systems with a level above the relationship level, changes should ensue -- in the error signal from above that sets the reference signal for relationships; or in the output gain of the system (k), perhaps via a reorganizing loop that randomly tinkers with gain. There may be other possibilities.

As for whether PCT must "look at the brain" (i.e., neuroscience) or die, the reverse seems true. Simulations like the ones here ought to provide grist for neurophysiologists. For example, what would be the results if someone were to study a "simple" laboratory preparation, with Aplysia the marine snail for example, while at least provisionally adopting the idea that the creature controls the sensed states of certain variables. In such a preparation, after the experimenter confirmed that the creature controls ANY variables, all of the disturbances I described here could be applied to precisely-mapped neurons and the results observed. With the results of the PCT simulations as exact quantitative predictions, the PCT model would be subjected to the most rigorous of tests, and neuroscience might benefit from a model intended to explain how organisms purposefully create and maintain perceptions.

On the question of whether deficits in reaching, as sequelae of brain lesions, pose a challenge to PCT, I will depart from my stated plan of not mentioning sources from the net. Gary Cziko raised the question in a brief account of an interesting clinical case, and Mark Olson rushed to offer the example as evidence that PCT must "look at the brain."

I think the reverse is true; brain science must look at PCT. In my simulations, all three disturbances led to the cursor missing the target "by a little bit:" In one case, it went beyond the target; in two others, it "fell just short," a result that sounds a bit like the clinical report Gary cited. The clinical "deficit" seemed to have something to do with "mistakes" in controlling relationships. Did anyone ask the man if things looked alright when (outwardly -- where clinicians and other observers reside) he ended up in the wrong place? Wouldn't it be interesting if he said, "yes!"

Did the man's lesions alter or disturb a reference signal, making it negative compared to what it was before? Did the lesions add positive disturbance to an error signal or a perceptual signal? Did the lesions destroy one of the functions in a control system, or modify its workings, or disturb or disrupt a path carrying one of the signals in a control system? Knowing that a lesion was in some general part of the brain, and that outward appearances of the person's behavior changed, tells us nothing specific about the reasons for the changes; but the process of modeling and simulating the control of relationships raises some possible explanations that, to my knowledge, neuroscientists and neuropsychologists have not considered. Until research and theory in the neurosciences catch up with the science of control by living systems, "looking at the brain," or looking to the neurosciences, for evidence that will change the nature of the model in PCT, probably will not be necessary or fruitful.

Until later,

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Date: Mon Dec 14, 1992 5:45 am PST
Subject: CLOSED LOOP

From Greg Williams (921214)

I'd be happy to put both the Models/Worlds and Blind Men papers in CLOSED LOOP if that's what the authors want. I think they'll both probably fit in the next issue (due out 1-15-93). Tom, Bill, and Rick, is that OK? I'll put in a little blurb saying that they aren't exactly "threads," but worthy of publication -- and enabling the heading toward a broader JOURNAL OF LIVING CONTROL SYSTEMS. If we need to have a few more pages than usual, that will be OK, I think. And Ed Ford can have extra copies made for sending out by the authors.

Let me know what you think.

Greg

P.S. "One whole minute" ain't so great. I can reliably predict that my son Evan will watch "Indiana Jones: The Last Crusade" for nearly two hours any time he says he wants to watch it and he starts up the VCR. :-))

Date: Mon Dec 14, 1992 10:28 am PST
Subject: RE: Teaching PCT

From Tom Bourbon (921214 08:58)
I taught PCT (orginally CST) from 1973 until a few months ago, to undregraduates (starting at the first-year level) and master's-level graduate students. I will try to organize some of my ideas on that experience and will post them later. (And they were undergraduates.)

Tom Bourbon

Date: Mon Dec 14, 1992 3:16 pm PST
Subject: tom' post

Ok, I just read Tom's long post and realize that there is a misunderstanding about what I am saying. Did I say that the clinical report was a CHALLENGE to PCT? If so, it is not what I meant. I meant that here is a perfect opportunity for PCT to get involved--I knew when I first saw the videotape of the patient that it was a problem at the Relationship level--just like Tom said. So....since we believe it is a Relationship problem and we know that there is a bilateral lesion to the superior parietal area, then lets start mapping our hierarchy. We cannot map the hierarchy without neuroscience data--that seems very clear--how else will you know where some function occurs unless you know about what happens when areas are disabled (or brain scan data)?

I don't think we disagree very much --I think we agree alot. My only disagreement is to think that you can do it ONLY topo-down. You gotta have both. The bottom limits the top ad the top inform the bottom.

I'd write alot more but I should have quit an hour ago.
MArk

....then you've got something related to memory, no matter how you describe it. Hubel and Wiesel (sp?) found some pretty interesting (although incomplete) stufff about the organization of the occipital cortex and probably never even heard of PCT. Do you want to claim that they are wronb because they have the wrong paradigm? We know alot of visual processing occurs in occipital. We know that attention mechanisms are involved in at least prefrontal areas. We know that the hippocampus is involved in at least one form of memory. WE didn't need PCT to find that out, because we are working at a levelof analysis below PCT. Sure PCT could help inform the processs but it isn't as necessary as it would be for levels above PCT--the rest of psychology.

Making good models occurs from going both bottom-up and top-down. I think its a mistake to think we can come up with a hierarchy without looking at what the brain is doing. The sorts of processes that can be disassociated via lesions I think is "weirder" than anything we would imagine without such research. Ya gotta have both.

Mark

Date: Mon Dec 14, 1992 3:08 pm PST
Subject: neuroscience

I have made 4 attempts to mail the first part of the message you received and am way too frustrated to try again. The basic jist of the message is that although I agree with Tom's commments, the negative case is overstated. Good research can be done in neuroscience without any knowlege of PCT. This is not true for psych in general. But because PCT is a computational model, at a higher level of anlaysis than neuropsych

stuff, this is true. I'd say more but my patience is out--I've written 4 long responses now--Mark

Date: Mon Dec 14, 1992 3:17 pm PST
Subject: Re: One Whole Minute

[Gabriel to Powers 921214 10:28]

Bill, I'm reposting your msg in full because I have readers not on CSGNET. Thanks for your reply, mine follows lower down the page(s); I think the discussion is very worthwhile.

>[From Bill Powers (921213.0700)] >Bill P.

Bill, I read, and I think I understand you. Put very simply, you are saying that a human being can follow a random movement of a cursor with a finger, and that this is explained by control theory.

The gang of three are interested in the reasons why people running software projects don't seem to be able to follow what is, at least to the gang of three, very obvious behaviour of a software project that tells an observer "It's not under control" in the very exact detailed sense of your book, and why the managers can't close the feedback loop.

If we take your premises, and the view that if we scale ALL the time constants by three or four orders of magnitude the control problem doen't change, your and Tom Bourbon's achievements of explaining control for a minute in hand movement OUGHT to be applicable to managing software projects - the time scales do go from milliseconds to seconds, for fast actuators, and from seconds to hours for slow ones.

Why then are almost all software projects so badly screwed up, and why are a very few great successes. Having been privileged to participate in both successes and failures, having put some five years of my life into what turned out to be a very instructive failure for which I was largely responsible, and seeing others make the same mistakes as I did, I'd like to alleviate some of the frustration and waste I see out there in softwareland.

I`d like to leave the answer on behalf of the gang of five to one of our members who has more combat experience than I do. I think there are a number of possible candidates, and one of them will step forward onto the stage if we wait, and give them a chance to plan before action. About the mathematics. It's really just the way I think. It's only deep to those who haven't happened to read the dictionary and grammar for the lingo. But as Avery might tell us, we can't neglect to consider that fact. And for me at least, it's both terrifying and wonderful that a topologogist can't tell the difference between a donut and a coffee cup. Terrifying because such abstraction misapplied leads to some of the mistakes the Kennedy Camelot made. Wonderful because the ability to abstract like that can lead to great discoveries about things that are similar, but where the similarity is hidden by a host of details. And that's just as true for Priestly's discovery of oxygen, as it is for today's high energy physics, which is where the mathematics I use to leverage insights comes from.

I don't think we are really as far apart on that issue as may seem to be the case. But we all act on our perceptions.

Finally I want to reverse myself. I think the debate is proper for the public forum, and I'm honoured to participate. Thank you for your patience and careful thought. There's a quote some place about all travelling to the same ultimate destination. I'm proud to be among your company if you will accept me there.

Very Sincerely and in Friendship

John (gabriel@athens.eid.anl.gov)

Date: Mon Dec 14, 1992 3:51 pm PST
Subject: Re: Drowning in Data

>From: "Bill Cunningham, ATCD-G (8" <CUNNINGB@MONROE-EMH1.ARMY.MIL>

>XXXXXXXXXX sez brigade commander can't make decisions fast enough relative
>to the data rate, screened for his definition of relevance. Recognize that
>they are giving brigade access to every sensor in the Western world.
>Commander's screening guidance is temporal/geographic and therefore it's
>all relevant and he's drowning on input he never dreamed of. My interpretation
>goes right back to e-mail report where commander hadn't articulated intent
>and decision tree adequately, so he's given the wrong criteria for relevance.

>Gee, where have we heard this before?

>Bill C.

The gang of three see it every day in softwqreland

Date: Mon Dec 14, 1992 3:53 pm PST
Subject: Error; Brains; Lesions

[From Rick Marken (921214.0900)] Tom Bourbon (921214 01:30 CST) --

>Until research and theory in the neurosciences
>catch up with the science of control by living systems, "looking at
>the brain," or looking to the neurosciences, for evidence that will
>change the nature of the model in PCT, probably will not be
>necessary or fruitful

Wonderful post Tom. Thanks for doing all the nice work and recognizing its implications for behavioral neuroscience.

This nice little post of yours is just another great example of why PCT (on network CSG-L) is the place to be. The simulations helped us see what is not clear from the algebra: when you disturb p, p is still brought to r but the external variable is off by the amount dp. This is a good way of showing that what is controlled is perception (not "reality"). It also shows how a person can seem to be doing something "wrong" (from the observer's perspective) when s/he is doing something "right" (keeping the perception at its reference) from his/her own perspective.

Of course, all of this would never be of interest to "real" living systems scientists because it's just too simple (where are those Nth order partial differential equations,

Tom?). But for simple minded folk like me it was dynamite. Again, thanks for doing the work, Tom. Great post.

Best Rick

Date: Mon Dec 14, 1992 5:13 pm PST
Subject: Zentall's a mensch

[From Rick Marken (921214.1000)]

In the spirit of putting everything related to PCT on the net, I am taking the liberty of posting my reply to Tom Zentall -- the ONE reviewer (out of six -- seven really, counting Harnad) who recommended acceptance of my paper. Tom sent me a note this morning expressing his regret about the rejection. Here's my reply:

Tom

>Sorry to learn about the fate of your submission to Psycoloquy. My feeling
>was that your submission involved just the type of provocative theory that
>this journal was meant to disseminate.

How sweet of you! Thank you very much. As you know, I completely agree with your suggestion -- I know that the ideas in the paper are provovcative. I though it would be nice to discuss them in a public forum with people who might be interested. Thus, I was quite disappointed with the rejection.

Luckily, I am not in a position where I must publish or perish. I was a psychology professor at one time, tenured long ago; now I am working as a human factors/systems engineer. But I am still very much interested in fundemental issues in psychology -- I don't see how one can do human factors type work without some useful conception of human nature -- ie. a theory.

I don't understand why (most) psychologists don't want to discuss the issues raised in my paper. I believe that the reviewers missed the point of the paper, by the way. I was not proposing a theory of behavior, really. My point was:

"If organisms are in a negative feedback situation with respect to the environment (as described by simultaneous equations 1 and 2) then their behavior will APPEAR to be s-r, s-r-rnf or cognitive (depending on the circumstances under which it is observed) -- when, in fact, it is NOT; control is neither s-r, s-r-rnf or cognitive behavior -- it is controlled perceptual input."

This is what I wanted to discuss. Obviously it's a debatable proposal -- but only if people are willing to debate it. None of the reviewers spoke to this point at all; the paper was rejected simply because the ideas were too simple (though not so simple that they were understood by the reviewers), not intelligently developed (I don't know how to be more intelligent) and "old hat" (though I have never seen these implications of control discussed by anyone other than Powers (in one Psych Review paper in 1978) -- and I have never seen Powers' ideas debated in any detail).

Anyway, I might respond to Harnad about this but my impression (based on previous experience with attempts to publish in B&BS) is that Stevan has no interest in seeing a public discussion of the implications of feedback control for psychological science -- he figures psychology is WAY past that.

Thanks again for the note -- I'm still waiting for your reprints. And I also encourage you again to join CSG-L -- apparently the only place where people can discuss the implications of the nature of control for understanding living systems.

Best Rick

Date: Mon Dec 14, 1992 6:07 pm PST
Subject: GANGWARS

RE: CONVERSATIONS BETWEEN POWERS, CUNNINGHAM & GABRIEL

I generally grazed through the traffic rather than rolling into each bail, because I am generally too tucked into the quagmire on my plate to do anything else. This time, however, I feel compelled to respond to the issues raised by Bill Powers.

I certainly don't understand all the math that John Gabriel uses to try and capture the essence of our innumerable offline conversations. I do find the effort worth the risk of oversimplification simply because of the insights into observed phenomena that the exercise has given me.

My interest in what the Gang of 3/5 is (are?) up to is very narrow. I have no desire to try and solve any universal questions; although I have enough intellectual curiosity to appreciate the lure of doing so. My interest in PCT is merely to acquire another useful way of dissecting problems. I think PCT can be used to organize one's thinking about human interaction, even if the specifications of the complete model are, as yet, unknown. One must only make sure that there sufficient "tooth tests" to guard against placing absolute trust in incomplete understanding.

I would never let whatever insight I might gain from applying PCT to the questions of field comand get in the way of better instincts and judgement. I would, however, use whatever insight I could to help me get a firmer grasp on the size and shape of the particular struggle that I face. The reason that I would rely on incomplete understanding and knowledge is quite simple - there is nothing better, and SOME insight may save the lives of my comrades and my enemy.

I am one of those bastards that you call on when your national interests (as expressed by your President), or your corporate interests (as expressed by your Board), or your personal interests (as expressed by your complaint against your fellow) get threatened or abused. As military officer, economist/consultant, or lawyer, I get to deal with the trash that others create. I will use whatever paradigm, model, augury, or insight that helps me deal with that trash with the very least cost to all concerned. One need only kill, or break a man's spirit and see the consequences, to know that total victory should be left only to those who think they ARE gods - not merely that they talk to God.

You will continue to pull on my coat, Bill Powers, and remind me that my understanding is inexact and incomplete. But you can't keep me from picking up and using whatever tools I find useful - even the one however crudely fashioned by you.

Date: Mon Dec 14, 1992 6:15 pm PST
Subject: Re: PCT and war

[gabriel to powers 921214 11:35 CST] >[From Bill Powers (921213.1730)]

>John Gabriel (921213 16:52 CST) --

>I'm not against trying to cope with the world as it is and doing
>what's necessary to survive, within limits (survival is not
>necessarily the most important goal, as generations of heros and
>martyrs have shown). But when we go to war, I think we should
>just go to war and do our best to win. Forget the justifications.
>They're all hogwash. If someone's trying to kill you you try to
>kill him first. Why try to make more of it than that? As long as
>we find some nobility in lethal contests, we'll be reluctant to
>let go of that kind of social interaction. We have to remember
>that everyone fighting in a war has gone insane. We wouldn't let
>people run loose in the streets if they acted that way when they
>had a dispute with the neighbors.

Absolutely.

>So in answer to your question, if someone tried to harm my new
>grandson I would do my best to prevent it. I wouldn't stop to
>theorize. But I wouldn't try to pretty it up afterward. Nor would
>I then devote all my efforts to erecting an impregnable wall to
>keep the bad guys away from him -- and him inside.

Right On. And if you build a wall, you keep good things out as well as bad. Had several friends killed and hurt mountaineering, and they were not about to give up on account of the risks. I still sometimes ride a (very fast) motorcycle. But I'd be a damn fool to go out without my helmet any time, and without leathers if I'm going fast of far.

>What I would do afterward is what I am doing now: trying to work
>toward a real science of human behavior. A real science of human
>behavior is the ultimate protection against bullets.

Think on that. And Heinlen's remark that an armed society is a polite society, also on Roy Chapman's rule "Never point a gun at anything/one unless you are willing to take personal responsibility for its' their destruction." Don't really understand personal responsibility like I understand control theory, and perhaps nobody else does, but I know it's important. Don't read me as a driving a pickup truck with an AR-15 in the rack behind the seat either - I know you won't, even though I have an AR and a Garand, as well as an SMLE.

>>But on lethal force etc. there are interesting issues in animal
>>behaviour - see the Altmans on Baboon Society, and Rolf
>>Peterson and others on Wolves.

I can't tell if you've read Peterson and the Altmans. The thing that makes them interesting to me is the way the society where the carnivores can do real harm to each other, but don't because if they do they will be too torn up to catch the next moose, moves gradually towards the chicken run where nobody has claws and teeth, and it's fatal to be crowded and at the bottom of the pecking order.

>I don't aspire to be either a baboon or a wolf. All violent
>confrontations, I suspect, are simply a matter of incompatible
>goals; it's not that we inherit aggression, but simply that we
>are all control systems. We do what works. If it doesn't work, we

>try harder or try something else.

>>Escalation is always a problem, witness what is happening in
>>the cities. Just exactly why I want to have better decision
>>making in Govt AND in Defense.

>Yes. Control theory shows why conflicts tend to escalate. It is
>the nature of conflicting control systems to raise their opposing
>outputs to the maximum possible level. The only permanent
>solution is to resolve, not win, the conflicts.

Perhaps. In many ways certainly. Let me say one thing that may bring down a firestorm. Before the railroad and the telegraph and good maps (Gauss's contribution) by and large wars had a not unreasonable ecological function - they closed down incompetent governments. Problem since 1870. How to close down an incompetent government without unacceptable cost in lives, misery, ... Trade wars perhaps. Better to be laid off than killed or badly mangled.

>>If you want, let's debate off line, but perhaps just let it drop.

>No need for either. Control theory has a lot to say about
>conflict. But it won't be of much use until we get away from
>statistical generalizations and start getting real data with
>which to improve the theory. PCT can be effective if we decide to
>make it effective. The way to make it effective is to demand that
>it work every time, and accept nothing less. No more trends
>and tendencies and indications and probabilities and scenarios and
>situations. That approach doesn't work worth a hoot.

Right on. See my comment below about those in the advertising business who have learned how to apply PCT.

>If we're
>going to apply PCT to the problem of war, let's find out what
>brings people into conflict and what can get them out of it
>again. And I mean REALLY find out, not just do a bunch of
>statistical studies. Give me one one-hundredth of a percent of
>the military budget and we'll get started. This is a solvable
>problem. If anybody really wants to solve it.

Beautiful post. Perhaps we can do some of each. For the folk on both sides getting shot at in Somalia some kind of better IFF would help. Had a neighbour say yesterday "How can a few bullets damage a helicopter?" I bit my tongue, but was tempted to explain how one bullet in a critical hydraulic line could ground the bird and get its occupants shot. Very ill conditioned equations of motion - For the want of a nail .... Is a central issue in war and in software. A small mistake early on if not recognised and fixed leads to catastrophe because of large investement in bad strategy. Does PCT have guaranteed fail safe sure fire solution? WELLLLL.... Easy to recognise and correct small error of hand position in picking up glass of water. How about error of judgement in intial grip for a bout of arm wrestling? HMMMM. How do you learn to spot those small mistakes soon enough without millennia of Darwin backing you up? Increasingly critical problem when communication works faster, and those who have been applying PCT in advertising agencies have a substantial hand in deciding who gets to be president and C in C. Perhaps it was better when it took three months to go from Oregon to Washington to kick butt of elected rep.

Completely agreed - how do we keep people from wanting to shoot at each other? No, I don't want to say that - don't believe in police/ baboon state. Why do people want to shoot at each other, and WHAT might make them want to stop? There, that doesn't sound like an ad for HCI and Sarah Brady. Law and Order aint as simple as its' advocates on Right or in HCI would have us believe. This'll probably bring down another firestorm - don't own a TV, believe it rots the brain cells. I still grieve for the London Times of 1938-1948 in spite of the Astors (we all packed up and went to NZ in 1947 and things have been going downhill in London ever since).

Sincerely Best John (gabriel@eid.anl.gov)

Date: Mon Dec 14, 1992 6:55 pm PST
Subject: CLOSED LOOP

[From Rick Marken (911214.1200)] Greg Williams (921214) --

>I'd be happy to put both the Models/Worlds and Blind Men papers in CLOSED
>LOOP if that's what the authors want. I think they'll both probably fit in
>the next issue (due out 1-15-93). Tom, Bill, and Rick, is that OK?

Sounds great to me!

If Bill and Tom bite, I'll e-mail you the latest copy of "Blind men" -- ie. the one that was rejected.

Thanks Rick

Date: Mon Dec 14, 1992 8:20 pm PST
Subject: Neurological research

[From Bill Powers (921214.1045)] Mark Olson (921214.1010) --

You may have less frustration in sending if you set your right margin to 65 and be sure you have a hard carriage return at the end of each line. If you send a long string without a return you can overrun a buffer in your mainframe, which will cause the transmission to be rejected. Remember, mainframe software was not made to serve people; people were made to serve mainframe software.

Your point about using information about the brain is a good one. I did a good deal of that in writing BCP. In fact when I went to work at the VA Research Hospital in Chicago, I was full of ambitions, with the Northwestern University Medical School (and library) right across the street. I thought good, I can just go through all the neurological literature and look up what the various parts of the brain do, and build the model around that. I was soon disillusioned. Maybe the information was there somewhere, but if so it was like a card index that someone had dumped on the floor, stirred, and put back at random. You'll find some neurological references in BCP, but not nearly as many of them as I had hoped to accumulate. I did make a note about Hubel and Weisel. Basically, however, my ambitious neurological project was a bust.

As you say, brain researchers have found a good deal of interesting material without using PCT, more in recent years with improvements of technology. The problem is not with

the technology, however. It's with the concepts of behavior against which neurological findings are compared.

When you stop to think about it, neurological findings are ALWAYS based on SOME theory of behavior. Without any theory of behavior, all you have is a record of lesions in various parts of the brain and some recordings of spikes and potentials from electrodes in anatomically, but not functionally, known areas. You see the theory of behavior not in the findings about the brain, but in the descriptions of external correlates of brain activity.

This problem has not escaped brain researchers. Mary brought home a book on lesion research recently: Damasio, H. & Damasio, A. R.; _Lesion Analysis in Neurophysiology_ (New York: Oxford University Press,1989). In the introduction, D&D say

> "Naturally, the lesion method can only be as good as the finest level of cognitive characterization and anatomical resolution it uses. In other words, the method's yield is limited by:
>
> 1. The sophistication of the neurophysiological testing or experimentation with which anatomical lesions are correlated.
> 2. The sophistication of the theoretical constructs and hypotheses being tested by the lesion probes.
> 3. The degree of sophistication with which the nervous tissue is conceptualized ...
> 4. The anatomical resolution of the methods used." (p. 9)

From a modeler's point of view, the sophistication of neurophysiological testing and experimentation is not very high. In fact, evaluations of what is wrong with the behavior of a person with a brain lesion tends to rely on subjective and rather crude classification of symptoms rather than models of brain function.

A large part of testing for neurological deficits consists of presenting stimuli to patients and recording how they respond. If you hold up a card with a picture, or point to an object, can the subject utter its name? If you tell the subject to point to your finger, then the subject's own nose, then the finger again and so forth, do the movements seem normal, and are the end-points accurately located? Do the movements seem retarded or uneven? Are there tremors or oscillations? Can the patient repeat back a spoken sentence, read a written sentence, follow spoken or written directions to do something? Can the subject name colors correctly, in all parts of the visual field? Can the patient speak the correct name of a seen person? Can the patient sing when so commanded? Is reading speed impaired? Is grammar correct? Can the patient understand and/or generate sentences of normal length? In general, is the response competent and appropriate to the stimulus?

And so on and so on. What's going on is nothing more than an informal assessment of superficial aspects of behavior to see if the patient can do all the things that normal people do, and in the familiar way. An atmosphere of formality is generated by using Latin terms -- alexia for inability to read, prosopagnosia for inability to recognize persons, constructional apraxia for inability to make well-formed sentences (I think). If you strip away the Latin, what you have left is just a subjective description of what the person can't accomplish that normal people can. The main thing the Latin terms do is to allow you to say that the person "has" alexia, "has" prosopagnosia, etc.

These deficits give some clues as to what various parts of the brain accomplish. But they don't tell us what those parts of the brain DO -- that is, what functions are carried out in these particular networks that normally result in the externally visible consequences that we call reading, pointing, naming, and so forth.

Such reports of what's wrong are analogous to the report a technologically naive person makes to an auto mechanic: "it makes a funny noise sometimes; it pulls to the left; the acceleration is sluggish above 30 miles per hour; the steering wheel shakes." When the mechanic sets out to fix the problem, he doesn't look for a funny noise or a pull to the left or a center of sluggishness or a steering-wheel shake. Those are just the symptoms, outcomes, consequences. The mechanic understands how the car works, so he looks for a hole in the exhaust pipe, a tight wheel bearing, a malfunctioning carburetor, or anunbalanced tire. He doesn't say, "Oh, you car has odd-noisia, or dextromobilia, or accelerotardia, or manipulo-oscillia" and go look up the recommended treatment in a big thick book. He reasons out what might underlie the symptoms on the basis of the theory of operation of an automobile, and that theory tells him what is REALLY wrong with the car. That's what a good theory of behavior does for you, when it's tied to the actual functions of the device. It lets you reason your way to the layers of organization that underly superficial symptoms.

Even the interpretation of neural connections themselves is conditioned by the background theory:

"In the new approach, subjects' behavioral responses are not just linked to the stimuli that eventually triggered them, but are connected to mind processes and representations that handled the stimulus and generated the responses according to some mechanism. The investigations no longer shy away from formulating hypotheses about those mechanisms and attempting to test their validity, indirectly, by measuring external responses." (p. 14).

The brain is a maze of connections. If you are under the impression that stimuli cause responses, you will select a path through this maze that connects inputs to outputs, and ignore all other connections (even while noting, in passing, that they exist). You'll stick an electrode somewhere the in the middle of this network and present a stimulus to the senses and look for a response from the electrode. Then you'll try to stimulate that spot and see what response results. If you get a response you'll say that you've traced out an S-R path from input to output. If a reference signal entered into this path somewhere, you'd never find it.

There's no way NOT to apply a model when you're doing brain research. The normal accepted model is an S-R model. This way of thinking about behavior limits what brain research can discover -- or at least which of its discoveries it can purport to make some sense of.

A neurologist who understood PCT would do different kinds of investigations and see different meanings in the findings. Most certainly, such an investigator would use different methods of assessing deficits. The investigator would be trying to find out not just what this person can control and can't control, but how the parameters of control have changed from those shown in normal people. Quantitative experiments would replace informal subjective classifications of symptoms. Perception and action would not be considered separately, but as a closed-loop system. The whole approach to diagnosis would change. Instead of just going through lists of behaviors that a normal person should be able to produce, the researcher would be building up a picture of the functions that enable such superficially-observable behaviors to be seen. The specific quantitative deficits would point to specific underlying brain functions like perception, comparison, and output that would produce such symptoms if operating in a particular defective way.

I think that PCT would revolutionize neuropsychological research.

Best, Bill P.

Date: Mon Dec 14, 1992 9:02 pm PST
Subject: Presenting PCT, Arm (Little Man)

[Avery Andrews 921215.1106]

There are alternatives to the `hard line proseletizing' approach to presenting PCT on the one hand, and watering it down on the other. It is for example quite obvious that `cognitive scientists' do not on the whole understand feedback mechanisms well enough to make an intelligent assessment of what can actually be done with them. You can therefore simply present yourself as someone who thinks that the potential of servomechanism control has not been properly exploited, and is trying to rectify this. In academia it is perfectly respectable to push some idea hard to see what can be gotten out of it - what's not allowed is to claim that the idea is the solution to everything. & people are hypersensitive such claims, since they are the hallmark of crackpottery, and tend to perceive them even where they don't actually exist.

If servo control & its potential were properly understood, Arm-type programs would be as thick on the ground as SHRDLU's and unification-based parsers (like, for example, mine), & people long since would have written programs where Arm-like components were used as the executive modules of SHRDLU's, Chapman & Agre's game-players, etc. (and surely Chapman would'nt have allowed Sonja to crash into walls (`which looks really stupid') if had a real grasp of methods which would have made it easy to keep her from doing it).

So the idea is just to present reasonably complicated PCT models of specific phenomena, explain how they work, and let people draw their own conclusions about the foundations of cognitive science. But, of course, these models have to be `of' things that people are already interested in, for some reason, no matter how silly (unfortunately, tracking seems to be a non-topic, but that's just a fact that has to be lived with for at least a while), and they have to actually do things that people haven't done, thinking that they were too hard, and doing these things has to look easy.

Therefore I see Arm (Penni & agree that `Little Man' has the wrong flavor to make the right impression on our friends out there in Scienceland) as a big step forward in presenting PCT principles in an effective way, at least to the audience that I am thinking about (even better if it could be integrated into a SHRDLU or Agre & Horswill style Toast program, something I've been doing some thinking about, & might try to do if I can get Arm running under Unix/X-windows). If people find such a simulation interesting and `cool', they will be more inclined to find out something about the principles whereby it was constructed.

But I do think that it would be more effective if the accompanying documentation made a clear distinction between

a) things that are well-supported by the neurophysiology (e.g. the lowest level systems)

b) things that are suggested by the neurophysiology

c) pure speculation

with references provided for (a) and (b).

Avery.Andrews@anu.edu.au

Date: Mon Dec 14, 1992 10:04 pm PST
Subject: Theory

[From Dag Forssell (921214)]

Here is a slide from a presentation I am planning. I want to highlight the common (I perceive) bias against theory.

What does "THEORY" mean to you?

	APPROACH TO THEORY	LEVEL OF SCIENCE	TYPE OF KNOWLEDGE
Type 1	Hunch, expectation based on experience. Intuitive / Formal	Common sense / Statistical research	What works (sometimes)
Type 2	Explanation, prediction, test.	Engineering science	Why it works (all the time)
Type 3	Logical reasoning.	Abstract science	Abstraction

Continued to the right.

	METHOD OF LEARNING	TIME TO LEARN	PREDICTION CAPABILITY	RESULTS
..1	Trial & error / data collection	Long	Poor	Spotty
..2	Create theory, test theory	Short	Excellent	Confident
..3	Deduction	Short	N/A	N/A

The idea here is to help the audience realize that when they say: "Don't bore us with theory, show us what to do," what they object to is what seems like useless abstractions. By contrast, a good engineering type theory is very useful.

QUESTION:

Do you think that a mixed audience will relate to these "definitions" of theory and level of science? Any suggestions on rewording, ever so small, which will help will be appreciated.

Thanks, Dag

Date: Mon Dec 14, 1992 10:24 pm PST
Subject: neuroscience

Anyone read NATURE'S MIND by Michael S. Gazzaniga? Any comments? Any cautions?

Date: Mon Dec 14, 1992 10:58 pm PST
Subject: Gang warfare

[From Bill Powers (921214.1900 MST)] Thomas Baines (921214) --

> I am one of those bastards that you call on when your
>national interests (as expressed by your President), or your
>corporate interests (as expressed by your Board), or your
>personal interests (as expressed by your complaint against your
>fellow) get threatened or abused. As military officer,
>economist/consultant, or lawyer, I get to deal with the trash
>that others create. I will use whatever paradigm, model,
>augury, or insight that helps me deal with that trash with the
>very least cost to all concerned.

And I'm glad you are one of those bastards, because the world is the way it is. I keep saying that I'm not against coping with whatever we must cope with, by whatever means we have so far developed. We need armed forces and police and laws. That's because we have never developed anything better.

You must understand, however, that coping and revolutionizing are two different activities. The copers of the world live in the world, they are part of it, they accept it the way it is and try to do their best with it. That's a full-time job. The revolutionary theorist, on the other hand, is not part of the same world except physically. The revolutionary theorist does not accept the world the way it is. He doesn't take sides, at least when wearing his official hat. He sees war, in this case, as a problem for both sides, brought about by insufficient or incorrect understanding of human nature on both sides. The very steps taken by both sides to preserve themselves, as they see it, are the steps that lead to war. When you push on a control system, it pushes back. The pushing back is natural, as is the pushing. It is human nature that leads to the escalation of conflict, and when people understand the inevitability of conflict when control systems clash, they will cease to be surprised when their efforts bring on the very thing they wish to avoid. They will understand that the pusher is as much responsible for the resistance as the pushee. They will cease to expect to get what they want by physical force.

That's a long way down the road. But we have to start down it sometime. Now seems like a good time to me.

John Gabriel (921214.1135 CST) --

>>A real science of human behavior is the ultimate protection against bullets.

>Think on that. And Heinlen's remark that an armed society is
>a polite society, also on Roy Chapman's rule "Never point a gun
>at anything/one unless you are willing to take personal
>responsibility for its' their destruction."

I'm afraid that despite his enormous story-telling skills, I eventually came to regard Heinlein as being stuck in an adolescent fantasy. What he called a polite society was simply one in which everyone tried not to give offense in order to avoid being killed: a society based on fear (mislabelled "respect"). I think I would rather live in Dodge City 1992 than in Dodge City 1872. My idea of a grown-up society is one in which everyone respects the will of others because that's the only social system that makes any sense. Not because they see a gun pointed at them. That basis lasts only as long as it takes for the other guy to turn his back.

As to Roy Chapman's rule, lots of people just love it because it gives them the feeling that other people's lives are theirs to spare or waste as they will. Why is being willing to take responsibility a good enough excuse for shooting someone? As if you could avoid the responsibility by not acknowledging it. Frankly, I would be afraid to suggest to someone with a private arsenal that guns should be taken out of private hands. I would be afraid he'd shoot me, and be willing to take responsibility for it.

>I can't tell if you've read Peterson and the Altmans. The thing
>that makes them interesting to me is the way the society where
>the carnivores can do real harm to each other, but don't
>because if they do they will be too torn up to catch the next
>moose, moves gradually towards the chicken run where nobody has
>claws and teeth, and it's fatal to be crowded and at the bottom
>of the pecking order.

I have read them, but not seriously. I eat meat, but not that of my own species. I'm not sure we know why lions don't enter into lethal contests very often. I might guess that it's because fighting hurts, and lower animals are too stupid to think up reasons to keep on doing things that hurt.

>Let me say one thing that may
>bring down a firestorm. Before the railroad and the telegraph
>and good maps (Gauss's contribution) by and large wars had a
>not unreasonable ecological function - they closed down
>incompetent governments.

But it takes two incompetent governments to get into a war. The government that wins is more competent at only one thing: winning a war. That's not much guarantee of competence in anything else.

>How to close down an incompetent government without
>unacceptable cost in lives, misery, ... Trade wars perhaps.
>Better to be laid off than killed or badly mangled.

That's the problem as I see it: that there always seems to be an acceptable cost in lives, misery, etc. "Sure, we'll get our hair mussed, but we'll lose maybe ten million, twenty million TOPS." (Prof. Buck Turgidson).

>Completely agreed - how do we keep people from wanting to shoot
>at each other? No, I don't want to say that - don't believe in
>police/baboon state.

But that's exactly the question. The way we approach it with HPCT is by studying the hierarchical goal structure.

>Why do people want to shoot at each other, and WHAT might make them want to stop?

Now you're on the same track that I'm on. Only the problem isn't to make them want to stop. It's what they're trying to accomplish at a higher level by shooting. The basic strategy is to find out what they want and figure out how they can get it by a different means. Nobody (sane) has a highest-level goal of propelling a bit of lead into another person's body. That's just a means to an end. It's basically a very inefficient means, because it gets other people mad and they tend to start shooting back at you (whether they're the Good Guys or the Bad Guys). Then everybody gets hung up on shooting and they forget what they originally wanted, and like as not destroy any possibility of getting it anyway. Look at Somalia. That's a polite society?

It's a process called "going up a level." You keep doing it until you reach the level where something is still free enough to change a goal. If someone wants money or power, you ask what he wants it for. And then you arrange for him to get it without the money and the power, or at least without unreasonable amounts thereof. Whatever people want, they usually want it for a higher reason that they've lost track of or never really worked out. Or, on second thought, haven't really cared that much about since they were teenagers.

Picking up loose end from yesterday:

>Bill, I read, and I think I understand you. Put very simply,
>you are saying that a human being can follow a random movement
>of a cursor with a finger, and that this is explained by
>control theory.

Yes. This can also be put somewhat more generally. A human being can control a visual (auditory, kinesthetic, etc.) perception by means of producing a motor output with the muscles. This process is explained by control theory. In fact, there is nothing else a human being can do by way of behavior.

So the "tracking" experiment is simply a prototype of all human behavior. If you've seen Demo 1, you will know that in place of the cursor and target you can substitute all sorts of other perceptions that are affected by the motor output: the pitch of a sound, the shape of a geometrical or nongeometrical figure, a rate of rotation of a triangle, a relationship of symmetry between two shapes, and on and on. The same model predicts the behavior (as shown in Demo 2) in each case. I have shown demonstrations of controlling the names of presidents (from a chronologically- sorted list) and controlling the answer to an arithmetic problem (to make it be the right answer, one less than the right answer, etc.). If you have the budget, you can supply different ways for the muscles to affect the perceptions -- cranks, levers, push-buttons, steering wheels, guns, computers, nuclear bombs, whatever. Figure out what perception the person is controlling, disturb it, and measure the parameters of control. Basically all you need is some ingenuity. Same model.

You don't have to lay out the whole hierarchy in order to demonstrate how the model works for a single control process at any level you please. Pick any perception a person can affect with muscles. Get the person to pick a reference level for it. Get the person to control it in the presence of disturbances (in a constant state, please, to accomodate the experimenter). Match the model to it and thereby measure the parameters.

In this process you may want to bring in some more sophisticated mathematics than we simple pioneers use. You may want to investigate control in Hamming spaces, control of logical propositions or computer programs, or control of tensors or chaotic states. Who knows what will prove appropriate? But as long as you remember that motor outputs are

being used to control perceptions relative to reference levels, you'll be using the same model and not letting in some other less competent model, like SR theory which is always sneaking in by the back door.

Maybe when the person isn't cooperating in a control experiment he or she will go back to behaving in some other way. But I'll bet a considerably chunk of my life (and have done so) that when you've found all the different control processes you can, there won't be much left over.

Of course what's left over will be very interesting.

Best to the Gangs, Bill P.

Date: Mon Dec 14, 1992 11:35 pm PST
Subject: input functions vs. disturbances

[from Wayne Hershberger 921214] Grandpa Powers: Congratulations.

> Bill Powers (921212) There are others who would argue on your side [Wayne]...that any separation of the organism from its environment is a conceptual mistake. It's all just one big system, so there's no point in trying to take it apart into components in order to understand it.

This is NOT my point; rather, I am saying that when taking something apart that works, one wants to keep track of all the working parts and to not mistake a limited set of parts for a complete set. However, I am also suggesting that in taking functional wholes apart one wants to divide them into whole functional parts.

> Bill Powers (ibid) I for one have never found that contemplating the WHOLE THING leads to anything but bafflement...We could just as well say that the actions of the Little Man consist of altering...output, not perception...because when you start with the motor output forces and keep adding all the things directly related to and dependent on them, you end up back with the output forces after one trip around the loop. Making all the substitutions to eliminate intermediate terms, you end up with output = f(output). Or perception = f(perception), or error = f(error). It all depends on where you start.
>
> In our modeling efforts, we have found it definitely useful to distinguish the organism from its environment.

Perhaps, but it seems to me far more useful (indeed, essential) to identify the two inputs to the canonical loop: the reference input and the disturbances (considered collectively). These two inputs to the loop (neither is an input to the organism) mark the functional joints at which the loop is best carved. There is no better example of this argument than your own 1978 _Psychological_ _Review_ article where you identify p* and d, using the organism/environment distinction, effectively, for pedagogical purposes.

The reference signal and disturbance provide the basis for saying that control systems control their inputs rather than their outputs. That is, at the point in the canonical loop where the loop is intersected by the reference signal (i.e., at the comparator), it is the loop's input to this intersection (the variable in the loop downstream of the

final disturbance) not the loop's output from this intersection which corresponds to the reference value. THIS IS TRUE WHETHER OR NOT THERE ARE ANY EFFECTORS OR RECEPTORS (I.E., ORGANISM/ENVIRONMENT INTERFACE) IN THE LOOP. So, whether or not organisms can control environmental variables appears to be another question, altogether. It depends upon where the organism/environment interface is located relative to the last disturbance in the loop.

In your HPCT you have labeled each loop's input to a comparator p (for perception), and hypothesized that higher order perceptions are "subjective EVs" realized by complex input functions--no problem. But the question of whether or not organisms also control objective EVs depends upon the location of the last disturbance in the loop, not merely upon the complexity of the input functions. There appear to be two different matters at issue here. What do you think?

When I said that conceptual EVs (involving imagination) and perceptual EVs are lawfully related I was agreeing with your earlier post when you said:

> Bill Powers (921210) the behavior of the environment is
> lawfully and reliably related to the perceptual signal, as
> long as the form of the input function remains the same.

I read your expression "the environment" as referring to a disciplined conceptual model comprising objective EVs (i.e., EVs of physics). I am using the expression perceptual EVs to include all levels of perception including conceptual levels--as you do. The lawfulness that concerns me is not the regularity between lower level and higher level perceptions, but between EVs described objectively (by scientific models) and EVs described subjectively.

Because the lawfulness of these related realizations may mean nothing more and nothing less than the reality of the laws of their relation, the expression "Boss Reality" seems far to redolent of agency and substance to serve as an appropriate label. Boss Reality sounds like something with a mind of its own. It puzzles me why you use the expression while seeming to reject its connotations--why not coin phrase?

Warm regards, Wayne

Date: Mon Dec 14, 1992 11:35 pm PST
Subject: Re: Neurological research

[Gabriel 921214 20:55 CST] (Powers same date)

>Remember, mainframe software was not
>made to serve people; people were made to serve mainframe software.

Agreed. Missing feedback 3 terminal controller some place. Gang of 3 busy in search of same. Finding an awful lot of lesions in corporate consciousness. Seriously Bill, you and I agree too often for it to be possible for us to disagree on fundamentals. Small unweighted Hamming Distance. But weights are perhaps large for Aristotelian attributes where we disagree. Seems to me this should be called strong but isolated disagreement. Delta functions at a few isolated bits integrate up to significant heat at times, but not large distance all the same.

>Your point about using information about the brain is a good one.
>I did a good deal of that in writing BCP. In fact when I went to

>work at the VA Research Hospital in Chicago, I was full of
>ambitions, with the Northwestern University Medical School (and
>library) right across the street. I thought good, I can just go
>through all the neurological literature and look up what the
>various parts of the brain do, and build the model around that. I
>was soon disillusioned.

Perhaps it really is true that the real world is just very complicated and the great simplifying principles only go so far in the face of a channel where we still have less than perhaps 1 millionth or one billionth of the complete system specification. But that in no way diminishes the significance of the simplifying principles. Without them, we would even more badly drowned in data we don't understand.

>As you say, brain researchers have found a good deal of
>interesting material without using PCT, more in recent years with
>improvements of technology. The problem is not with the
>technology, however. It's with the concepts of behavior against
>which neurological findings are compared.

Fundamental truth. Without a good abstraction, you just have a pile of unorganised data. Without a taxonomy you can't even begin to communicate about the attempt to abstract. Linnaeus done good!! Hugh Dingle, who taught my wife her courses in ecology and animal behaviour used to talk about the field naturalist phase of scientific theories.

>When you stop to think about it, neurological findings are ALWAYS
>based on SOME theory of behavior. Without any theory of behavior,
>all you have is a record of lesions in various parts of the brain
>and some recordings of spikes and potentials from electrodes in
>anatomically, but not functionally, known areas. You see the
>theory of behavior not in the findings about the brain, but in
>the descriptions of external correlates of brain activity.

This is what I might call the "Symmetry Theory" that we don't have yet. The meta-Gestalt of neuroanatomy.

> "Naturally, the lesion method can only be as good as the finest
>level of cognitive characterization and anatomical resolution it
>uses. In other words, the method's yield is limited by:
>   1. The sophistication of the neurophysiological testing or
>experimentation with which anatomical lesions are correlated.
>   2. The sophistication of the theoretical constructs and
>hypotheses being tested by the lesion probes.
>   3. The degree of sophistication with which the nervous tissue
>is conceptualized ...   4. The anatomical resolution of the methods used." (p.
> 9)

At risk of igniting burned out flames, some place in the first few pages of one or another of H. Weyl's books on Group Theory he says "All the real work is down in the mud and the blood."

>From a modeler's point of view, the sophistication of
>neurophysiological testing and experimentation is not very high.
>In fact, evaluations of what is wrong with the behavior of a
>person with a brain lesion tends to rely on subjective and rather
>crude classification of symptoms rather than models of brain function.

A rather good description of the work of a field natural historian. The fact that clinical medicine a) can sometimes fix what hurts, and b) probably has the world's biggest collection of incompletely correlated facts within its' collective perspective, does not make it a mature science. I suppose I'm both fortunate and unfortunate in being inclined towards mathematics and physics. The physics at least is perhaps closer to being a worked out vein of gold. The mathematicians can take comfort however in being always incomplete. >

.....
>And so on and so on. What's going on is nothing more than an
>informal assessment of superficial aspects of behavior to see if
>the patient can do all the things that normal people do, and in
>the familiar way. An atmosphere of formality is generated by
>using Latin terms -- alexia for inability to read, prosopagnosia
>say that the person "has" alexia, "has" prosopagnosia, etc.
>.....

But professional jargon has a useful place besides telling those who don't have union cards to stay out of the discussion - an instruction eliciting hostility from all of us factual omnivores. It conjures up to those "in the know" a very large card index of shared experience. And when a really neat theory like PCT satisfactorily abstracts part, but not all of that card index, the owners of the card index find their livelihoods threatened. If you can abstract all of the index then you win the pot, and the previous owners of the intellectual territory are eventually unemployed. But, as the historians of science point out, only after the generation holding both the card index and the levers of power have all died or retired. The ecological purpose of intellectual warfare. Some time appropriate around April 1st, I'll publish on the net a taxonomy of academia invented by an old friend whom I have not seen for almost 40 years - Margaret di Menna, who originated the classification the day after the party to celebrate her Ph. D. If there are any readers out there in Kiwiland who know her, please say the taxonomy is still in use.

>.....
>Such reports of what's wrong are analogous to the report a
>technologically naive person makes to an auto mechanic: "it makes
>a funny noise sometimes; it pulls to the left; the acceleration
>is sluggish above 30 miles per hour; the steering wheel shakes."
>When the mechanic sets out to fix the problem, he doesn't look
>for a funny noise or a pull to the left or a center of
>sluggishness or a steering-wheel shake. Those are just the
>symptoms, outcomes, consequences. The mechanic understands how
>the car works, so he looks for a hole in the exhaust pipe, a
>tight wheel bearing, a malfunctioning carburetor, or anunbalanced tire. He
> doesn't say, "Oh, you car has odd-noisia, or
>dextromobilia, or accelerotardia, or manipulo-oscillia" and go
>look up the recommended treatment in a big thick book. He reasons
>out what might underlie the symptoms on the basis of the theory
>of operation of an automobile, and that theory tells him what is
>REALLY wrong with the car. That's what a good theory of behavior
>does for you, when it's tied to the actual functions of the
>device. It lets you reason your way to the layers of organization
>that underly superficial symptoms.

Bill, I think it's sometimes hard to distinguish between a malfunctioning carburettor, and accelerotardia if you don't know what a carburettor is. And the problem is very difficult when the mechanic is still trying to articulate what a carburettor is, but in trade school jargon, not neurophysiological jargon.

Your previous paragraph is going to come back to haunt you the next time you complain about triangulation in Kanerva spaces.

Bill, I think I've pulled your leg hard enough so I should stop before exceeding the elastic limit so far that a perceptual feedback circuit causes you to seize a 2x4 and beat me around the head.

But all in fun, and I hope in a good cause. Perhaps I should publish the taxonomy on Jan 1st. CSGNET is the most wonderful colony of boffins I've ever had the privelege of associating with, and between us perhaps one day we'll predate on the buzzards who review papers submitted to the trade union journals. Tom Baines has an interesting theory of the origins of revolutions - like the 1917 Soviet one, rather than the scientific variety. But I am inclined to think it holds for the scientific ones too. I don't remember whether my comment about the bloodless wars in academia was made on-line or off-line. But just like the chicken run, it's hell being at the bottom of the pecking order. Scientific territory is just as subject to ambition and militarism as any other kind. Only the dictators have no guns and no police force, just the threat of no tenure.

Sincerely, and with affection for all my fellow boffins.
Sic Itur ad Astra, and nil Bastardio Carborundum.

John Gabriel

Date: Mon Dec 14, 1992 11:35 pm PST
Subject: Re: Theory

[gabriel 921214 21:46]

I love 'em. And although I masquerade as a mathematician, much of what I do might be engineering. I think every engineer I know will have enormous recognition reflexes about this, also all the good teachers, managers, and leaders of all kinds. Perhaps the other two of the gang of 3 will risk their mgrs' wrath and post them on bulletin boards (the paper kind). No use posting at Argonne. The worst place of all to preach is among those who (mistakenly) believe they are converted.

Marian - do you want to expound on the Hugh Dingle stages of development of a scientific theory - from bird watching to taxonomy to Newton, F=ma and Einstein?? Look in my .mailrc for the csgnet alias if you do.

A small mathematician/logician's nitpick. Besides deduction, there is induction. Both are actually predictive - Kepler's Laws demonstrate a central acceleration proportional to the inverse square distance with a universal constant of proportionality for the planets observed. That's deduction. The induction is the Gedanken experiment of putting a hypothetical planet in place and remarking that it may be any material object.

Would you say that inferring Pythagoras' Theorem from some simpler propositions was predictive. Moot point. Was it true all along before anybody even mentioned the topic. Hence Diodorus, and the stone at the bottom of the ocean that is not, has not, and never will be seen. Or the tree in the forest. But probably not the sound of one hand clapping.

Actually I have real trouble distinguishing between deductive and inductive reasoning, just as I do between forward and backward reasoning - probably one of those blind spots we all have.

So. Bottom line. I am a bit uncomfortable about your N/A entries. But otherwise absolutely delighted.

John Gabriel

Date: Mon Dec 14, 1992 11:36 pm PST
Subject: Re: tom' post

From: Tom Bourbon (921215 00:19 CST) Mark William Olson (921214 11:21 CST)

First, you have my deepest sympathy for the many frustrating hours you describe -- banging out long replies on a bad on-line editor, only to see them vaporize! I spent many unpleasant sessions like that at my former university.

Now for one of your posts.

>Ok, I just read Tom's long post and realize that there is a
>misunderstanding about what I am saying. Did I say that the
>clinical report was a CHALLENGE to PCT? If so, it is not what I
>meant. I meant that here is a perfect opportunity for PCT to get
>involved- ...

My mistake. I was picking up on phrases which I thought meant that if we did not use what is known about the brain, PCT could not advance much farther.

As for "a perfect opportunity for PCT to get involved," so far no reviewer or editor of manuscripts from this lab, on imaging brain structure and function, has tolerated the slightest suggestion that behavior is purposive or that it controls perception? Neuroscience is not waiting for us with open arms!

>-I knew when I first saw the videotape of the patient that it was
>a problem at the Relationship level--just like Tom said.
>So....since we believe it is a Relationship problem and we know
>that there is a bilateral lesion to the superior parietal area,
>then lets start mapping our hierarchy.

I don't follow you here. I am serious. Exactly what are we supposed to map onto what? From the case you described, what do we know about the superior parietal area (SPA)? Which portion, or portions, of the SPA should we consider, and how, specifically, does the SPA figure in the man's control of relationships?

According to reports, in the video the man came close to reaching the comb then stopped short. In my post I described simulations of disturbances to three different parts of a loop that controls relationships. All three disturbances could lead to a model that stops short of reaching a target. I suggested that if the man's lesions produced effects like those of any one of the disturbances in my simulations, then those effects might have produced the behavioral results seen in the tape.

As I said in my post (Tom Bourbon 921214 01:30 CST)

>Did the man's lesions alter or disturb a reference signal, making
>it negative compared to what it was before? Did the lesions add
>positive disturbance to an error signal or a perceptual signal?
>Did the lesions destroy one of the functions in a control system,
>or modify its workings, or disturb or disrupt a path carrying one
>of the signals in a control system? Knowing that a lesion was in
>some general part of the brain, and that outward appearances of
>the person's behavior changed, tells us nothing specific about the
>reasons for the changes; but the process of modeling and
>simulating the control of relationships raises some possible
>explanations that, to my knowledge, neuroscientists and
>neuropsychologists have not considered.

I am not certain how the information about this fascinating clinical case might guide us in mapping between brain function and a PCT hierarchy. Deficits like those in the tape might be produced by any one of several changes in brain-as-control-system. Which of those possible changes, if any, might have been produced by the lesions? Or if none of the disturbances I modeled resembled the real causes of the man's problems, which other portions of the control loop might have been altered by his lesions, and how were they altered? Unless we know that, we do not know which label to place at the SPA location on the "map."

>We cannot map the hierarchy without neuroscience data--that seems
>very clear--how else will you know where some function occurs
>unless you know about what happens when areas are disabled (or
>brain scan data)?

How can there be a science of brain function if a majority of brain scientists do not at least occasionally examine the possibility that, by their behavior, organisms control many of their perceptions and that they incidentally control the states of certain variables in their environments?

Seeing deficits as sequelae of specific injuries, and seeing images in brain scans during certain kinds of performance, cannot legitimately be taken as meaning that some "function" occurs in a given place. In most cases, the alleged "functions" are too poorly defined to be taken seriously, but they are often taken very seriously in neuroscience and neuropsychology. Bill Powers discussed that point at length in an earlier reply to you -- the network citation is lost to me.

PCT modelers CAN work at determining whether control occurs, and if so, whether it is best described and modeled as though produced by hierarchical perceptual control systems. If control occurs and can be modeled that way, the results of our effort might inform neuroscience. I offered my simulations as a modest example of how we might go about that project.

>I don't think we disagree very much --I think we agree alot. My
>only disagreement is to think that you can do it ONLY topo-down.
>You gotta have both. The bottom limits the top ad the top inform
>the bottom.

I do not understand what you mean. You gotta have both of what? Is PCT "top-down" and if so what does that mean? I don't know. Is neuroscience "bottom-up?" If so, I truly do not understand what that means.

I am happy that you raised these topics (control of error; neuroscience and PCT) on the net. Keep plugging away at that messy screen editor!

Until later, Tom Bourbon

Date: Mon Dec 14, 1992 11:36 pm PST
Subject: Re: Presenting PCT, Arm (Little Man)

(Avery Andrews 921215.1106)

>There are alternatives to the `hard line proseletizing' approach to
>presenting PCT on the one hand, and watering it down on the other.
>It is for example quite obvious
>that `cognitive scientists' do not on the whole understand feedback
>mechanisms well enough to make an intelligent assessment of what
>can actually be done with them. You can therefore simply present
>yourself as someone who thinks that the potential of servomechanism
>control has not been properly explpoited, and is trying to rectify
>this. In academia it is perfectly respectable to push some idea hard
>to see what can be gotten out of it - what's not allowed is to claim
>that the idea is the solution to everything. & people are
>hypersensitive such claims, since they are the hallmark of crackpottery,
>and tend to perceive them even where they don't actually exist.

Wish I'd been smart/perceptive enough to say that. Oh well, too soon old and too late smart. NIIIICE, Avery. Congratulations. And congrats to Penni too for her notes to the NET. They are always a delight. If all you folk disappeared, I'd miss you something awful.

John

Date: Mon Dec 14, 1992 11:54 pm PST
Subject: A new kind of revolution

[From Rick Marken (921214.2200)] Thomas Baines (921214) --

> My interest in PCT is merely to acquire another useful way of
> dissecting problems. I think PCT can be used to organize one's
> thinking about human interaction, even if the specifications of the
> complete model are, as yet, unknown.

Avery Andrews (921215.1106) --

>There are alternatives to the `hard line proseletizing' approach to
>presenting PCT on the one hand, and watering it down on the other.

>In academia it is perfectly respectable to push some idea hard
>to see what can be gotten out of it - what's not allowed is to claim
>that the idea is the solution to everything. & people are
>hypersensitive such claims, since they are the hallmark of crackpottery,
>and tend to perceive them even where they don't actually exist.

>So the idea is just to present reasonably complicated PCT models of
>specific phenomena, explain how they work, and let people draw their
>own conclusions about the foundations of cognitive science. But,
>of course, these models have to be `of' things that people are already

>interested in, for some reason, no matter how silly (unfortunately,
>tracking seems to be a non-topic, but that's just a fact that has to
>be lived with for at least a while), and they have to actually do things
>that people haven't done, thinking that they were too hard, and doing
>these things has to look easy.

I think both of these comments represent the same misconception about PCT -- and one that is difficult to disabuse people of. It is the idea that proponents of PCT are pushing it as an improved tool for solving existing problems or "a better model of things that people are already interested in". PCT has never been (or, at least, should never be) pushed as "the solution to everything", as Avery suggests it is. In fact, PCT's main problem is that is suggests that many of the problems people want to solve (how does reinforcment control behavior?, how do people generate grammatical sentences? etc etc) may not be problems at all; that is, they may not be actual phenomena. That's what the "Blind men" paper was about -- not that PCT helps us understand S-R causality, selection by consequences or planned output -- it suggests that these phenomena may not be what they seem.

Since PCT makes us theoretically suspicious of the nature of the phenomena that many life scientists think of as important, PCTers avoid trying to explain phenomena that don't occur with high reliability; so we don't accept statistical findings as indications of a phenomenon. So most of the phenomena that are described in the literature of the life (and, certainly, the behavioral) sciences are simply not phenomena to PCT. There is nothing to explain.

PCT is a new kind of revolution in science. Unlike previous, familiar revolutions, PCT was not developed to account for the existing array of data PLUS some new and puzzling findings. PCT shows that the existing array of findings are not findings at all -- they are just noise with important sounding names and descriptions attached.

PCT is based on a single insight -- organisms exist in negative feedback situations with respect to their environments. The result of this relatinoship is that perceptual variables are maintained in reference states against disturbance. Examples of this kind of behavior have been demonstrated and modelled precisely -- the kind of behavior we see is what has always been called "purposeful behavior". Powers has spent years trying to explain the IMPLICATIONS of this insight; not by re-explaining all the data of the life sciences (a demonstrably useless exercise because this data is noise) but by showing how things will look when you are dealing with feedback control systems and how these appearances can be probed to get at the organism's purpose -- the variables it controls.

Those of us who "proselytise" PCT are not trying to push it as an alternative to existing theories or a better way to explain the "important" discoveries of psychology -- most of these discoveries are not discoveries at all. We are just saying that the life sciences made a mistake in their first step out of the gate -- an understandable mistake because there was no science of feedback control at the time. Now we understand how such systems work -- so WE CAN START ALL OVER AGAIN -- because we have to (if organisms are feedback control systems and I think the evidence that they are is rather overwhelming).

The revolution we propose is to hit the reset button -- ignore all or most of the existing data. It's crumby, statistical stuff because THIS IS WHAT YOU GET WHEN YOU STUDY CONTROL SYSTEMS USING TOOLS DESIGNED TO STUDY INPUT-OUTPUT SYSTEMS!!! PCT does not have the solution to existing problems; it can't because existing problems are not problems at all -- they are just what you see when you work from the wrong premise.

We don't know where the science of PCT is going or what data it has to explain because the reserach HAS NOT BEEN DONE YET -- IT HAS ONLY BEEN STARTED (and I mean just basely; by a handful of people doing the very elementary balls rolling down planes kind of stuff -- the much maligned "just tracking" studies).

On this net we speculate about what PCT has to say about all kinds of wild and crazy stuff --- religious behavior, language behavior, clinical processes, etc etc. But it's all just (well informed and fascinating) HOT AIR.

The reason I want people to get into PCT is not so that they can solve their existing problems -- it's so that they will stop chasing that chimera and start doing PCT science (whatever that is) -- because there ain't many people doing it and we won't get to the interersting stuff until we get a pretty credible base of solid PCT facts.

The message of PCT may still be crazy -- but it's a different kind of crazy than what Avery suggests. PCT is not asking people to see it as the new grand,unifying explanation of everything. It wants people to forget everything they've done (if it was based on a pre-PCT, open loop view of behavior) and START OVER. That's what a real revolution is, though, isn't it?

Best regards Rick

Date: Mon Dec 14, 1992 11:54 pm PST
Subject: Re: War and PCT

From: Tom Bourbon (921215 00:48) [gabriel to powers 921214 11:35 CST]

Actually, this is a request for information. The following remark occurred during your musings in "PCT and war:"

>How do you learn to spot those small mistakes soon enough without
>millennia of Darwin backing you up? Increasingly critical problem
>when communication works faster, and those who have been applying
>PCT in advertising agencies have a substantial hand in deciding
>who gets to be president and C in C. Perhaps it was better when it
>took three months to go from Oregon to Washington to kick butt of
>elected rep.

"... those who have been applying PCT in advertising agencies have
a substantial hand in deciding who gets to be president and C in C."

I collect citations of PCT and I am always looking for new areas in which PCT is applied. I have not seen, perhaps because I never looked for them, applications of PCT in advertising. I would like to add that specimen to my collection. Can you provide a few specific citations and examples?

Until later, Tom Bourbon

Date: Tue Dec 15, 1992 4:00 am PST
Subject: Law and Order/Fratricide

[Gabriel 921215 05:29 CST]

Bill P. points out that among other things PCT could lead to better understanding of reasons why people do/do not shoot at each other.

We have two quite different mechanisms in the two societies chronicled by Rolf Peterson and the Altmans.

Among the wolves:-

Stop hassling you idiots. If you cut each other up, we'll none of us be able to catch the moose.

Among the baboons:-

Eat your nice leaves junior, and don't sass the boss or the leopard will get you.

And, uncomfortably close to home:-

In Los Angeles recently, as reported by LtC. William V. Wenger

As I (Wenger) watched in disbelief, a gang member in the car in front of me, waiting to make a left turn, shot the occupants in the cars on either side of him. I was unarmed, but quickly followed the shooters, as did several police cars, until they were apprehended 2 blocks away. I reported what I had witnessed to the police, and then drove to our headquarters armory to supervise the mobilzation of my Army National Guard battalion in Ingelwood, California.

The feedback mechanisms for Altman and Peterson's reports are quite clear. The need for the pack to function successfully in hunting, in order to eat, and predation by leopards on baboons, who have no serious resource shortages in the way of food, but are the victims of predation.

Any comments on exactly what feedback mechanisms were working and what were not in Los Angeles??

John (gabriel@athens.eid.anl.gov)

Date: Tue Dec 15, 1992 2:45 pm PST
Subject: Law and Order/Fratricide

From Rick Marken (921215.1030)] Gabriel (921215 05:29 CST)--

>The feedback mechanisms for Altman and Peterson's reports are quite clear.

I didn't see them clearly at all. Could you give a quick diagram of the "feedback mechanisms" operating in this situation.

>Any comments on exactly what feedback mechanisms were working and
>what were not in Los Angeles??

Looks like the gang member wanted to see someone shot, saw them shot and left (because they also wanted to percepive themselves as being "not in custody", prbabaly) -- control of feedback. It works because the gang member's actions have effects, via the environment, on the gang member's perception (of things like whether or not people are shot).

Are you really intrested in what higher level perceptual goal might have been\ satisfied by produce the percpetion of someone being shot? I don't think you can tell from just looking at the results described in your little scenario.
Best Rick

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: Tue Dec 15, 1992 4:02 pm PST
Subject: VOR as Feedforward?

[from Gary Cziko 921215.1956 GMT]

Bill Powers and/or Wayne Hershberger and/or Greg Williams and/or other interested parties:

I have begun discussion with a neurophysiologist on my campus about PCT. Since my wife works with and is a good friend of his wife, we keep bumping into each other and so I thought I would see how he responded to PCT ideas.

Here is one reaction:

>However, the system I study (the vestibulo-ocular reflex or VOR)
>is feedforward. Its input (head velocity) is not affected by its
>output (eye velocity). So this system does not control its input,
>but just responds to it. I've done some modeling work on this
>system, and in particular, I've tryed to show that other conceptual
>tools can be used to go beyond control theory in attempting to
>understand the neurophysiology of this system. I'd be happy to
>send you a copy of a relavent paper, if you're interested.

I've heard this quite a few times before (together with the statement that the eye has no propioceptive sense as to where it is). But I have trouble making it mesh with Bill's strong statement of no important relationship between efferent neural output and behavior.

I can see how the eyeball is a pretty well protected from disturbances, but nonetheless how can the brain send a computed command to it to exactly compensate for head motion without on-line feedback? (Our ability to do this can be easly demonstrated: put your finger about a foot in front your eyes and move your finger side to side about 4 inches at about two cycles per second. You will see a blurry finger in spite of your eyes attempting to track it. Now move your head side to side at the same speed and keep the finger still. You will see it perfectly clear).

Even protected from outside disturbances, there is still the acceleration to velocity to position problem which Bill claims makes computing muscle responses highly sensitive to error. If indeed there is no proprioceptive feedback about eye position and this is pure feedforward, doesn't that show that such efferent computations ARE possible?

But then what happens when the muscles tire? And has anyone ever tried to disturb the VOR by putting molasses-like eye drops in the eye or resisting its motion by somehow attaching a rubber and tugging during head movements?

I realize that there is a higher level feedback system which uses vision here. But would that be fast enough to make this a real feedback system in spite of the lack of proprioceptive feedback?

I remember bringing this topic before, but apparently my current confusion means that either I didn't get answers to these questions or that I didn't understand them at the time. Let's try again.--Gary

Date: Tue Dec 15, 1992 4:03 pm PST
Subject: Re: neuroscience

From: Tom Bourbon (921215.0925 CST)

Re: Mark William Olson   (14 Dec 1992 10:35:50 CST)

You were having trouble with your text editor and only part of your message arrived, but I think at least part of your point came through.

>(cont).
>then you've got something related to memory, no matter how you
>describe it. Hubel and Wiesel (sp?) found some pretty interesting
>(although incomplete) stuff about the organization of the
>occipital cortex and probably never even heard of PCT. Do you

>want to claim that they are wrong because they have the wrong paradigm?

No. I never did and I never will. I started teaching long enough ago that, for the first several years, whenever I discussed the work of Hubel and Wiesel I included a totally safe prediction that they would share a Nobel Prize. They did, and they deserved it. But I also predict that when someone finally picks up the challenge and the opportunity to study nervous systems as though organisms control some of their perceptions, that person will learn things Hubel and Wiesel did not, and could not, learn. And I believe it is totally safe to predict that the person, or someone else soon after, will win a Nobel Prize. If I had the skills and techniques to work at the single-cell level with Aplysia, the marine snail, I would drop everything else and begin a crash program, starting with behavioral studies to test whether the creature controls anything. After demonstrating the obvious, I would begin a systematic analysis of the control-system properties of the creature's exquisitely mapped nervous system -- anatomically exquisite, I do not trust most of the functional maps. They were created in S-R, or I-O, research procedures. If anyone out there is interested in a trip to Oslo, I believe I just handed you a ticket.

>We know alot of visual processing occurs in occipital. We know
>that attention mechanisms are involved in at least prefrontal
>areas. We know that the hippocampus is involved in at least one
>form of memory. WE didn't need PCT to find that out, because we
>are working at a level of analysis below PCT.

Yes, we do; and no, "WE" didn't. But the meaning of what we know is not very clear. Many of the biggest problems in our understanding of how brain and behavior-psychology are related come from the vagueness or the inadequacy of our ideas about the behavioral and psychological phenomena we try to explain. What does it mean to say that a particular "area" or "region" of the brain is "involved" in a particular function or process, whose definition or existence is not established? Which of the many conceptualizations of attention can we explain in terms of involvement in the prefrontal areas? Are there "forms" of something called "memory" and how is the hippocampus involved in one of them? Please realize I am NOT saying that people do not find correlations between, on the one hand, damage to or stimulation of different parts of the brain, and on the other hand, the behavior of organisms. But the correlations are often poor, and so is the localization of any perturbation applied to the brain. And our conventional understanding and portrayal of behavior is often poor.

To explain what we think we know about behavior, we use what we think we know about brains; to explain what we think we know about brains, we use what we think we know about behavior. Most of what we think we know about either topic comes from research in which brains, neurons, whole organisms, small groups, societies, behavioral actions, and consequences of actions are all conceived of in classic S - R terms, or in more modern, but identical, I - O terms. All of that accumulated knowledge must be re-examined in light of the fact that organisms control many of their perceptions.

>Sure PCT could help inform the process but it isn't as necessary
>as it would be for levels above PCT--the rest of psychology.

PCT is not about a few "higher" levels of perception. It is about control by living systems. Living systems control the perceived states of so many variables that control, achieved through negative feedback interactions with the environment, is probably a defining property of life. If that is so, no part of the life sciences is immune from the need for a theory that explains control. PCT explains control.

Until later,   Tom Bourbon

Date: Tue Dec 15, 1992 4:57 pm PST
Subject: Re: Opening it up

[This mail was originally sent on Thursday 10 Dec but returned because
of the breakdown of the internet connection]

[Martin Taylor 921210 17:10]   (Bill Powers 921209.0900)

Bill--Little Man 2 arrived today. It runs fine, so if anyone else has a bad copy, it at least isn't a general flaw. We have been talking about whether we can get it to run on a Mac, and what would be involved. Is anyone else doing that (Is that what Rick wanted to do? I've forgotten).

Bill to Bob Clark (921205)

>I'm glad to see you opening up the discussion. "The" hierarchy is a
>figment of my imagination, building on OUR imagination. For most of the
>levels I've proposed, the only backing for the specific definitions is
>anecdotal and subjective. As far as I'm concerned, these or any other
>levels won't be "facts" until we have put them to experimental test.
>...
>I agree with Rick Marken about decisions: they represent conflicts.
>Unless there were at least two competing goals to satisfy, there would
>be no need to make a decision. You would just do whatever is required to
>achieve the single goal.

I have been spouting off around here at any opportunity about the power of PCT, and so a couple of colleagues have asked me for a PCT approach to, and predictions about planning/decision making. The situation is one in which imagination must be used. The real world cannot be addressed to look for error. Here's the task they are using:

> The planner is given a sheet of squared paper on which is a map of the aisle of a supermarket, with goodies such as "yoghurt", "cookies", "Coke," "Oranges" marked on the shelves, one item to a square. There is an "In" door and an "Out" door, and two robots with shopping baskets, a blue one with a basket that can hold 6 items and a red one with a basket that can hold 11 items. The blue robot can move (must move) 2 squares in one clock tick, while the red robot moves one square. Between the two robots, they have to pick up 15 items on a shopping list in a time less than T ticks, where T is specified by the experimenter as 20% longer than the best time the experimenter or a few selected people have managed to achieve. The robots can transfer goods between their baskets if they stand on the same square. The baskets are actually stacks, one item wide, so that every item is

explicitly on top of or under every other item in the basket. Drinks cannot go on top of anything, and bakery goods cannot go under anything. Dairy products must be picked up "as late as possible before finishing." The expedition is finished when both robots stand at the OUT door with the 15 items in their baskets.

The subject in this experiment is required to specify every move of each robot, without retracting a move, so the plan must be pretty well completed in detail in imagination, without violating any of the constraints. The experimenters ask the subject to vocalize as much as possible the thoughts that go on in making the plan, and to gesture and mark up a sheet showing the map of the supermarket. They think they have identified four levels of abstraction in the planning process, but these levels do not seem to correspond with any PCT levels (as one might expect, given Rick's PCT motto # 1: "you can't tell what anyone is doing by looking at what they are doing," as modified in LP for communication: "you can't tell what anyone is saying by listening to what they are saying.")

When I thought about this within the PCT structure, I came up to a point where it seems to me that there is a need for modelling that is not accomplished by the canonical "imagination loop." As I understand it, the imagination loop asserts to the perceptual input function that a reference level (the output signal of the ECS as seen by a lower ECS) has been satisfied. But in this planning situation, one has to imagine that the world has been unkind, and that the desired perception has been blocked. (The robots can't transfer goods because the red robot hasn't got to the rendezvous, for example. Or perhaps the transfer won't work because it would put fruit on top of bread.) One can't see the problem in the real world, because it hasn't happened. The same sort of thing happens when planning in the face of danger. Making a wrong move may kill you, so you can't control the error in real life. You have to do it in imagination.

It seems to me that there must be somewhere a facility that isn't memory that allows one to put a simulation of real world impedances into the imagination loop. I don't see where it comes from within standard PCT.

Bill says:

>At the level I call "programs," symbol-handling processes occur which I
>characterize as a network of choice points. These are like the TOTE
>unit, in that there are tests for conditions, with the choice of a
>branch being determined by a rule applied to the results of the tests.
>The term "choice" seems to imply a decision, but in fact there are no
>decisions at this level either. The conditions encountered at each
>choice-point, plus the rules, completely determine the path to be
>followed next. Only when there is ambiguity or when the rule is self-
>contradictory (calling for more than one mutually-exclusive path to be
>followed) is anything like a decision required. If you have an algorithm
>for making decisions, you don't have to decide anything!

To me, this works only after the "plan" has been produced. The plan is the program that determines the choice given the perception. It is in the making of the plan that the imagined perception needs to provide the problem, assess the conflict possibility, detect the barrier that the world will put in the way of achieving the desired perception. The decisions are in the plan making, I think. But where is the world model that allows the imagination to know that decisions are required?

==========

Related, but different.

I detect now and in some discussions of last year two quite distinct uses of the term "reorganization." One works on-line and affects what to do right now, the other affects how to do things for ever afterwards. The latter is what I have always addressed when I have talked about reorganization-- changes in the link structure among ECSs, changes in the perceptual input functions, changes in reference input functions, and so forth. It's a long-term process, on-line in a way, in that it occurs when what one is doing is not working. But its effects are permanent until once again reorganized away.

The other form of reorganization seems to be what has been talked about from time to time, as in Bill's posting:

>If we eliminate programs -- the execution of algorithms for choosing
>paths -- from decision-making, what is left? As far as I can see, only
>the cases in which for some reason we wish to do two contradictory
>things at once. At that point we must reorganize or simply suffer the
>paralysis of conflict.

A while ago, we had a little discussion about choosing to take a car or a bike or to walk to a destination. Bill argued for a decision-making process within the ECS that was trying to satisfy the reference to perceive its organism as being at the destination. I argued for the impedance of the world, as demonstrated in the world's inhibition of taking any two modes of transport simultaneously. Now I'm asking about the imaginary process of deciding NOW which to take (making the plan), when I will travel tomorrow. Where is the information about the world's constraints held, and how does it come into play? I don't see where reorganization comes into the act, but quite a bit of Bill's posting to Bob Clark (921205) seems to suggest that he does.

In puzzlement. Martin

(PS. The internet connection still seems flakey, so I don't know when this will get out. Maybe right away, maybe not.)

Date: Tue Dec 15, 1992 5:07 pm PST
Subject: Presenting PCT

[Avery.Andrews921216.09109] (Rick Marken (921214.2200))

>I think both of these comments represent the same misconception about PCT -

I disagree. If someone accepts (a) that they don't understand feedback as well as they thought they and (b) feedback mechanisms are much more central in the organization of behavior than is generally supposed, the rest follows. E.g. that it's a significant achievment to predict tracking behavior for one whole minute, that we ought to try to find out what Aplysia is controlling for, etc. Even if this assessment is overly optimist, surely people who have begun to suspect the truth of (a & b) will be more sympathetic reviewers of PCT papers, & more likely to recommend funding of this kind of research.

Date: Tue Dec 15, 1992 6:37 pm PST
Subject: Newt

[From Bill Powers (921215.1530)] Gary Csizo? or Jim McIntosh?

I got a message with a header saying it was from Jim McIntosh and signed -Gary, and containing a message to and from Linda Littleton. TOTAL CONFUSION.

According to Linda Littleton's reply, the cause of my mail not getting out to NetNews is a bug in her gateway program, which evidently she will now proceed to fix. I have not changed anything in the way I send things, and anyway what goes out on the net by way of header information is completely out of my control. I asked at Fort Lewis' computer center, and they say they have used the same software for three years. If all Linda has to do is collapse consecutive blanks (that's IBM for spaces) to a single space, this shouldn't take long -- it's a nobrainer.

Bill Haley --

Thanks for the encouragement. After all this time I'm not about to give up. None of this is new to me. In fact it's kind of nice to see others experiencing the problem -- now they won't think I'm a whiner.

Dennis Delprato (921215) --

Very very nice analysis of The Problem. I keep thinking that if we could just make the explanations clearer, the referees would get the point. But every time we make it clearer, they see more clearly why they don't want that stuff in THEIR journal.

Gary Cziko (921215.1956 GMT) --

The vestibulo-ocular reflex (VOR) seems to be a true open-loop system. It moves the eyes counter to the direction the head moves.

Like all open-loop systems it is crude and approximate. I believe that the mean error for a head-turn of 40 degrees or so (in the dark) is something like 15 degrees. Wayne Hershberger probably has more truthful numbers. If you fixate on something and turn your head as rapidly as you can to one side, you'll see the image jump, and then a saccade or two to correct the error. All this system can do is reduce the amount of correction the control systems have to apply after a very sudden head-turn.

For slow head turns the eyes simply stay locked on target. The amount of reflex signal must be small, because if there were any such reflex signal it would cause an error in tracking. The detectors in the macula report angular velocity, which gives the eyes a jolt to the side that is large for larger peak velocities and then an opposite jolt when the movement stops abruptly. Or something like that. This is effective for large head movements that occur within the normal saccadic delay. The amount of this reflex adapts over a period of perhaps 20 minutes, implying a higher level of (very slow) control of the correction. When the target is made to move with or against the eye movement, there is at first a very large error that gradually becomes smaller on successive trials. The final accuracy is never very good, as I understand it. About what you'd expect of an open-loop system. It's nothing compared with the arc-minute precision of optical tracking.

There are probably other such open-loop systems, but they aren't very important for normal control situations.

Martin Taylor (921210. 1710)--

The problem with the Little Man disk was at the receiving end. Glad to know yours is working.

As to the planning question, as I was reading the problem I was thinking how I'd go about solving it. I realized first that I would do it in imagination, as you suggest. But then I realized that this would enable me to circumvent some of the more restrictive rules about the stacking order of the items in the basket. I can do this in imagination because I can run the simulation as many times as I like; if items come out in the wrong order I can back up and change part of the route to get them in the right order. In fact I can run the procedure forward and backward, revising as many times as I like, until I get lucky and find a solution.

Another realization was that I would visualize the locations of the items, and then run paths from one item to the next in imagination or on a piece of paper, like drawing construction lines. I'd first locate the items on the list, then start with the ones that had to go under others and go zip-zip-zip from one item to the next, drawing the lines. Then I'd follow the lines, picking up the items in order. Of course I realize that in really solving the problem I'd have to get both carts into the act to handle access to the items that were on odd squares that one cart couldn't reach; the easy way is to bring both robots along and fill the basket of the robot that has to skip squares first, making transfers as necessary, and then let the one-square robot finish up the list. I'm sure this wouldn't be a minimum-time solution, but I guarantee that if I found any solution I would lose interest in the problem immediately. In fact, I already have. The only places in real life where the rules get that complicated are in games. One has to be an intellectual-game aficionado to put any real effort into meeting all the requirements.

The same thing goes on in chess, doesn't it? You can't actually touch the pieces, but in imagination you can say "If the knight goes there, it can attack there, there, there ... and now the bishop or the rook can reach it so that's no good ..." Or am I betraying the reason that I'm such a poor chess player?

I'm very much an analog problem-solver, and I rely heavily on trying all the combinations until I hit one that works (even when there are, as I find out later, after giving up, 100E100 possible combinations). That's pretty stupid, but it does answer one of your questions about "reorganization" that isn't part of the reorganizing system. I think we learn search strategies as program-level algorithms, and use them to find ways of affecting controlled variables. The algorithms we learn can be crude and inefficient, like mine, or very clever and efficient like good chess-playing programs. We have to reorganize to learn or invent these algorithms, but once they're learned they can substitute for true reorganization if they work fast enough to prevent intrinsic error. This applies to other things we learn like reducing algebraic expressions to canonical forms. This isn't learning a specific set of moves, but learning how to search for moves that will get us closer to the final form we want. The initial learning of these strategies, in algebra class, requires some painful reorganization, but once they're learned even a duffer can fumble through to the answer just by trying everything possible and legal in imagination (supplemented by that wonderful invention, writing).

One more note. When I solve problems in imagination, I don't usually see all the environmental "barriers" that makes some solutions unworkable. But in the course of running the problem in imagination, the barriers show up all by themselves and get in the way. It isn't that I anticipated them; it's just that the mental model of how things work naturally spits up those barriers when I try to make the world work in a way that violates my model of it.

Here's an example. Imagine a golf ball. Now imagine a coke bottle. Now, without stopping to analyze anything, imagine dropping the golf-ball through the opening into the bottle. Does it go through? Mine doesn't; it bounces off. The hole is too small.

I just checked this out with Mary, and she had no trouble at all dropping the golf ball into the coke bottle. I asked "Well, what about a beach ball?" Oh, no -- it wouldn't fit inside the bottle! But what about getting it through the neck of the bottle? Oh, well, when you're imagining, it just sort of squeezes through -- you didn't say how real-world this was supposed to be. Then we tried threading a needle with a piece of knitting yarn -- sure, no problem, you just get an embroidery needle... thus erasing my model with an ordinary needle in it, which of course has an eye too small to get the yarn through.

As to the coke bottle revisited, we then decided that when you really mean it, the golf ball refuses to go through. A BB-shot is easy -- it drops right in. But a marble? Well, that's borderline. The mental models aren't that accurate. You'd have to go get a coke bottle and a marble and try it.

The upshot is that we both decide things of this sort in imagination just by trying them and seeing what happens. We don't refer to symbolism, like "a golf ball is 1.5 inches in diameter and the opening in a coke bottle is about 3/4 inch, and 3/4 is less than 1.5 so it won't work." We just look, in imagination, and see if it works.

This doesn't work for everything; I can't see the square root of 7.5 just by looking at a length of 7.5 in imagination. I have to go the symbol-manipulation route, on paper.

Obviously, explaining how these mental models work in such shocking detail isn't going to be easy. The imagination connection is probably remotely related to what is really going on. Furthermore, there's no reason to think that everyone plans in this way; there's probably no one method of planning. One person draws columns on a piece of paper and lists everything; another just starts pushing possibilities around. "Planning" is really a catch-all word, that covers lots of things people can do at different levels.

Best to all, Bill P.

Date: Tue Dec 15, 1992 6:38 pm PST
Subject: Re: PCT and war

[Martin Taylor 921215] Back on the Internet, with 70 unread messages!
(Bill Powers 921213.1730)

>>Escalation is always a problem, witness what is happening in
>>the cities. Just exactly why I want to have better decision
>>making in Govt AND in Defense.
>
>Yes. Control theory shows why conflicts tend to escalate. It is
>the nature of conflicting control systems to raise their opposing
>outputs to the maximum possible level. The only permanent
>solution is to resolve, not win, the conflicts.

Why, then, do most species stop fighting short of lethality, whereas humans are among the very few who don't? The argument from integrating output would seem to suggest that all species should fight to the death in the case of an unresolvable conflict (can't both have this doe).

Giving up seems to be the most common way of ending conflicts. One participant "decides" that winning is unlikely, and concedes. Humans are considered wimpish, cowardly, poor specimens, if they follow that sensible rule. I would have thought that if humans have more levels of control systems in their hierarchy, they would be more able to avoid or resolve conflict than would other species; at least a naive application of HPCT would seem to lead to that conclusion.

Martin

Date: Tue Dec 15, 1992 6:38 pm PST
Re: continued neuro [Martin Taylor 921215 19:30] (Mark Olsen (cont) 921214)

>Hubel and Wiesel (sp?) found some pretty interesting (although incomplete)
>stufff about the organization of the occipital cortex and probably never even
>heard of PCT. Do you want to claim that they are wronb because they have the
>wrong paradigm? We know alot of visual processing occurs in occipital.

You've put your finger on one of the reasons why I decided in graduate school and later never to take neurophysiologists at their word. We knew (as experimental psychologists) in 1959 about the need for spot detectors and line detectors, and the like, because we got it from the requirements imposed by psychophysical results. We discussed it in our group discussions. At that time neurophysiologists said there was no such thing in the brain, so we said "OK, but it's a functional requirement." A few years later Hubel and Wiesel said "there are these funny specialized detectors in the visual system--wonder what they are for" and got a Nobel prize for it.

This happens over and over in neurophysiology. The psychologists needs a function to explain something, the NP (complete?) says "No way," and a few years later another NP says "Look at this neat thing--wonder what it does." Think of linear correlators, for example (1970s. I think). I can't think of other specific instances at the moment, but I remember many occasions of "Oh, so now they agree that the brain can do what we said it must but they said it couldn't" over the last three decades.

I think the situation is the same as regards PCT and psychology or neurophysiology. If a PCT analysis suggests that some function is necessary (such as the passive parallel monitoring and active quasi-serial control structures), then I will believe it, and I will tend to accept it when people say by other methods that they have found how such structures are made at some other level of description. I will not believe them when they say that they know that these things can't be, or are not, done in the brain.

It's nice to know what neurons CAN do, singly or in groups. But I would never trust a neurophysiologist to tell me what they DO do, and I would not believe one who told me what they CANNOT do.

Martin

Date: Tue Dec 15, 1992 8:23 pm PST
Subject: Presenting PCT

[From Rick Marken (921215.2000)] Avery.Andrews (921216.09109) --

>If someone accepts (a) that they don't understand
>feedback as well as they thought they and (b) feedback mechanisms are
>much more central in the organization of behavior than is generally
>supposed, the rest follows. E.g. that it's a significant achievment
>to predict tracking behavior for one whole minute, that we ought
>to try to find out what Aplysia is controlling for, etc.

Yes. This is a good idea. And your earlier suggestion that we develop models (like the "Little Man") that show the capabilities of the control architecture is also good. I didn't mean to jump on it so negatively. But your comments (even though I misinterpreted them) did jog a good thought; one of our problems in PCT is that we can rarely go to the research results in the literature and say "we have a model that can account for your results better than any other model". This is because most results in the literature are statistical; yet they are quoted as fact. Just after my "Blind men" paper was rejected there was a paper posted through Psycholoquy -- I guess it's the one that was accepted. Unfortunately I did not save it (bad sport that I am) but it was on sentence comprehension or something. Well the article sounded really interesting, deeply cognitive -- about how inferences were drawn from sentences and all. Well, it turns out that they actually described an experiment to test their little theory (it was not really a working model). Guess what. Two groups of subjects read sentences -- and there was some independent variable manipulated (the target of the inference or something) at two levels -- conditions A & B. I can't rememebr the details -- all I remember are the results which were something like "as expected, condition A did significantly better than B ($t= 2.36$, $df=23$, $p<.05$). Well, we know that means that a slight majority of people in A did as expected -- but a lot of people in A did what people in B did and vice versa. But the conclusion that will live on (and be the target of theory) will be that A leads to significantly more of the behavior (whatever it is) than B. There is just nothing for PCT to do with this kind of data. PCT is a working model of "one organism at a time" and we expect matches between model and data that are extremely accurate -- or we go back to the drawing board. We'd be at the drawing board night after night if we used PCT to try to predict this kind of noise.

The more one understands feedback control, the less one sees in the psycholgical research literature that tests that understanding. Ultimately, you realize that PCT is designed to exlain control -- and you can't explain control until you know that it is happening. None of the existent research in psychology provides any evidence that control is happening in a particular situation -- or of what is being controlled. The inescabale conclusion is that we must start the whole enterprose of psychology all over again -- with the understanding that organisms are locked in a negative feedback situation with respect to their environment -- ie. they control.

But we can build snazzy models in the meantime too.

Best   Rick

Date: Tue Dec 15, 1992 9:06 pm PST
Subject: Re: Presenting PCT

[Avery Andrews 921216.1551]   Rick Marken (921215.2000)

I agree that statistics without models is not nice if there is a reasonable prospect of doing better. But I also think that just complaining about it doesn't accomplish much. Like, it hasn't, has it?

Perhaps, naively, I think that if people see concrete prospects for doing better in areas (*not* methodologies, but big questions, like `how do people manage to get their hands to their coffee cups', or `how do people manage to give and get directions for getting to places) that they are interested in, they might go along. But then the powers of obscurantism are deep and potent, so maybe they won't.

Back to my Xwindows fun & games (I've managed to animate a vertical bar, moving horizontally -- a *long* way from a working arm, but here's hoping

Date: Wed Dec 16, 1992 5:29 am PST
Subject: CLOSED LOOP; mollusc of opportunity

From Greg Williams (921216)

I plan to put the papers by Tom & Bill and by Rick in the next CLOSED LOOP, together with the second (concluding) part of the thread begun in the last issue. Rick, please send the latest version of your paper via e-mail. Tom, Bill, and Rick, I'll try to send galleys to you for final checking ca. first week in January.

>Tom Bourbon (921215.0925 CST)

>If I had the skills and techniques
>to work at the single-cell level with Aplysia, the marine snail, I
>would drop everything else and begin a crash program, starting with
>behavioral studies to test whether the creature controls anything.
>After demonstrating the obvious, I would begin a systematic
>analysis of the control-system properties of the creature's
>exquisitely mapped nervous system -- anatomically exquisite, I do
>not trust most of the functional maps. They were created in S-R,
>or I-O, research procedures. If anyone out there is interested in
>a trip to Oslo, I believe I just handed you a ticket.

I don't think you need to do the neurophysiology yourself. What you need to do is sort through the volumes (one big book, one small book, and many papers) on APLYSIA neurophysiology and behavior studies, see what you can find relevant to the creature-as-control-system (reinterpreting results in the light of PCT), and then come up with concrete, detailed proposals regarding exactly what crucial experiments are needed to be done by Kandel and the other experienced workers in the field, given the (I predict) suggestive results already in hand. Don't just claim that "they done it wrong" in vague generalities. How, SPECIFICALLY, can they do better? Hand Kandel et al. the ticket. (Actually, I think Kandel would get a Nobel any way.)

As ever, Greg

Date: Wed Dec 16, 1992 10:30 am PST
Subject: N/A and Oslo

[From Dag Forssell (921216 07.20)] gabriel 921214 21:46

>So. Bottom line. I am a bit uncomfortable about your N/A entries.
>But otherwise absolutely delighted.

Instead of N/A, it is now: "Depends on type 2."

Tom Bourbon (921215.0925 CST)

>If anyone out there is interested in a trip to Oslo,
>I believe I just handed you a ticket.

We can send William T. Powers to Oslo for developing the science necessary for PEACE on earth. Anyone else will have to go to Stockholm, where all the other prizes are awarded.

Tom, It is delightful to have you as a regular contributor on the net. I am very impressed with the quality and relevance of your contributions. Glad you have your own editor and E-mail address, so you are with us all the time and get inspired to express yourself.

Best, Dag

Date: Wed Dec 16, 1992 10:31 am PST
Subject: citations;social;Gabriel

From Tom Bourbon (921216 09:18 CST)

Re: Citations of PCT; Social literature; John Gabriel

If anyone has citations for applications of PCT, in any setting, I want them. Keep my address on file and send or post anything you find. I will be grateful.

For one specific application, Clark McPhail, one of his graduate students, and I are looking for citations of PCT, and any of the half-baked derivatives of PCT, in the literature on the behavior of people in crowds, gatherings or small groups. We also need citations of work in which authors claim that in crowds people lose their minds, come under control of a group mind, lose "the ability to self-monitor," become "deindividuated," and so on. You probably get the picture. Please send anything you can.

Even more specifically on the topic of citations, this is to follow up on a post that might have been overlooked:

>Tom Bourbon (921215 00:48) >>[gabriel to powers 921214 11:35 CST]

>>Subject: PCT and war

>Actually, this is a request for information. The following remark
>occurred during your musings in "PCT and war:"

>>How do you learn to spot those small mistakes soon enough without
>>millennia of Darwin backing you up? Increasingly critical problem
>>when communication works faster, and those who have been applying
>>PCT in advertising agencies have a substantial hand in deciding
>>who gets to be president and C in C. Perhaps it was better when
>>it took three months to go from Oregon to Washington to kick butt
>>of elected rep.

>"... those who have been applying PCT in advertising agencies have
>a substantial hand in deciding who gets to be president and C in
>C."

>I collect citations of PCT and I am always looking for new areas
>in which PCT is applied. I have not seen, perhaps because I never
>looked for them, applications of PCT in advertising. I would like
>to add that specimen to my collection. Can you provide a few
>specific citations and examples?

I am serious. I will appreciate any citations you can provide on people in advertising agencies who use PCT to sell products and to influence presidential elections in the United States. My collection of applications will be incomplete without citations for applications as important as those. Don't keep them for yourself!

Until later,

Tom Bourbon Magnetoencephalography Laboratory Division of Neurosurgery, E-17
University of Texas Medical Branch PHONE (409) 763-6325
Galveston, TX 77550 FAX (409) 762-9961 USA

Date: Wed Dec 16, 1992 10:58 am PST
Subject: RE: N/A and Oslo

From Tom Bourbon (921216 09:40 CST) Dag Forssell (921216 09:38)

No, Oslo is where they give the prizes for the unheard of in science. And to think, I was giving my grown children a hard time about their declining knowledge of geography! Glad to have someone like you who will go ahead and tell me my geographic fly is unzipped!

Warm regards, Tom Bourbon

Date: Wed Dec 16, 1992 12:26 pm PST
Subject: Re: Presenting PCT

[From Rick Marken (921216.0800)] Avery Andrews (921216.1551) --

Well, It's tommorrow for me now too.

>I agree that statistics without models is not nice if there is a reasonable
>prospect of doing better.

With or without models, the data of psychology (by and large) is useless. I think this is a very important point and one worth some discussion. Except for some operant conditioning and perceptual (single subject) data, I can't think of any research results in psychology that would qualify as anything other than statistical accidents -- saying absolutely NOTHING about how or why an individual organism does ANYTHING.

I propose an exercise for anyone with easy access to the psychological literature. Just open a journal randomly to any research article and see what kind of data are collected and whether it could help us understand the behavior of an organism. I really think it would help us see what all the high falutin' theorizing in psychology is based on -- NOISE.

>But I also think that just complaining about
>it doesn't accomplish much. Like, it hasn't, has it?

I am not complaining about this at all. In fact, I have done quite a bit of PCT research; I even have a collection of papers describing it -- the "Mind Readings" book . I have been able to fool several journals into publishing this work by explaining how it addresses psychologists' concerns (like how coordinated behavior works) even though the studies themselves have seemed rather strange to the editors. The only complaint I have is not having enough time to do PCT research -- I have to make a living after all. I guess another complaint is that nowhere near enough people have bought "Mind Readings" -- if they did, more people would see what the beginning of a PCT research program looks like and I'd be able to remodel my backyard.

The fact of the matter is, I don't care if psychologists (and other life scientists) get it or not. I bring the point up (about the fact that PCT requires RESTARTING psychology from scratch) only to let those who are interested in PCT know why it might be frustrating (and counter-productive) to try to apply PCT to psychological data collected in the "old fashioned" way.

In fact, the result of this effort (if not abandoned) is what I will call Carver/Scheier PCT or C/S PCT (actually, it's a very appropriate name -- Chicken S**t PCT). This is the kind of PCT where people use some of the terminology of the model -- but not the essence (control of perceptual input variables). C/S PCT misses the whole point of PCT -- which is necessary if you are going to use PCT to account for statistical results of conventionally conducted reseach. I don't mind if people do this -- it just makes things confusing (to the audience, not to real PCTers) so it's annoying.

I know that complaining will get us nowhere. I've been uncomplainingly doing PCT research and modelling for twelve years (which does get us somewhere, I hope) and I'm still doing it. That's what I want people to help out with -- but I also know that in order to do good PCT research they MUST ignore most of the existing psychological research. Not doing the latter is the path to C/S PCT -- which is the path to hell.

So consider my cautions about the value of existing psychological research a WARNING, not a COMPLAINT.

Best   Rick

Date: Wed Dec 16, 1992 1:02 pm PST
Subject: Re: Imagination in planning

[Martin Taylor 921216 11:30]   (Bill Powers 921215.1530)

Bill, I think my conscious impression of what one imagines during planning is very like yours, and I can imagine both your and Mary's version of the golf ball in the Coke bottle. But none of it addresses the point that I wanted to raise. Perhaps I can try another approach.

All the perceptions involved in the conscious imagination are, by hypothesis, obtained from perceptual signals in the same ECSs that would be involved in actually executing the plan. When the plan is executed in the real world, these perceptions are derived from sensory signals that reflect the reactions of the world to the actions evoked by the outputs of the ECSs at the various levels involved. When the plan is executed in

imagination, the perceptions are derived from sensory signals that originate in some other place.

Whenever this question has been discussed, on CSG-L or in BCP, that "other place" has been either a memory store or a looped-back output signal that normally contributes to the reference signal of a lower ECS. In the first case, the "sensory" signal is one that has occurred before in the real world; in the second, it is what would be sensed if the reference to the lower ECS were to be satisfied by the actions of that ECS controlling its perceptual signal. But in the real world, some imposed constraints may prevent that lower ECS from bringing its percept to its reference level. In the planning situation, these constraints have never yet been encountered in the real world. They are imagined as a consequence of something else, such as verbal instructions.

In the versions of imagination that I have seen discussed, I see no place for simulations of a real world never experienced. It begins to feel as if some construct other than an ECS is necessary so that a variety of imaginary-world impedances can be placed in the imagination loop, to match the impedances that might be found in the real world for which the plan is being made.

It is important for the survival of the organism that the possible variety of impedances be insertable in the imagination loop, especially in situations where there is danger (implicit in some of the possible impedances that might be encountered with different action choices).

I still feel that the necessary structures may exist within the hierarchy, but I can't find them. Pushing the question to lower levels only moves it; the problem stays the same.

Martin

Date: Wed Dec 16, 1992 3:22 pm PST
Subject: neuroscience

OK--I figured out that I gotta limit my message length.

First, I never saw Bill's post to me--I don't know if that was my fault or the computer's. Second, when I speak of levels I am speaking not of perceptual levels but levels of analyis--like molecular, biological, psychological: that sort of thing but not quite that distinct. I can describe what is going on in terms of neurons, or systems of neurons, or go lower and talk of the chemistry, or go higher and talk of the "computational models" (PCT). I don't have to know much abut chemistry to have a good computational model and vice versa. So when I say that PCT is at a different level than neuropsych, this is what I mean. At one level there are goals, at one level there are neurons firing--nothing new. (I don't remember what I was saying in relation to this, however, unfortunately).

Third, I completely agree that I can't equate function with place--that's what I tried to write before and its what I am writing in my neuropsych final--but you got to start somewhere. If I was doing a memory experiment (and by the way, neuroscientists are one up on this topic over PCTers as far as I can tell) I would do it exactely the same [switch to new post]

Mark

Date: Wed Dec 16, 1992 3:48 pm PST
Subject: Re: citations;social;Gabriel

[John Gabriel to Tom Bourbon 921216 11:54 CST]

>If anyone has citations for applications of PCT, in any setting, I
>want them. Keep my address on file and send or post anything you
>find. I will be grateful.

>I am serious. I will appreciate any citations you can provide on
>people in advertising agencies who use PCT to sell products and to
>influence presidential elections in the United States. My
>collection of applications will be incomplete without citations for
>applications as important as those. Don't keep them for yourself!

Well, not the firestorm I half expected, but the seemingly innocuous request for citations. CAPIVI. I am speared. BUT

There's a difference between doing PCT in detail, with all the hierarchy, about which I have some fairly serious doubts because there's so much ECSs seem to me unable to accomplish without interactions so extensive that the interactions become more important than the ECS's, and Bill's wonderful insight with which I completely agree, that people or any other organism, or even state machine, do things because of their perceptions, (for state machines, read inputs) or sometimes to change their perceptions. Carelessly put - In order to change some of their perceptions is the way the previous thought should have been stated.

If you want a PCT based statement of policy for an advertisng agency, I don't have it, and don't plan to waste time looking for it.

If you deny that one of the purposes of advertising is to change perceptions of products, political candidates, or other things, so that the behaviour of those viewing the advertising is changed, I think you are very likely wrong, and I believe others will agree with me.

Now, I'm not saying the hierarchy is nonsense, it isn't, but I only believe it somewhat more than I believe in the Id and Ego. It's one of many at times useful approximations to the truth, which we do not now know, and which it can be reasonably argued neither we, nor our intellectual inheritors - I've phrased that carelessly, but no matter - may ever know.

There does seem to me a very convincing explanation of puzzling things in the reliability of people in doing jobs like picking up glasses of water, or following randomly moving cursors. Following the cursor has very clear reference signal - the cursor position, and I have no trouble understanding it. Picking up the glass of water I have some trouble tracing all the signals, but I think a cogent case can be made that they have homomorphs somewhere in the neuro- anatomy and I think a homomrorphic finite state automaton (a computer and software) might very well be built to mimic the process of scene understanding, and picking up the glass without spilling it.

I've been building feedback systems since 1941, and doing their mathematics since 1946 - the little book by Carter published in the UK on Heaviside's methods that my father and his colleagues used to teach people who went on to build radars and gun turrets, so I

think I understand control and feedback quite well. I don't know the literature of neuropsychology and human behaviour at all, and have only a nodding aquaintance with the literature of animal behaviour and population genetics, and some of those other things that involve feedback mechanisms.

So, I don't know your world very well, and I apologise for treading on toes when I do so. I do know a little about the state of the art in UAVs - the 1 ton equivalent of picking up the glass of water, and I know that the PCT hierarchy is not yet quite satisfactorily implemented in that area, and I think that when SOMETHING is done that really works, it will owe to Bill P. and all of you. I gratefully acknowledge that debt. I think it will have some other important things in it besides the current content of PCT. I'm glad and proud to be in the group if I'm acceptable, I admire you all.

I'm disinclined to chop logic about detail that I think will be replaced by other better insight.

Now let me say once again what I AM interested in because I may be able to explain my mild irritation with total missionary zeal for PCT.

I think the following things are well founded, and unlikely to be replaced except possibly in the sense that Einstein replaced Newton, and I reemphasise that Newton is good enough for a lot of Govt. work provided you stay outside the fence of Los Alamos - to make a terrible pun.

1. The mathematical theory of linear feedback systems as put forth
   for example by Bode - Network Analysis and Feedback Amplifier
   Design 1945 Van Nostrand. If all the participants of CSGNET
   had read and understood what Bode has to say, there would be
   less confused argument about reference signals and dp. But it's
   not fair or reasonable to ask that, any more than it's possible
   for me to have read to any extent in psychology. There are
   other books easier to read, and less adequate in dealing with
   multiple feedback loops.

2. The idea of classical contact transformations, as first put forward
   by W.R. Hamilton. The classical contact transformation is the
   operator that takes system state from that at time T to that
   at time T+dT. It is itself state dependent, but quite clearly
   its eigenvalues and eigenvectors now tell you a very good
   approximation to system state a moment from now, given the
   present state. This idea becomes the idea of a semigroup for
   systems having discrete states, and the symmetries of the
   system are the group of operators commuting with the semigroup.
   This path of investigation quickly becomes a mathematical
   mountaineering feat, which is a pity, because I think it is the
   proper theory of Gestalt, and perhaps some other things. It is
   my ambition, one day, to "push these ideas to their limit" as
   Avery Andrews has said. And if I do, I'll try to hew a more
   accessible staircase to the peak.

3. The idea of the rate of information transmission down a discrete
   channel, being the the upper bound of the number of binary
   decisions a recipent can make in a second, and first put
   forth by Claude Shannon in the two 1949 papers in BSTJ. These
   are eminently readable for those who know high school algebra,

provided the discussion of continuous channels is left on one side. This idea is very important, because "information" is what travels along feedback paths, and "decisions" are determinants of conscious behaviour. Thus, Shannon provides framework much simpler than Bode, for discussion of very general theories of control in the style of PCT. Although Shannon does not discuss stability, I think stability is only an important side issue as compared to information. Certainy in Bill's book information in the form of reference signals is central, stability is usually assumed, in fact I am not sure the issues in positive feedback systems as implementors of memory are even discussed.

4. The ideas of conventional Decision Theory and Value Systems are an adequate first approximation to human behaviour in decision making to support qualitative analysis, and if estimates can be made about values, quite good quantitative analysis.

Thus, for very general rather abstract theories, still having concrete numerical consequences once you understand the parameters, itesm 2. 3. and 4. seem to me a sufficient foundation for discussion of decision making in a wide variety of human activity. And Bill P's obervation that you can only act on what you perceive goes to the heart of matter. It is exactly why the light that Shannon sheds on reliable perception is important.

Thus, decisions and behaviour in societies of one kind or another, seem to me driven by information in exactly the sense that Bill observes, when he says we can only act on what we perceive, and on reliable information in the exact sense of the theories of Shannon and Bayes, together with the kind of searches of solution spaces performed in any one of a number of theoretical frameworks, from resolution based theorem proving a la Herbrand, and Wos, optimisation after the style of many people, e.g. M. J. D. Powell, or J. More' and other more outre' approaches to the problem of discovering new things.

I am very much interested in the problems of making wise decisions, value systems and objective functions for searches and so on.

I think these things are a long way away from your interests, which makes fertile ground for misunderstanding. And perhaps we don't have the same ultimate objective after all. Very likely, as in the problem of the Spanish Barber, we shall just have to follow our ideas around and see.

Please forgive my legpulls on Bill P. He's old enough to stand up for himself anyhow. Also I confess with some pride to interest in things military, and a concern to keep casualties on both sides of armed conflict to a minimum. If you decimate an enemy his descendants will be your enemies for generations, as Tom Baines points out. Thus to make Bill C's point once more I may be concerned with things military, but I am not on the side of slaughter or armed dictatorship. A Citizen Army is the fail safe control system for Eric Linklater's description of democracy - "A well shod electorate, and Govt. with a tender backside."

Best John Gabriel (gabriel@eid.anl.gov)

Date: Wed Dec 16, 1992 3:56 pm PST
Subject: neuroscien cont

I would do memory research just the same. Just because we would describe it differently, does not mean that we would investigate it differently. We would investigate differently if we were working at higher levels of analysis (such as social psych research) but not at levels below. For example, if you wanted to know about neurochemistry sorts of things during a task, you don't care whether percpetions or output is controlled--its just not relevant at that level. If you want to know how people learn to play tennis, its very relevant. Wouldn't you want to know whether the monkey could remember under which bowl the food was located? I agree that faulty paradigms lead to faulty data but its not necessarily true here. For another example, whether or not you are right about whehter God exists or not is not going to change whether your PCT research is valid.

Maybe I should ask if PCTers are evn intererestd in mapping brain functions. If we are--like it or not, lesion data and brain scand is all we got. Concering analyss of such data, I agree with what Tom said about how we cannot say area z is responsible for funciton x (I just wrote that in my neuropscyh final) and I agree that we should be asking whether the lesion related to input, refernce, etc, and I agree that PCT could help neurosciene--that's my point. But I don't think we should shun present research--we would do it the same and interpreet it diffently.

Mark

Date: Wed Dec 16, 1992 4:45 pm PST
Subject: Error control (again!!!!)

[From Rick Marken (921216.1200)]

I'm bringing this to the net from a couple of offline exchanges with Martin Taylor. Martin said I could and I think it's worth- while. Maybe by opening this up to other minds we can clarify things. Here is some background:

Martin said:

>Bill's model doesn't control the derivative of the squared error, in any
>normal sense.

I said that it does. The derivative of squared error (de^2) is one of the intrinsic variables controlled in Bill's model of reorganization.

Martin said:

>Now what is controlling what? I think what is being controlled is the value
>of some intrinsic variable.

And I said

>de^2 is the intrinsic variable being controlled relative to an intrinsic
>reference which happens to be 0;

And Martin said:

>This makes no sense to me. In words, you are saying that whatever the

>error is in the intrinsic variable (say blood CO2 level, for example),
>what is controlled is that this error should not change. To me, the
>only thing that makes sense is that there is some reference level for
>blood CO2, and if this is too high or too low, and particularly if it
>is moving in the wrong direction, then the main hierarchy needs reorganizing.

Martin is missing my point. I assume that de^2 is the output of a perceptual function whose input is error signals from many different control systems in the hierarchy: de^2 = d(e1+e2+....en)^2 say. This signal (de^2) is compared to a reference value, r, which is intrinsic because it is set by evolution -- it is not part of the regular perceptual control hierarchy -- de^2 is a perceptual measure of "how well the hierarchy is doing". The error signal that results from subtracting de^2 from r (r - de^2) is converted into an output signal whose amplitude determines the probability of a random change in some parameter of the VERY SAME control systems whose error signals (e1, e2, etc) contribute to the value of de^2. The change in parameters might improve the performane of the control systems (reducing the error in the systems and, hence, bringing de^2 closer to the reference, 0) or it might make their performance worse (more error, increased de^2, bigger difference between de^2 and r and greater probability of another change in parameters soon).

I think this is something like how Bill's reorganization system works -- it improves performance in a set of perceptual control systems by changing the parameters of those systems in order to keep a perception of the ambient error in those systems low. In this case, the measure of ambient error in the lower level control systems (de^2 -- if that's what Bill is using) is the controlled INTRINSIC variable.

There are probabaly MANY intrinsic variables that are controlled -- (like your example of CO2 level) and they are probably controlled by reorganizing existing control systems. But it is quite likely that the level of error in the control systems themselves (what we subjectively experience as stress, I would guess) -- possibly perceived as de^2 -- is one of those intrinsic variables.

Martin says:

>But e*de/dt is not a controlled variable, any more than (in fact less
>than) is the error in a normal ECS.

It may not be a controlled variable in your model -- but in the context in which I was discussing it (and I'm almost sure in Bill's reorganization model too) IT IS. At least, it CAN be a controlled variable. I think Tom Bourbon did some modelling of this kind to. Martin, I think you are confusing the error signal in a control system (any control system -- regular or reorganizing) -- which IS NOT CONTROLLED -- with a perceptual signal (in any control system) which might DEPEND on error signals as INPUTS to the perceptual function which produces the perceptual signal; this signal CAN BE CONTROLLED.

Martin says:

>Another way of seeing that d(e^^2)/dt is not controlled is to note that
>zero is not a reference level for it. Negative values are even better,
>becasue they show that the intrinsic variable is really being controlled.
>A zero value is neutral in this respect, in that the error in the intrinsic
>variable may be large but unvarying, or the error may be small while changing
>wildly. Neither indicates good control, but both are compatible with good
>control (momentarily).

Well, this is all quite unclear to me.

>If you want to put it on the net, collect all the postings and send them
>out as one. But I don't think it worthwhile. I haven't seen anyone else
>that seems to be interested in the matter (except presumably Bill, by
>inference)

Here it is on the net. There may, I hope, be others interested who just don't feel like commenting. But I think it's worth it to discuss this in public because it seems like you either have a very different understanding of how reorganization (and control) work than I do or we're speaking very different languages. The language problem could be overcome if you just show me your model.

Regards   Rick

Date: Wed Dec 16, 1992 4:50 pm PST
Subject: Gang Warfare

[From John Gabriel 921216 13:00 CST]   >(From Bill Powers (921214.1900 MST))

THIS POST IS FAR TOO LONG. Those who want to get to the thing I REALLY want say that's new should turn to "I dont' disagree with you. I just have....." about 25 or 30 lines before the end.

>>>A real science of human behavior is the ultimate protection against bullets.

>>Think on that. And Heinlen's remark that an armed society is
>>a polite society, also on Roy Chapman's rule "Never point a gun
>>at anything/one unless you are willing to take personal
>>responsibility for its' their destruction."

>I'm afraid that despite his enormous story-telling skills, I
>eventually came to regard Heinlein as being stuck in an adolescent fantasy.
>What he called a polite society was simply
>one in which everyone tried not to give offense in order to
>avoid being killed: a society based on fear (mislabelled
>"respect"). I think I would rather live in Dodge City 1992 than
>in Dodge City 1872. My idea of a grown-up society is one in
>which everyone respects the will of others because that's the
>only social system that makes any sense. Not because they see a
>gun pointed at them. That basis lasts only as long as it takes
>for the other guy to turn his back.

Yes, I agree with you in lots of ways about Heinlen, and yet he makes a not unreasonable point if you think about his background.

>As to Roy Chapman's rule, lots of people just love it because it
>gives them the feeling that other people's lives are theirs to
>spare or waste as they will. Why is being willing to take
>responsibility a good enough excuse for shooting someone? As if
>you could avoiud the responsibility by not acknowledging it.
>Frankly, I would be afraid to suggest to someone with a private
>arsenal that guns should be taken out of private hands. I would
>be afraid he'd shoot me, and be willing to take responsibility

>for it.

Well, Col. Chapman does not want students who don't understand that lethal force is serious business in his school either. But if you work for the FBI, or for parts of the Army, the things Roy teaches are statistically likely to save your life. And for ordinary citizens like the lady in the cafeteria in Texas who could not draw her gun from her handbag in time to shoot back at the chap who killed the ordinary citizens, and would have preferred to have it holstered where she could get at it, the no open carry law condemnned her to spend the rest of her life in a wheelchair.

But perhaps we have to stay out of the question of how to determine who is a responsible citizen enough to be alolowed open carry.

I have not been through Roy's school yet, I have been through one taught by one of his instructors who was in charge of all firearms training for the police in a big city for 20 odd years. If I can show I am of as good moral character as good policemen, and I am perhaps slighly better trained, why should I be any more danger to you than is Tom Baines or your local police, even if I do go armed. (I don't by the way, except to shoot at paper on approved ranges). But if open or concealed carry were allowed in Illinois, I might well go armed if I were driving where carjacking is common.

But we can continue this argument indefinitely, and probably not get any further. If Roy graduates somebody out of his school, I in fact feel safer with that person beside me armed. He or she is just backup for the police force I think we all agree to. Let's continue offline if you like.

>>I can't tell if you've read Peterson and the Altmans. The thing
>>that makes them interesting to me is the way the society where
>>the carnivores can do real harm to each other, but don't
>>because if they do they will be too torn up to catch the next
>>moose, moves gradually towards the chicken run where nobody has
>>claws and teeth, and it's fatal to be crowded and at the bottom
>>of the pecking order.

>I have read them, but not seriously. I eat meat, but not that of
>my own species. I'm not sure we know why lions don't enter into
>lethal contests very often. I might guess that it's because
>fighting hurts, and lower animals are too stupid to think up
>reasons to keep on doing things that hurt.

NO! THERE IS A FEEDBACK SYSTEM. Those wolves who are disabled by internal quarrels in the pack, die of starvation, as does the rest of the pack, and their quarrelsome genes are not so strongly perpetuated. In fact there's an even stronger mechanism. Most pups are fathered by the Betas, the Alphas are too busy with the duties of pack leadership. Just as well, If you train, keep, or breed dogs, you know that two alpha male personalities, or even more so two alpha females in too small a yard need very careful management by the "super alpha" i.e. you, to prevent fratricide. Darwin always triumphs in the long run.

>>Let me say one thing that may
>>bring down a firestorm. Before the railroad and the telegraph
>>and good maps (Gauss's contribution) by and large wars had a
>>not unreasonable ecological function - they closed down
>>incompetent governments.

>But it takes two incompetent governments to get into a war. The

>government that wins is more competent at only one thing:
>winning a war. That's not much guarantee of competence in
>anything else.

NO. The German decsion to march into Poland 12 months after Munich was unilateral, and it did eventually destroy the two governments that ignored the rule of the wolf pack. It also brought down the UK Prime Minister who had run the war, after the war was ended, and probably a good thing too - otherwise India would have been an even bigger mess. And if the UK had not declared war when they did (but not in 1938 - Watson Watt only just got radar going in time as it was) I might have been killing sentries with a borrowed rifle, as well as my Corsican friend.

>>How to close down an incompetent government without
>>unacceptable cost in lives, misery, ... Trade wars perhaps.
>>Better to be laid off than killed or badly mangled.

>That's the problem as I see it: that there always seems to be an
>acceptable cost in lives, misery, etc. "Sure, we'll get our hair
>mussed, but we'll lose maybe ten million, twenty million TOPS."
>(Prof. Buck Turgidson).

I think you missed my point. I agree the costs have clearly been unacceptable since about 1870. There's a lot of European history and economics back of the statement. Yes, the 100 years war was a frightful affair, as was the Mongol invasion of China, and so on. But it was not on the scale of even the American Civil War. Things are not as simple as PCT would suggest. Yes, perhaps the zero sum game arising from population pressure might have been alleviated earlier by the scientiufic discoveries that often came a generation after European wars. But I doubt it. Exactly the same phenomena that annoy you so badly in trying to get PCT published, work against other innovations too. And the pressure of war breaks down the barricades. This is really Tom Baines' story, and we should ask him to tell it, but there is a great difference between a peacetime army, and army fighting a "phoney war", and the real thing which only settles into its stride after some 12 months and is strong on inovation, and weak on bureacracy.

Perhaps that's just what you mean when you say "If you have to fight a war - get on with it and do it right" But things are different - a squad today with M16s has more fire power than one of Wellington's regiments, and events move correspondingly faster.

But TOTAL war (i.e. the involvement of a whole nation) only dates to 1914, and indeed the French and British misjudgement was partly about how many conscripts the German Army could mobilise quickly. The railroad and the telegraph made the changes that thoughtful military people could perhaps see from the American Civil War, and the Franco Prussian War. We can argue about pre 1800, but we all agree pretty much about 1914 and 1939.

I'm not saying that wars were ever "fun", but I do think that perhaps once they served a useful purpose in human ecology. Forest fires aren't "fun" either for the deer and other small animals that get caught, but my friends who know the business say you get to choose between the fairly frequent small ones we used to have 100 years ago, and today's, which when the start are fed by half a century of dead wood.

In 1940 or 1941 the US Army undertook the Louisiana Maneuvers to prepare for the inevitable involvement in Europe. That experience saved this country's young men from the bitter lessons learned by the BEF in France, and NZEF in Crete.

If you want to crack the code for what the Gang of 5 are up to, it is to introduce the kind of innovation that usually requires bloodshed (you own or your allies) to persuade an army not to fight last time's war. And until war and policemen are no longer needed - you see we do agree with you part of the way - that would be a good thing. I expect the gang of 5 will continue. Just as in the summer of 1938, my father spent his time with a Brunsviga hand caculator making new Gunnery Tables for the Navy, instead of grading School Certificate Exam Papers in Mathematics.

Unlike the situation in 1938, we don't actively EXPECT a major conflict. But we think it likely that our president will find it necessary to send young Americans into danger in places like Somalia. And we want them to have the best possible chance of coming home unharmed.

>>Completely agreed - how do we keep people from wanting to shoot
>>at each other? No, I don't want to say that - don't believe in
>>police/baboon state.

>But that's exactly the question. The way we approach it with
>HPCT is by studying the hierarchical goal structure.

OK. Now I think we agree. I went through all this in my reply to Tom Bourbon of an hour or two ago. But I have some serious doubts, set forth loc. cit. about the hierarchy, and some suggested alternatives.

>>Why do people want to shoot at each other, and WHAT might make
>>them want to stop?

>Now you're on the same track that I'm on. Only the problem isn't
>to make them want to stop. It's what they're trying to
>accomplish at a higher level by shooting. The basic strategy is
>to find out what they want and figure out how they can get it by
>a different means. Nobody (sane) has a highest-level goal of
>propelling a bit of lead into another person's body. That's just
>a means to an end. It's basically a very inefficient means,
>because it gets other people mad and they tend to start shooting
>back at you (whether they're the Good Guys or the Bad Guys).
>Then everybody gets hung up on shooting and they forget what
>they originally wanted, and like as not destroy any possibility
>of getting it anyway. Look at Somalia. That's a polite society?

>It's a process called "going up a level." You keep doing it
>until you reach the level where something is still free enough
>to change a goal. If someone wants money or power, you ask what
>he wants it for. And then you arrange for him to get it without
>the money and the power, or at least without unreasonable
>amounts thereof. Whatever people want, they usually want it for
>a higher reason that they've lost track of or never really
>worked out. Or, on second thought, haven't really cared that
>much about since they were teenagers.

RIGHT ON, except what about the people with whom compromise is not possible. Like the guy with the AK47 in Stockton. See V. Clausewitz, or the courts of law or any of the other machinery we have to resolve disputes where individuals cannot agree without some kind of arbitrtration if it really isn't a zero sum game. Or war, if it is, and lives are at

stake anyhow. This gets very quickly into ZPG vs the Horn of Plenty theory of support for scientific research, subsidy to the Steel Industry or whatever.

>Picking up loose end from yesterday:

>>Bill, I read, and I think I understand you. Put very simply,
>>you are saying that a human being can follow a random movement
>>of a cursor with a finger, and that this is explained by
>>control theory.

>Yes. This can also be put somewhat more generally. A human being
>can control a visual (auditory, kinesthetic, etc.) perception by

.....

>You don't have to lay out the whole hierarchy in order to
>demonstrate how the model works for a single control process at
>any level you please. Pick any perception a person can affect
>with muscles. Get the person to pick a reference level for it.
>Get the person to control it in the presence of disturbances (in
>a constant state, please, to accomodate the experimenter). Match
>the model to it and thereby measure the parameters.
>bet a considerably chunk of my life (and have done so) that when
>you've found all the different control processes you can, there
>won't be much left over.

I don't disagree with you, I just have trouble with some of the extrapolations. Let me put it like this. The Bohr quantum theory of the copper atom eventually explained quantitatively the characteristic green colour of the Cu flame spectrum (but only as late as about 1950, and only then by including electrostatic contributions to total energy that have no no classical or non relativistic explanation) The properties of copper as a metal arise only when these interactions are more important in the physics than the Bohr atom. I feel the same way about ECSs and straightforward control theory. I doubt it explains the Stock Market prices, the guy in Stockton, or even provides all the insight a CEO needs to run a company or a General a campaign. You are the H. Bohr of PCT, and nobody will ever be able to take that away from you, but we still need our Schrodinger for the theory of metals, and Dirac for the theory of electron spin. To say nothing of Weyl, Bethe, and van der Waerden for my favourite soap box of symmetries. But those are all opportunities, not threats to the beauty and power of the things you have already done. We acknowledge and celebrate your achievements.

You know, I think we can argue a long time about the different projections we see of the same "real" thing on the wall of Plato's cave. But I also think we probably agree well enough "for Govt. work" And as Bill C. remarks "There aint no such thing as ground truth." And perhaps thoughts in the mind of God, or "of a higher reference system."

>Of course what's left over will be very interesting.

Absolutely. There are more things I want to say, but I've said them already in a post for Tom Bourbon, so I don't need to clutter up the net a second time.

John Gabriel (gabriel@eid.anl.gov)

Date: Wed Dec 16, 1992 5:41 pm PST
Subject: why rather fight

[From: Bruce Nevin (Wed 921216 16:08:13)]

> [Martin Taylor 921215]
>
> Why . . . do most species stop fighting short of lethality, whereas humans are among the very few who don't? The argument from integrating output would seem to suggest that all species should fight to the death in the case of an unresolvable conflict (can't both have this doe).
>
> Giving up seems to be the most common way of ending conflicts. One participant "decides" that winning is unlikely, and concedes. Humans are considered wimpish, cowardly, poor specimens, if they follow that sensible rule. I would have thought that if humans have more levels of control systems in their hierarchy, they would be more able to avoid or resolve conflict than would other species; at least a naive application of HPCT would seem to lead to that conclusion.

A way to an answer lies I think in someone's (Gary's?) questions and observations a year or so ago about how a person could ignore or override perceptions that in imagination anyway result in intrinsic error. Bill argued then that the image of heroic resistance to torture is a myth.

If the situation is simply a squabble over who gets to eat or mate on this occasion, it is easier to back down. If winning on this occasions constitutes a change or confirmation of desired status as one perceives that one is perceived by others, it is harder. It might be unbearable to be thought a coward, or a traitor, or a wimp, and the imagined consequences of that social assessment (a sure thing) might outweigh the imagined consequences physically of getting beat up losing a fight (perhaps perceived in imagination as not so sure a thing).

The higher levels of the hierarchy have to be there for this. I think we see a fair amount of this in mammal and primate societies. But the elaboration of status and of institutionalized consequences associated with status that we humans have developed depends I think on language.

Sorry I can't spin that out more clearly. Hope it's enough to get the point over. Gotta run,

Bruce bn@bbn.com

Date: Wed Dec 16, 1992 5:42 pm PST
Subject: Reply to Rick Marken I think.

[Gabriel 921216 14:59CST]

Rick, I think you asked what were the feedback paths for the wolves and the baboons. I think I'v answered in two long posts one in reply to Bill and theother to Tom Bourbon.

They are not what you are asking about, which is why I'm interested in more general things than the simple control theory of Arm.

But there are feed back paths. They just change the Markov matrices in the population genetics to select against excessive quarellsomeness in wolves, and excessive carelessness in baboons.

There are some intererestin phenomena in people too. I expect everybody on the net knows the Outward Bound program. In New Zealand we sort of ran our own, mountaineering, motor cycle racing, and other such things - always done on a shoestring - I could spend three summer weeks in the mountains for less that it cost me in room rent and board when I was a summer student at NZ's equivalent of NBS. And the bike I raced was older than I was, and I learned a whole lot of machinist skills I would never have had otherwise keeping the damn thing running.

Anyhoo The bottom line in all those things was that screwing up was surefire guaranteed to get you hurt, and could quite easily get you killed. And it wasn't just making an error of judgement about a corner or a hold. Ignoring a frayed throttle cable on the grounds that "It'll do another race" was just as serious even if it meant not going to the cinema so that you could pay for parts, or infinite patience coaching a chap who played Rugby for the National Football team through his B.S. in mathematics, so as to pay for parts to rebuild front forks that were going unstable (see I even learned my control theory the hard way).

So you learned the "sense of personal responsibility for your actions" that Bill P. took exception to, the hard way. Just like the wolves.

Some place in my background there's a quote that's bound to raise more hell on the net. Probably from some NZ ex serviceman who had been in Crete and in prison camp after that, whom I taught mathematics in 1948. Only the guys who've been shot at have the right to originate these kinds of remarks, although those of us who have led more quiet lives can quote them - "All the fun of War, most of its' Discomfort, but only 1% of the danger." - about mountaineering.

John (gabriel@eid.anl.gov)

PS Now, I gotta stay off the net except for business for the Gang of 5, or I never get any work do done. Midnight oil for me until 2 AM I'm afraid.

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 indeterminancy 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: Wed Dec 16, 1992 6:54 pm PST
Subject: Re: Error control (again!!!!)

[Martin Taylor 921216 17:40] (Rick Marken 921216.1200)

Rick,

Neither you nor I can talk for Bill. But since you are quoting your interpretation of what he says, I will, too, since neither of us think we are altering his model.

When I said it was OK to put our exchanges on the net, I said to put it all out together, precisely to avoid the kind of selective quotation that you did put out.

But since you have excluded most of the background and interpretive context, I guess I have to try to make it intelligible.

The argument was roughly (as seen from my side): if there is an intrinsic variable (i.e. a variable outside the main hierarchy, in a Bill-P-type reorganization hierarchy) with a reference level R and perceived level P, giving error E, then I assert that the controlled variable is P. Rick asserts that it is d(E^^2)/dt. The output of the control system for this variable is a stream of events that cause reorganization in the main hierarchy. I assert that Bill P has simulated reorganization with the rate of this stream proportional to d(E^^2)/dt if that is greater than zero, and zero if that is negative. Rick asserts that the control level for d(E^^2)/dt is zero. I don't understand that, because if the error is decreasing in absolute magnitude, the variable is probably under control (there could be a coincidental helpful disturbance, which is why I say "probably").

That's the essence of how I see the issue.

Now what might be an intrinsic variable. I used blood CO2 concentration, but I'm quite happy with overall error in the main hierarchy. But if overall error is to be used, Rick's formula won't work. The squared sum of algebraic error is only a measure of the current dynamical disturbance in the net, and errors introduced by a saturating positive feedback loop (and they all must in practice saturate) will be swamped by the large number of actually controlled loops that are responding to current disturbances. So the derivative of the squared sum of algebraic error will not even include the effect of the loop that requires reorganization.

I think that Bill and I have previously accepted that the sum of squared errors (not the derivative of that sum) might well be an intrinsic variable. We differ, I think mildly, on how localized a chunk of the hierarchy this kind of sum is likely to be taken over. If the sum of squared error is an intrinsic variable, its controller may well have a reference value of zero. But in no way could its derivative be an intrinsic variable to be controlled, because the only suitable reference value would be minus infinity (the hierarchy is controlling superbly).

Whatever intrinsic variable might be under consideration, I think we agree on how Bill sees the reorganization happening. A reorganization event changes something in the main hierarchy, which may or may not improve the control of the intrinsic variable. If it does not, then another reorganization rate is likely to happen relatively soon. If the intrinsic variable now turns out to be under control, no more reorganization is likely to happen soon.

So I think that, as I said to Rick in one of the parts he didn't quote, our disagreement is of words, not models. Only now I realize that the words at issue were not what I thought they were. Rick was talking about error in the hierarchy AS an intrinsic variable, but using an expression that couldn't work, whereas when I used the word "error" I was referring to error IN an intrinsic variable (which might well be overall error magnitude in the main hierarchy).

>The language problem could be overcome if you just show me your model.

I hope that I have shown it to you, and I hope you see it as the same as Bill's (and I hope Bill see it that way, too). That's because I was not trying to discuss MY model (which is a little different) in our private interchange, but Bill's.

Martin

Date: Wed Dec 16, 1992 6:58 pm PST
Subject: "a hierarchical network of control systems" ?

[I am still often surprised at how little an abstract tells you. And it's fascinating how a small variation in word choice and arrangement can allow a large variation in interpretation. Is it possible that Harry Klopf is talking about anything like HPCT? Is he already on this net? Ray Allis 921216.1600]

> Alife Digest, Number 091
> Wednesday, December 16th 1992
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> ------------------------------
>
> Date: Wed, 16 Dec 92 12:43:41 JST
> From: degaris@etl.go.jp (Hugo de Garis)
> Subject: SAB92 Report, Hugo de Garis (ETL, Japan)
>
> Dear ALifers,
> Here is a report on the recent SAB92 conference in Hawaii.
> Hope you can use it in your ALife email network.
>
> Cheers,
> Hugo de Garis.
>
> SAB92 Report
>
> Simulation of Adaptive Behavior SAB92 Conference, Hawaii, 7-11 Dec 1992.
>
> by
>
> Hugo de Garis
>
> Electrotechnical Lab (ETL)
> Japan

[couple hundred lines removed ...Ray]

> Harry Klopf's (Wright Lab) "Modelling Nervous System Function with a
> Hierarchical Network of Control Systems that Learn". (Part of) Abstract -
> "A computational model of nervous system function during classical and
> instrumental conditioning is proposed. The model assumes the form of a
> hierarchical network of control systems. Each control system is capable of
> learning and is referred to as an associative control process (ACP). Learning
> systems consisting of ACP networks, employing the drive reinforcement learning
> mechanism (Klopf 1988) and engaging in real time, closed loop, goal seeking
> interactions with environments, are capable of being classically and

> instrumentally conditioned, as demonstrated by means of computer simulations".
> This paper felt important, and that it made more than the usual incremental
> contribution to the field. Klopf's neural network hierarchies actually LEARN.
> If there had been a best paper award, my vote would have gone to Klopf.
>
> End of ALife Digest
> ********************

Date: Wed Dec 16, 1992 6:59 pm PST
Subject: Re: citations;social;Gabriel

I'm going out of town until next week but before I go I've just got to make one quick comment on:

John Gabriel to Tom Bourbon (921216 11:54 CST)

>And Bill P's
>obervation that you can only act on what you perceive goes to the
>heart of matter. It is exactly why the light that Shannon sheds
>on reliable perception is important.

I think Plato predated Bill P. in this observation. Bill P.'s important observation was that perception is one link in a causal loop in which we are all locked -- a loop that obviates Shannon's concerns about reliable transmission since, in living systems, perception is not the "start" of communication (or the "end", for that matter); it is both the start and the end at the same time. The excitement about information theory in psychology was based on PRECISELY the model of organisms that is disposed of by PCT ; INPUT --- CHANNEL -- OUTPUT. Shannon is great for ATT but not for PCT.

Regards Rick

Date: Wed Dec 16, 1992 8:13 pm PST
Subject: More for Rick Marken

[From Gabriel 921216 20:30 CST]

More about feedback mechanisms in the wolf community. After this I'm going to have to swear off anything but serious business on the NET for a month, but one last anecdote.

As I'm sure you know, the alpha wolf's job is essentially to know where to find the moose. This is what he learns in the two or three years he spends surviving as a lone wolf before a vacancy for an alpha opens in a pack, or a piece of unoccupied territory and an alpha female are encountered at roughly the same time. This is a very tough "Ranger Course", I think Rolf Peterson estimates that 90%-95% of the candidates wash out and their genes are lost to the gene pool. Essentially for most wolves, "Go along to get along" is a genetically sound strategy. Alphas have to be bloody minded and very able to survive as far as their graduation ceremony and marriage.

Now, when the alpha gets a bit too old and tired to find the moose, he retires and is held in honour and affection, and as long as there are enough moose to get by he eats. As we get old our calorific needs diminish anyhow. And the pack treat him with respect, and he can still keep up with everybody in their travels. Peterson tells however of one very autocratic alpha, who didn't listen, was not very good at finding moose anyhow, and so

on. The pack was being observed daily from a light aircraft, and one day, the pack was seen, but without its autocrat.

So Peterson backtracked, and came on a very large area of reddened snow, pieces of fur, and not much else. His comment was "The wolves held an election." This is the phenomenenon Tom Baines is constructing a publication around, with the Romanovs in 1917, and Marie Antoinette somewhat more than century before as a few of the particular instances.

There seem to be some interesting breakdowns of communication, and a chaotic bifurcation. This is what the American Revolution and the Constitution are all about, and background to the Amendments.

Publish or perish is not nearly so brutal as the wolves' election. But the observations about academic wars being vicious because nobody has real claws or teeth are absolutely to the point. I have seen a Dept. riven down the middle by a squabble over whether faculty should take their turn with graduate students in buying cookies and making tea for the social occasion before the weekly seminar.

This is what I mean when I say that Heinlen has a point, easily misunderstood, when he remarks that an armed society is a polite society. Fighting is serious business, and that is all Heinlen is trying to say. And all of Bill's counter arguments are well taken and sound, moreover arguments on both sides of the point are mainly only sound. They only become real when the wolves hold an election, or funding becomes scarce enough so that tenure is no longer a protection against involuntary termination.

I will write early in the New Year on the taxonomy of the inhabitants of the academic world. In the meantime, may I recommend the following light reading:-

Microcosmographia Academica, by F.M. Cornford, now almost a century old. It may be out of print. It used to be a family custom to hold about a dozen copies "for the delectation of friends and the confusion of enemies", since the 1930s. Sadly Heffers were not able to replenish my stock when I asked them a few years ago. If the copyright has expired or if I can obtain permission to do so, I may scan my only remaining copy, and publish a limited edition of a few hundred or perhaps a thousand. It used to be my custom to give a copy to each new incomimg director of Argonne. A very few (perhaps one) were even so polite to write a short note of thanks and acknowledgment.

For those of you who have any curiosity about what soldiers are like, there is a wide variety of books to read. Some of those my friends have enjoyed, and which they say have shed light on my colleagues in green suits are:-

The Face of Battle - John Keegan (The most brilliant evocation of military experience of our time - C.P. Snow) - I knew Snow at Harwell, but not very well (naturally). A delightful and brilliant chap.

The Second World War - John Keegan

About Face - Col. David H. Hackworth - Hackworth has been said by some to have been the most able infantryman of his generation. If he had not been disgusted by the situation in VietNam, and left the Army, he might perhaps have been a present or recent Chairman of the Joint Chiefs of Staff.

There are legions of others. One, ultimately concerned with the origins of the VietNam Memorial, but only as its' coda, is The Long Gray Line by Rick Atkinson.

People are even more interesting to study than wolves.

John (gabriel@eid.anl.gov)

Date: Wed Dec 16, 1992 9:01 pm PST
Subject: Re: neuroscien cont

From: Tom Bourbon (921216 22:44 CST)

Mark. Regrettably I lost the first half of your two-part post on neuroscience. I believe the second part got to the heart of the matter, so here goes.

>Mark William Olson (16 Dec 1992 13:11:31 CST)
>Subject: neuroscien cont

>I would do memory research just the same. Just because we would
>describe it differtnly, does not mean that we would investigate
>it differently.

And later,

>I don't think we should shun present research--we would do it the
>same and interpreet it diffently.

But that is just my point. PCT does not merely "describe" things differently -- it explains something that most neuroscientists, especially most memory researchers, do not know exists, or if they do they don't try to explain it in their literature. PCT explains control. Once you recognize that organisms control some of their perceptions, you cannot investigate memory exclusively the way you did before. Well, you can, but you will be right back to missing the point and you will never learn what "memory" is or how it occurs in brains. More on this after your next remarks, that follow.

>We would investigate differently if we were working at higher
>levels of analysis (such as social psych research) but not at
>levels below. For example, if you wanted to know about
>neurochemistry sorts of things during a task, you don't care
>whether percpetions or output is controlled--its just not
>relevant at that level.

Here, you come to the core of the problem. Before you can search for anatomical and physiological correlates, substrates or call them what you may, for memory, you must define memory. Most often, it is defined in terms of changes in behavior during or after certain tasks. Tasks. Living system doing things.

What kinds of tasks will you choose? That is an important consideration; the kind of task will determine what you can call evidence (a) that memory exists and (b) that you have found it and its correlates-substrates-etc. If you think living systems react to stimuli, you will use tasks in which they respond to stimuli and you probably will define learning as a particular change in which responses are associated with which responses, or in how often response (or response class) X occurs in the presence of stimulus (or stimulus class) Y, and so on. You will employ the revered behaviorists' tool kit.

If you think living systems process inputs, cognize on them, select appropriate outputs, and either in parallel or serially, plan and produce outputs, you will still use many of the items in the behaviorists' tool kit, but you might irreverently call them by new names.

In either case, if you are true to the grand traditions in the literature on physiology-anatomy-chemistry-etc of memory, you will look for memory in the parts of the nervous system between where stimuli-inputs come in and stimulate, and the parts where responses-outputs go when they are on the way out. You will conceive of memory as a step between in and out and you will assume it has a function or form or quality that lets it mediate between what comes in and what goes out.

If you realize that living systems control some of their perceptions, you will not use tasks that treat them as though they are funnels into which causes pour and out of which effects emerge. Nor will you think of memory as a process-place-thing that resides somewhere between the orifices of the funnel and matches them up in "proper" fashion. What you look for, where you look for it, and how you decide whether or not you found it all depend on your ideas about the bigger picture -- the levels you say do not matter.

You said, "...if you wanted to know about neurochemistry sorts of things during a task, you don't care whether percpetions or output is controlled--its just not relevant at that level." I would describe the situation differently: "If you want to know about neurochemistry sorts of things during a task, you had better determine, right up front, whether perceptions, or outputs, or both of them, are controlled -- that determination is crucial in all else that follows in your research at the level of neurochemistry."

>Maybe I should ask if PCTers are evn intererestd in mapping brain functions.

Check out my return address!

Until later, Tom Bourbon

Date: Wed Dec 16, 1992 10:30 pm PST
Subject: Re: citations;social;Gabriel

From: Tom Bourbon (921217 00:16 CST)

Ask a simple question ... !

John Gabriel made a simple declaration:

>[gabriel to powers 921214 11:35 CST]

>"... those who have been applying PCT in advertising agencies have
>a substantial hand in deciding who gets to be president and C in
>C."

I made a simple request:

>Tom Bourbon (921215 00:48)

>I collect citations of PCT and I am always looking for new areas
>in which PCT is applied. I have not seen, perhaps because I
>never looked for them, applications of PCT in advertising. I

>would like to add that specimen to my collection. Can you
>provide a few specific citations and examples?

When John did not reply, I repeated my request with a simple additional remark:

>Tom Bourbon (921216 09:18 CST)

>I am serious. I will appreciate any citations you can provide on
>people in advertising agencies who use PCT to sell products and to
>influence presidential elections in the United States. My
>collection of applications will be incomplete without citations
>for applications as important as those. Don't keep them for
>yourself!

I am not certain what happened next.

>[John Gabriel to Tom Bourbon 921216 11:54 CST]

>There's a difference between doing PCT in detail, with all the
>hierarchy, about which I have some fairly serious doubts because
>there's so much ECSs seem to me unable to accomplish without
>interactions so extensive that the interactions become more
>important than the ECS's, and Bill's wonderful insight with which
>I completely agree, that people or any other organism, or even
>state machine, do things because of their perceptions, (for state
>machines, read inputs) or sometimes to change their perceptions.
>Carelessly put - In order to change some of their perceptions is
>the way the previous thought should have been stated.

Say what? I asked for a few citations.

>If you want a PCT based statement of policy for an advertisng
>agency, I don't have it, and don't plan to waste time looking for it.

Excuse me. My mistake. This is a network of people interested in PCT. When you posted a direct statement implying that at least some people in some advertising agencies have been applying PCT and that they "... have a substantial hand in deciding who gets to be president" and Commander in Chief in the United States, I was interested and I thought your remark was offered in the spirit that most of us make our posts on CSG-L. I asked for more information. Sorry to have wasted your time.

>If you deny that one of the purposes of advertising is to change
>perceptions of products, political candidates, or other things, so
>that the behaviour of those viewing the advertising is changed, I
>think you are very likely wrong, and I believe others will agree
>with me.

Wait a minute! Where did that come from? I made one simple request for information. Why are you so eager to fabricate motives for me and then assert that they are wrong? I have re-read my posts at least a dozen times and they contain nothing even slightly related to your suggestion that I deny the purposes of advertising. Why would you think I said that? Now that you bring it up, I agree that advertising people try to affect the actions of others, but if you assert on this network that they are applying PCT, I wish you could find the time to tell us more about it. Were I a betting man, I would wager heavily that there is not one advertising person in the world who applies PCT, but I am willing to

change that opinion in the face of strong evidence. Why are you so eager to tell me that I am wrong (about something I never said) and then tell me that others will agree with you?

Later on, you express a, "... mild irritation with total missionary zeal for PCT." I am not at all sure where *your* missionary zeal is directed, but in the future I will try to stay out of your way.

Tom Bourbon

Date: Thu Dec 17, 1992 8:55 am PST
Subject: On wolves: A note from my science advisor

The expert on wolves (from the gang of three) has put straight about some citations. Here are her comments:-

>From @firewall.nielsen.com:mgabriel@nis.naitc.com Wed Dec 16 21:36:45 1992
>To: gabriel@eid.anl.gov
>Subject: Re: More for Rick Marken
>Cc: hculver@nis.naitc.com

>I can't resist adding a few comments about wolves. The comment about
>alphas being former lone wolves is straight out of a lecture by Rolf
>Peterson to a group of us on Isle Royale. I don't know if it's
>published anywhere. The comment about the wolves holding an election,
>however, is straight out of Durwood Allen's wonderful book, "The Wolves
>of Minong." It is almost the last chapter in Allen's account of the
>"big pack" (perhaps 23 members at full strength) and its leader "Big
>Daddy." I'm quite sure the "election" was held before Rolf's time.
>The comment about packs being very deferential to their elderly former
>alphas is from George Rabb's observations of the Brookfield Zoo pack. I
>don't think anyone knows how much of this applies to wolves in the wild,
>and particularly to wolves in the wild facing hard times. Nothing is
>simple.

>Cheers,   >Marian
 I also have to correct my citation about "All the fun of war ...." It's from John Jorrocks MFH a fictitious character from about 1890 on Foxhunting. Oh well - the unspeakable in pursuit of the uneatable to quote a later commentator. O tempora, Oh mores. And the fictional Jorrocks, being a grocer who had become rich from his efforts and purchased acountry estate was quite the (apparent) reverse of my friends who have been shot at.

Best   John

Date: Thu Dec 17, 1992 9:48 am PST
Subject: Re: Rick on (off) Shannon

[Martin Taylor 921217 10:40]   (Rick Marken 921216 sometime)

I really can't let Rick's latest non-sequitur go unchallenged:

> Bill P.'s

>important observation was that perception is one link in a
>causal loop in which we are all locked -- a loop that obviates
>Shannon's concerns about reliable transmission since, in living
>systems, perception is not the "start" of communication (or the
>"end", for that matter); it is both the start and the end at the
>same time.

Every piece of the loop is constrained by Shannon's observations/theorems. If (like Rick) you don't understand them, you are doomed not to understand PCT.

Martin

Date: Thu Dec 17, 1992 10:43 am PST
Subject: Signing off for a while

[From Gabriel 921217]

Just heard signs of an avalanche of things to do on the hillside above me.

Bill C. points out that the last few days of NET is example of failed attempts to establish layered protocols. He will probably elaborate in due course.

Back some time in New Year when I'm dug out. Meanwhile over and out. Don't unsubscribe me though or anything drastic like that. I hope to have time to read at least.

Best Wishes to All John

Date: Thu Dec 17, 1992 12:11 pm PST
Subject: In the eye of the beholder

[Martin Taylor 921217 11:30] (Bill Powers 921213 07:00)

>I'm perfectly aware of my limitations and my ignorance, but the
>fact is that when, as a teenager, I read Einstein's explanation
>of relativity, I may not have known what a tensor was, but I
>understood the idea perfectly well. When I read Norbert Wiener's
>account of control processes in living organisms, I may not have
>seen the relevance of stationary time series or followed his
>arguments about Newtonian and Bergsonian Time, but I understood
>how a control process worked and what it had to do with behavior.
>When people describe a phenomenon of nature and offer a clean and
>simple explanation of it, I usually understand what they are
>getting at. So, I think, do most people.
>
>When I read your post, John, I could not figure out what you and
>the others were getting at. Beneath my surface reaction, I was
>wondering "Why are they making it so complicated? What are all
>these theorems supposed to be about? What makes them think that
>any of this has anything to do with reality? Are these people
>really of such a different order of human intelligence that they
>can see simplicity and order in such seemingly vague and abstract
>conjectures?"

Complexity, like beauty, is in the eye of the beholder. You find it difficult to get people to understand PCT, which to you is very simple. To someone that does not have the necessary underlying concepts, the whole network of ECSs, plus the structure of the individual ECS, plus what it means to control ... all has to be put into a single perceptual input function, which is very complex. For people who have a feeling for control, but who use complicated inverse kinematics as an essential element, the PIF required for HPCT is simpler, but wrong, so it again seems to be complex.

John Gabriel thinks in terms of mathematics that you and I do not know. What he says seems complex, partly because we don't understand what to him are trivial implications. Many people on this net (Rick and Bruce Nevin for prime examples) have complained about my uses of Shannon information theory in connection with PCT, because they bring along with it an incorrect lower-level PIF that makes the consequences for HPCT seem unnecessarily complex. To me, Shannon information makes HPCT intelligible and simple.

>If this work is so fundamental and important, why don't I understand it?

There's no reason why you should, but perhaps there is an implicit social demand on we who have different backgrounds to explain more carefully why our view should be helpful to you. As I said, I don't understand John any more than you do, and I have reason to believe he doesn't understand me either.

> as a teenager, I read Einstein's explanation
>of relativity, I may not have known what a tensor was, but I
>understood the idea perfectly well.

Me, too. The answer is that Einstein is a brilliant writer who understood his subject so well that he could see just how to relate concepts in a way that made good sense for normal people. We aren't all so gifted.

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On another topic:

>At the risk of alienating friends,

No alienation here...I assume it refers to

> And why,
>beneath this work, does there seen to be such an unthinking
>acceptance of the premises of military philosophers? Would a
>scientific advisor to a torturer find it just as easy to become
>immersed in the technical problems, and to ignore the underlying
>repulsiveness of the whole undertaking?

The short answer to the second quetion is "Yes." Look at the Nazi doctors in the concentration camps, or the US teachers of Paraguayan and other South American torturers. Their personal high-level references permit it.

I think you would have a better explanation for this than anyone else. Is it your perception of something in the closer world that is not perceived by the rest of us? Is it a fact of Boss Reality, that your theory addresses? I suspect a bit of each.

As an exculpatory statement, let me explain that DCIEM is in the business of protecting people from stresses of all kinds. People in the military are subject to more stress than the average Joe. We deal with all ways in which people interact with their working environment, physiological, mechanical, psychological. The environment is military,

primarily for peacekeeping purposes (did you know that Canada has been involved in EVERY United Nations peacekeeping mission). I don't perceive myself as "scientific advisor to a torturer," though I willingly try to help our military to achieve their objectives if they ever get into a situation where they must fight.

In military terms, "going up a level" is the job of the politicians. If they do not succeed (as seems usually to be the case) in finding ways around a conflict of interest, and one side chooses to fight, the other side had better be ready to fight or to concede. Humans are, as we discussed over the last couple of days, not good at conceding, as compared to other mammals. So we have the (Bill-P-predicted) escalation of force. If no-one provides the technical support for that escalation, then the politicians' failure leads to a concession after defeat, which is worse (I think) than early concession. (I prefer "Better Red than Dead" to "Better Dead than Red"). Did MAD (Mutual Assured Destruction) actually keep the peace for 40 years between the Soviet Union and the USA, while exacerbating all the marginal conflicts that destroyed only a small part of the world rather than all of it?

I think that military research can be avoided only by the development of some solid means of resolving international conflicts by agreement. If the USA would acknowledge the authority of the International Court of Justice, it would help a lot. But the USA acknowledges the ICJ only when their rulings support the USA, which makes it very hard for the lesser nations to accept the ICJ as authoritative. One of the points at which it became very hard for me to maintain my present job, knowing that the Canadian military is allied with the US military, was when the USA repudiated the judgment against it in respect of mining Nicaraguan harbours.

Sorry to get political, but there are reasons for working with the military that can square with ethical principles. And I think that you, Bill P, should be better able than most people to understand that.

Martin

Date: Thu Dec 17, 1992 12:39 pm PST
Subject: Melius ex errore

[From: Bruce Nevin (Thu 921217 12:25:14)]

For when PCT becomes part of the academic establishment, here is a nifty motto, perhaps to be graven over the gothic entrace to PCTU, lifted (I am told) from the medieval schoolmen:

Melius invenitur veritas ex errore quam ex ignorantia

"It is easier to arrive at truth from error than from ignorance."

The communication quandary is how to get an audience from the confort of vested ignorance (ignoring) to the productive discomfort of error.

Bruce bn@bbn.com

Date: Thu Dec 17, 1992 1:31 pm PST
Subject: Misc up-a-levels

[From Bill Powers (921217.0930)] John Gabriel (921216.1154) --

Your paraphrase of the point of PCT is ambiguous enough to suggest that you may still have a big AHA waiting. If not, it does no harm to make the point again:

>... people or any other organism, or even state machine, do
>things because of their perceptions, (for state machines, read
>inputs) or sometimes to change their perceptions. Carelessly
>put - In order to change some of their perceptions is the way
>the previous thought should have been stated.

I'd put this in a slightly different way: organized behavior never occurs except with the aim of controlling some perception. To speak about "changing" a perception might imply that we can do an act to change a perception from one state to another, then relax until it's time to change it again. To say that we sometimes act to control perceptions could be taken to mean that we sometimes act for other reasons. In fact controlling perceptions almost always requires continuous behavior, because the perception is dependent on the behavior as well as on independent influences. And behavior never occurs except for the purpose of controlling a perception.

Behavior -- and perhaps this is the part you meant to edit -- is not done "because of perception." That's S-R theory. PCT says that the perceptions that matter most to an organism are as they are because of behavior, which in turn is as it is because of the difference between those perceptions and internal reference signals.

Just keeping the language tidied up.

I don't think I want to debate about the merits, necessity, justifiability, etc. of the military philosophy any more. Everything you have said points to a system concept and a set of perceptions that adequately explain why numbers of people act as they do with respect to military and related matters. The relationship of those perceptions and reference levels to observable actions is interesting from the theoretical PCT point of view, but the particular content of the reference signals and the perceptions is of no theoretical interest. Everyone argues that his or her own reference signals are right. What else? I would like to be able to do a serious study of these things, but I don't have the resources and PCT needs a lot of more basic development before that will be feasible.

More or less the same thing goes for wolves and baboons. Anecdotal accounts of behavior may suggest certain explanations, particularly if appeal is made to more conventional theories instead of PCT. But they don't give us the kind of information we would need in order to understand the perceptions being controlled by these animals, or the reference levels associated with them, or the amount and frequency of reorganization involved. The data you would need to get that kind of information is never taken because the people doing the observing don't know control theory and don't know what to look for.

As to your conversation with Tom about advertising: the only perceptions a person can control are that person's own perceptions.

>Now, I'm not saying the hierarchy is nonsense, it isn't, but I
>only believe it somewhat more than I believe in the Id and Ego.

Does the following list of perceptions strike you as being of the same nature as Ego and ID? Intensity, sensation, configuration, transition, event, relationship, category, program, principle, system concept. If so, I would be disappointed, because my intention

was to find types of perceptions on which everyone could agree and which seem obvious and self-evident in ordinary experience.

>There does seem to me a very convincing explanation of puzzling
>things in the reliability of people in doing jobs like picking
>up glasses of water, or following randomly moving cursors.
>Following the cursor has very clear reference signal - the
>cursor position, and I have no trouble understanding it.

If you think that the target position is an obvious reference level, how do you explain it when a person intentionally keeps the pointer two inches to the left of the moving target, or makes the pointer describe a continuous circle around the moving target (in two-dimensional "tracking")? Where is that reference condition to be found in the environment?

How do you explain a person keeping a checkbook balanced without appeal to some reference perception? I think you may be missing the main phenomenon that PCT is about -- which is not tracking.

>I've been building feedback systems since 1941, and doing their
>mathematics since 1946 ...

Well, you're way ahead of me there. I've been doing it only since 1953, and my approach began mainly through analogue computing. I could claim that my Navy electronics experience in 1944-46 counts somewhat, as I learned at least how to troubleshoot control systems if not to analyze them.

>I think the following things are well founded...

> 1. The mathematical theory of linear feedback systems as put
>forth for example by Bode - Network Analysis and Feedback
>Amplifier Design 1945 Van Nostrand.

Did Bode ever notice that control systems control their own sensor signals, not their outputs? I read most of that stuff, not with very deep understanding, but I never noticed any statement like that.

>2. The idea of classical contact transformations, as first put
>forward by W.R. Hamilton. The classical contact transformation
>is the operator that takes system state from that at time T to
>that at time T+dT ..

When this idea got into the hands of digital computer people it became an unfortunate idea. The idea that the brain passes from one discrete "state" to another, with nothing happening between states, is completely wrong. The brain isn't clocked; neurons are not in "1" or "0:" states; variables in the brain can't even be measured at an instant (because frequency is the significant measure in the context of behavior). And the idea of a whole-system transfer function is a delusion -- unmeasureable for most behaviors, and even where measurable, useless.

>3. The idea of the rate of information transmission down a
>discrete channel, being the the upper bound of the number of
>binary decisions a recipent can make in a second, and first put
>forth by Claude Shannon in the two 1949 papers in BSTJ.

But all real neural systems work with continuous, not discrete, variables. Neurons do not respond to incoming impulse streams by making "decisions" but by harboring continuously-variable chemical concentrations and potentials which in turn determine the frequency of outgoing impulses. The computations done by a neuron are analogue computations based on continuous internal electrochemical variables. Certainly information theory could be applied to these processes. But it doesn't help you model them.

>Although Shannon does not discuss stability, I think stability
>is only an important side issue as compared to information.

Stability is the most difficult consideration in designing a control system that actually behaves. If you've designed and built a lot of control systems, it's hard for me to understand how you could say information is more important. In all control- engineering texts I have seen, about one chapter is devoted to the basic principles of control per se, and all the rest is an exposition of methods for measuring and achieving stability. Information theory is hardly mentioned. Information content is of secondary interest in models of behavior, because normal perceptual signals are far above the noise level and are seldom ambiguous.

>4. The ideas of conventional Decision Theory and Value Systems
>   are an adequate first approximation to human behaviour
>   in decision making to support qualitative analysis,
>   and if estimates can be made about values, quite good
>   quantitative analysis.

I think that your definition of "quite good quantitative analysis" may be rather different from mine. Are you talking about predicting the time-course of behavioral variables in each individual case with an accuracy of, say, five percent? That's my definition of "good."

>I think these things are a long way away from your interests,
>which makes fertile ground for misunderstanding. And perhaps we
>don't have the same ultimate objective after all.

It does begin to look that way, doesn't it?

I think you're going through the second stage of learning PCT. The first stage is the big insight about control of perception. The second stage begins when you start to see that if PCT is correct, a lot of things you've believed up to now are probably either wrong or irrelevant. If you're willing for the second stage to continue, maybe you'll still be here a year from now.

I hope you will be.

Rick Marken (921216.1200)--

I think I have to side with Martin Taylor on this one, Rick. In a simple reorganizing model, e^2 might be a suitable driving signal for the rate of reorganization. But that isn't the controlled variable. It's the error signal.

I have also found in my experiments with reorganization that the time-rate of change of error signal is important, too. The most useful measure of error that I have found is the one that Martin suggested some time ago: e^2 * (de^2/dt).

Best to all, Bill P.

Date: Thu Dec 17, 1992 3:17 pm PST
Subject: Re: Harnad's reviews

[From Dick Robertson] (9212.17)

A loud Amen to Bill's comment on Harnad's reviews (I hope this gets through, the machine just went around the corner by itself.) I do think there is something in and of itself that is interesting and worth study in the way that people with established status fight to keep anything new from being accepted. Wouldn't it be fun to have a survey of the system concepts of those people?

Date: Thu Dec 17, 1992 8:15 pm PST
Subject: Re: citations...

[from Ray Jackson (921217.2100 MST)]

  Tom Bourbon (921216 09:18 CST)

>Re: Citations of PCT
>If anyone has citations for applications of PCT, in any setting, I
>want them. Keep my address on file and send or post anything you
>find. I will be grateful.

Hi Tom,
Your request got me thinking: Have you ever seen those ads for the Anthony Robbins' "Unlimited Power" program? Also, there's another self-help program out called "Investment in Excellence" by Louis Tice. I have only followed these programs with passing interest or through the experience of others. I haven't been able to look into these programs in depth. I just remember thinking that the reasons why these particular programs were more effective than others were because they seemed to incorporate PCT principles; that is, they seemed to focus on affecting perception as a by-product of creating or programming in a set of healthy and constructive internal reference signals for an individual to operate with.

In the business world there is Stephen Covey and "Seven Habits..." and "Principle-Centered Leadership". Also, you'll find an undercurrent of PCT in Deming's management philosophy; I finally got around to reading Dag Forssell's excellent "Observations, Interpretations, Commentary [on Deming]" -- (By the way, Dag, I have some minor editing suggestions for the piece I will forward by snail mail this weekend). But, once again, the reasons why these programs are successful seemed to me (and Dag) because they happened to be representative of the "common sense" found in PCT.

I hope that is the type of thing your looking for...By the way, even though I'm still a "PCT-lightweight", I always enjoy getting through your posts.

Regards, Ray

Date: Fri Dec 18, 1992 11:57 am PST
Subject: Feedback Is Too Delayed

[FROM: Dennis Delprato (921218)]

Two more impediments to mainstream experimental psychologists' giving serious consideration to PCT:

1. Widespread thinking of two basic choices: (a) central control and (b) closed-loop (or input or sensory or peripheral feedback) control, plus, of course, the eclectic combination of the two. According to this thinking, the closed-loop theory is that sensory "information" from one movement serves to instigate the next movement. There may even be a comparator involved (e.g., Bernstein). Now, none of this has anything to do with PCT--the very idea of central vs. peripheral control is by the boards. But, I do not believe PCT theorists have done a good job of addressing how their approach departs from this sort of very elementary, widely entrenched thinking.

2. One of the most frequently-heard reasons for ignoring feedback control (in framework of above) is that feedback is too slow relative to the speed of movements that can be made. Can anyone cite me a publication in which this issue is directly addressed from the standpoint of PCT? I am looking for a coherent presentation that is data-based.

Dennis Delprato

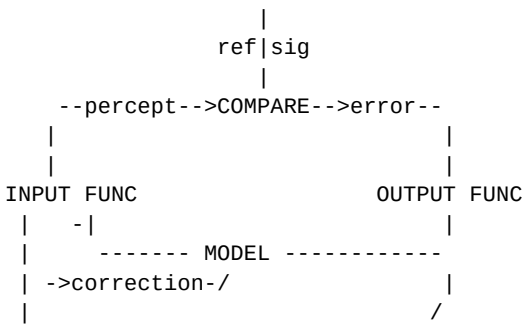
Date: Fri Dec 18, 1992 12:47 pm PST
Subject: Memory in the model

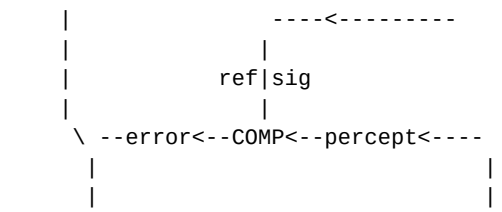
[From Bill Powers (921218.1030)] Allan Randall (921217.1614) --

Sorry, Allan. This network seems to have the property of putting everyone into a state of permanent overload while trying to get other things done, too.

The memory model in BCP was intended only to explore the role that memory might have in behavior. The initial impetus was simply the question, "How do we 'do the same thing again?'" This becomes a problem when you realize that the world is never the same twice in a row, so that to "do the same thing" we must actually produce different actions. Also, how do you know you're doing (accomplishing) the "same" thing? The only answer I can see is that you remember what happened last time. It must be your memory of the past perception that serves as the reference signal for this time. So how does that memory get selected? And the rest, I think, follows.

The idea of mental models must entail memory, as you suggest. There is one version of the HPCT model that we have tossed around for some time without really deciding about it, in which the world we experience is ALWAYS the modeled world, PLUS whatever error comes from lower systems. It works like this:





The bottom system is mirror-reversed to avoid crossing signals.

The higher system controls a perception made of the sum of a MODEL output and the lower-level error signal.

The basic idea is that when the higher system acts to control the model, its output enters both the model and the lower systems (as reference signals). If the lower systems succeed in bringing their individual perceptual signals to the demanded reference level, they will send no error signals to the higher system. That means that the model works; the outputs that control it have the correct effects when also used as reference signals for the lower systems.

If the lower systems can't produce the perceptual signals demanded of them, their error signals will enter the higher system, being added to the sensed behavior of the model. So the higher system will experience a perception of the model's behavior, plus the error signals, and the result will be that the perception is controlled correctly even though the model is not correct.

However, the presence of the error signal shows that the model must be revised. I have only indicated a "correction" based on the lower error signal, without saying how it's brought about. This correcting process will gradually alter the model until no error signals are being sent upward from the lower systems. Then the model will again be a sufficient representation of the lower- level world.

Algebraically, this revised model is exactly equivalent to the standard model in its performance. But there is at least one important difference. If some of the higher-level inputs from lower systems are lost, the higher system can continue to run on the basis of the model alone. This is like a tracking experiment in which the display is momentarily blanked out, or like a baby following a toy train with its eyes as the train disappears into a tunnel and reappears at the other end (like Piaget's experiment). The model supplies perceptions that for the moment are not available from the environment. Now, in this version, that is because the model is providing the perceptions _all of the time_, with error signals from lower systems being used for a continual, but slow, update of the model.

This is an intriguing possibility, because it says that the world of experience is even less directly related to the environment than under the old model. It says, in effect, that we're imagining EVERYTHING, but that what we imagine is slowly being corrected all the time to eliminate errors between our way of imagining and the way the world actually works. The world we experience would then literally be a model of the environment.

The model, of course, would be composed mainly of memories.

I'm still not seriously proposing this revision, because it has to be checked out against experiment to see if it really adds anything or is necessary. But it's good to keep in

mind as a way of handling some phenomena that the current model would have some difficulty with.

As to the RNA model of memory, I don't have any investment in it. Whatever the actual mechanisms, they have to account for the things memory is required to do. Defining what memory is FOR can be done without knowing how memory works.

Best, Bill P.

Date: Fri Dec 18, 1992 1:30 pm PST
Subject: awareness of error/ignorance

[From: Bruce Nevin (Fri 921218 13:24:41)]

I had posted (Thu 921217 12:25:14) the following so that we could all affect proper learnedness when PCT goes establishment:

> Melius invenitur veritas ex errore quam ex ignorantia
>
>"It is easier to arrive at truth from error than from ignorance."
>
>The communication quandary is how to get an audience from the
>comfort of vested ignorance (ignoring) to the productive discomfort of error.

  Bruce bn@bbn.com

Martin and I subsequently had the following exchange (lightly edited):

-=+=-=+=-=+=-=+=-=+=-=+=-=+=-START OF DIGEST-=+=-=+=-=+=-=+=-=+=-=+=-=+=-=+=-

Date: Thu, 17 Dec 92 14:34:01 EST
Subject: Re: Melius ex errore

Bruce,

I think I would distinguish recognized error from erroneous belief. It is easier to arrive a truth when error is detected than from ignorance, but harder to arrive at truth from a firmly held erroneous position than from ignorance.

Martin

Date: Thu, 17 Dec 92 14:55:04 EST

Or we could distinguish two forms of ignorance: erroneous belief and recognition that one does not know.

The Latin maxim does seem to depend upon awareness of error, but not necessarily awareness of ignorance.

I see a simple matrix:

```
        \  errore      ignorantia
         +--------------------------
known    | error       ignorance (pluck)
```

```
         |
unaware  | erroneous   ignorance (luck)
         | belief
```

It seems to me that an erroneous belief can be sustained only by a combination of luck and ignoring error (probably through imagination).

Thanks for the thought-provocation! Bruce

Date: Thu, 17 Dec 92 16:24:52 EST

I like it! (the 2x2 matrix)

Awareness of ignorance is an interesting thing. It means that one must know something about the area about which one is ignorant. I don't know if I am ignorant about the social practices of the Bowumbi of Alpha Centauri if I don't know of the possibility that the Bowumbi might exist and might have social practices. Aware ignorance is somehow focused, and there might be a choice (pluck) to maintain the state of ignorance in that resolving it is unlikely to alter one's ability to control perceptions that matter.

Funny how throw-away comments can lead to expanding conceptual structures.
Magellan never expected the Pacific Ocean when he went through the little
straits that bear his name.

Martin

Date: Fri, 18 Dec 92 08:44:00 EST

Martin,

Here is how I understand the process:

The perceptions to which we pay attention are more clearly defined and longer lasting in memory than those we disregard. In this way, we create reference levels for selected perceptions (selected by having attended to them). When we use these in imagination, unforeseen perceptions come up as ramifications and consequences. By reasoning about these, we develop/impose order and structure in them. In these perceptions of order and structure there are gaps. A gap of this sort provides a context for recognizing a perception ("real" or imagined) for what it "really is," and by that I mean perceiving it as a filler of that gap. (Related perhaps to Gibson's notion of affordances.) This we call intuition or insight.

The things we have conscious control over are: what we pay attention to, how well we pay attention to them, and how well we reason about them. The rest is on automatic pilot.

The part for which we have some conscious responsibility includes how we interpret perceptions as to what they "really are" (what they constitute at higher levels). And of course how attached we are to our conclusions as imagined perceptions. We know that one false premise puts the conclusions at random, but we often forget or ignore this in practice. When we use as premises conclusions taken from prior lines of reasoning from premises based on authority, etc., the house of cards looks pretty shaky.

Magellan provides a kind of example. I just looked him up in my Columbia Encyclopedia. He was looking for a passage to the "south sea" in five ships. When he got three ships through the strait named for him, he had no idea that his perception of a large body of

water before him constituted so vast an ocean. In the months to the next landfall they revised their conception of the relationships of land and water on the globe. When a few survivors on one last ship got past the Portuguese at the southern tip of Africa and back to Spain (Magellan died in the Philippines), others began to revise their perceptions (in imagination) of geography. What motivated the whole thing was consciousness of ignorance, in the form of a gap (passage to trade with the Indies) in what turned out in the event to be some erroneous beliefs about geography.

I suppose we could create a mini-digest of this exchange for the net, one way to make off-line dialog more generally useful.

Bruce

Date: Fri, 18 Dec 92 12:02:06 EST

I'll hold off on agreeing or disagreeing about "consciousness" and its relation to PCT processes. But given that, I have no quarrel with what you say.

>I suppose we could create a mini-digest of this exchange for the
>net, one way to make off-line dialog more generally useful.

Yes, it turned out to be more interesting than I thought when I posted my [. . .] come-back to your original comment. But most off-line dialogues don't turn out that way. By all means post it if you want.

Martin

-=+=-=+=-=+=-=+=-=+=-=+=-=+=-END OF DIGEST-=+=-=+=-=+=-=+=-=+=-=+=-=+=-=+=-

Bruce bn@bbn.com

Date: Fri Dec 18, 1992 2:18 pm PST
Subject: discrete perceptions

[From: Bruce Nevin ()Fri 921218 13:50:21]

(Bill Powers (921217.0930) to John Gabriel) --
Don't we get something like discrete states in the latch mechanism for steps in events and sequences? And indeed for category perceptions and on up?

It seems to me that, as the strength of some category perception grows, it becomes easier (more acceptable?) to fill in missing category-attribute perceptions by imagination. At some threshold, or seeming threshold, where imagined perceptions are integrated with real-time perceptions, it is as though the exemplar of the category is perceived as fully or truly present, whereas before there were only unsupported signs or symptoms that fostered a belief, readiness, or expectation.

There is an analogy to the relation of continua to discreta in language. In language, pronunciations are continuous phenomena, but speakers and hearers perceive words (morphemes) as (probably event-level) sequences of discrete tokens, where the types are sound contrasts established by social convention for their speech community. Children probably learn a limited stock of words first, and then learn the conventional contrasts and the type-token relation of sounds to contrasts in words, which in turn enables

learning and recognizing a richer stock of words. Language probably evolved in its first stages by such a route.

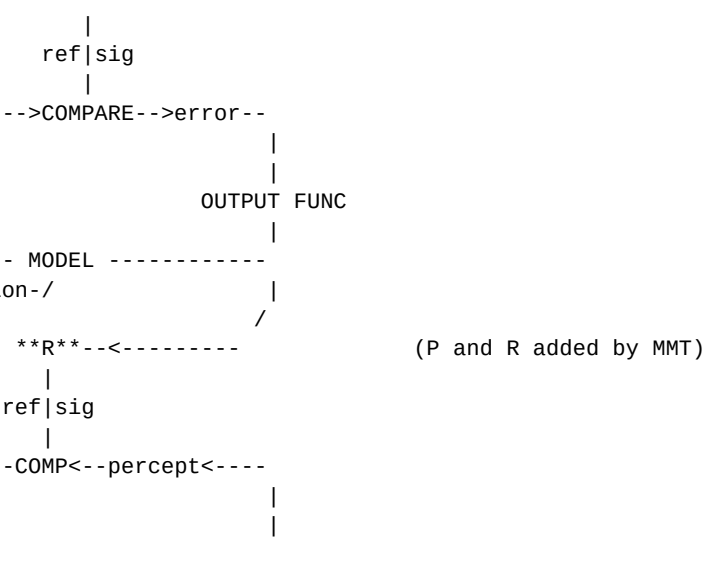
>>3. The idea of the rate of information transmission down a
>>discrete channel, being the the upper bound of the number of
>>binary decisions a recipent can make in a second, and first put
>>forth by Claude Shannon in the two 1949 papers in BSTJ.
>
>But all real neural systems work with continuous, not discrete,
>variables. Neurons do not respond to incoming impulse streams by
>making "decisions" but by harboring continuously-variable
>chemical concentrations and potentials which in turn determine
>the frequency of outgoing impulses. The computations done by a
>neuron are analogue computations based on continuous internal
>electrochemical variables. Certainly information theory could be
>applied to these processes. But it doesn't help you model them.

Date: Fri Dec 18, 1992 3:36 pm PST
Subject: Re: Memory in the model

[Martin Taylor 921218 15:30] (Bill Powers 921218.1030)

I have a problem with the specific detail of Bill's proposal. Not with the principle.

>There is one version of the HPCT model that we have tossed around
>for some time without really deciding about it, in which the
>world we experience is ALWAYS the modeled world, PLUS whatever
>error comes from lower systems. It works like this:
>



>
>
>The bottom system is mirror-reversed to avoid crossing signals.
>
>The higher system controls a perception made of the sum of a
>MODEL output and the lower-level error signal.
>

>The basic idea is that when the higher system acts to control the
>model, its output enters both the model and the lower systems (as
>reference signals). If the lower systems succeed in bringing
>their individual perceptual signals to the demanded reference level,
>they will send no error signals to the higher system. That
>means that the model works; the outputs that control it have the
>correct effects when also used as reference signals for the lower
>systems.

This seems to work when there is a one-to-one relationship between lower and higher systems, but will it work with many-to-many? The reference signal in the lower ECS is not the output from the higher, but a function of a vector of such outputs from higher ECSs. So when the lower perceptual signal matches its reference, the perceptual signal returned may well be quite different from the output that is sent (Points R and P that I added to Bill's diagram. Likewise the perceptual signal in the higher ECS is a function of a vector of signals like P from many lower ECSs.

If all the lower error signals are zero, then the higher one will presumably also be zero, but the reverse is not true. The higher ECS can have a zero error when none of the lower ones are satisfied. I'm not sure what this would do to the correction aspect of the memory module. Should there be a correction or not?

And what evokes a specific memory? In the BCP diagram, we mentally inserted "reference vector" as an addressing code for a scalar memory output--a table lookup function. That seems reasonable, but we were not at all clear how it would or could work. And in the context of the planning exercise that I described the other day (shopping robots in the supermarket), we could not see how to incorporate the real-world constraints presented verbally and never before experienced by any ECS operating through a real world CEV. That question, also, still hangs in the air.

Basically, I like the concept that the ECS deals mostly with model predictions, corrected by error. But I'm not sure what error, or what is the memory. Is it a vector of sensory inputs, to be corrected by the vector of incoming error signals (or more probably the incoming perceptual signals--more robust)? And what evokes one memory rather than another?

Remember my point about the reduction of information rate as we go up the hierarchy. It is this that gives play to predictive memory models.

Martin

Date: Fri Dec 18, 1992 3:43 pm PST
Subject: Re: Martin to Rick on Shannon

From: Tom Bourbon (921218 14:38 CST) >[Martin Taylor 921217 10:40]

>I really can't let Rick's latest non-sequitur go unchallenged:

>> Bill P.'s
>>important observation was that perception is one link in a
>>causal loop in which we are all locked -- a loop that obviates
>>Shannon's concerns about reliable transmission since, in living
>>systems, perception is not the "start" of communication (or the

>>"end", for that matter); it is both the start and the end at the
>>same time.

>Every piece of the loop is constrained by Shannon's
>observations/theorems. If (like Rick) you don't understand them,
>you are doomed not to understand PCT.

I know Rick has been out of town (L.A.) for a few days so he probably has not seen your post. It probably will puzzle him as much as it does me. In the past, both of us wondered how, specifically, Shannon's ideas, or any of the major concepts from information theory, would improve any of the quantitative predictions we make with our simple PCT models.

May I repeat and elaborate on an invitation I made about a year ago? I will send you one of my programs in which two systems (two people, the two hands of one person, two models, or a person and a model) interact and produce controlled relationships. The version I will send includes a procedure that uses data from a first run to calculate values of parameters for two PCT models. Then the program calculates predictions of performance during a second run under altered conditions. Correlations between the predicted and actual values of all variables reach or exceed the now-familiar .99+ and rms error in the predictions is low.

What I am about to say is a serious offer. It is not intended to be malicious or deceptive -- some reviewers of "Models and Their Worlds" believed Bill and I had bad intentions when we made a similar offer.

I invite you to add to the PCT model any features of information theory that you believe must be there. If necessary, use features from information theory to replace those from PCT. If your changes improve the predictions by the model, there will be no argument and no complaint: You will have demonstrated that a person who does not understand Shannon or information theory does not understand PCT. Please, invite participation by any of your colleagues who are interested. All of us are trying to better understand the phenomenon of control. If a better understanding comes from a model different from the present version of PCT, that is fine. Until later,

Tom Bourbon

Date: Fri Dec 18, 1992 4:52 pm PST
Subject: Misstatements; Latin mottos; Knowel,ed

[From Bill Powers (921218.1500)] Dennis Delprato (921218) --

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.? I really think that an article in a major psychological journal on this subject would give us a base from which to launch more introductory articles. If you would be willing to write such an article, great -- but let's start collecting the information. I suppose we could include statements from reviews, although they're hard to cite.

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 introducting 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.

One of the indications that PCT is an important new idea is the way people get enthusiastic about it until it impinges on their own life's work. They can see how it makes sense in other people's fields, and even in their own fields when it doesn't conflict with their own work. But there's the nitty-gritty point where they have to compare it against ideas they have spent years developing and justifying, and often defending against attack, and there progress bogs down. I don't suppose that there's a single person who now goes under the label of perceptual control theorist who hasn't run up against this wall. It's not a pleasant experience, although when they manage to get past the barrier I think most people would grudgingly admit that the struggle was worth it. If we could figure out a way to make this transition easier we would all have an easier time of it. But it's a purely personal battle that each one has to wage alone.

John Gabriel? Charlynne Clayton? Anyhow (921218) --

> AU BOOKMAN, LAWRENCE ALAN.
> TI A TWO-TIER MODEL OF SEMANTIC MEMORY FOR TEXT
> COMPREHENSION.

>Semantic memory consists of two tiers: a relational tier that
>represents the underlying structure of our cognitive world
>expressed as a set of dependency relationships between
>concepts, and an analog semantic feature (ASF) tier that >represents the
common or shared knowledge about the concepts in
>the relational tier, expressed as a set of statistical
>associations.

The concept of a higher-level discrete world that (re-) represents a lower-level analog world is consistent with my definitions of levels in the hierarchy; you draw the line at about the relationship or category level. But restricting the higher levels to "relationships" is too restrictive for me, and I am not in sympathy with treating the lower levels of perception as statistical (except in the sense that there is always a signal-to-noise ratio, pretty high for most perceptions).

> AU KNIGHT, KEVIN CRAWFORD.
> TI INTEGRATING KNOWLEDGE ACQUISITION AND LANGUAGE
> ACQUISITION.

>Very large knowledge bases (KB's) constitute an important step
>for artificial intelligence and will have significant effects
>on the field of natural language processing. This thesis
>addresses the problem of effectively acquiring two large bodies
>of formalized knowledge: knowledge about the world (a KB), and
>knowledge about words (a lexicon).

This treatment of the "knowledge base" leaves out the nonverbal knowledge base, which to me is indispensable in bringing order into any verbal knowledge base. If you treat a dictionary as a kind of knowledge base, and start trying to find out what some term like "inteligence" means, you end up going in small circles among a few basic terms, none of

which have any meaning unless you already know the meaning experientially. Enlarging the verbal knowledge base doesn't help with this problem. You can check the basic lexicon against a user's "intuitions," meaning experiences, but you can't get those intuitions into the computer's knowledge base. The only way to do that would be to give the computer human senses.

Bruce Nevin (921218.1350) --

>Don't we get something like discrete states in the latch
>mechanism for steps in events and sequences? And indeed for
>category perceptions and on up?

Yes, something like that. The event level is a sort of bastard level. I've never found it very useful for explaining the control of anything, except just to point at events that people seem to control. This level began life as the sequence level, and then suffered attrition as the transition level, then the level now called sequences, were peeled away and relocated. Pure sequence perception, in which ONLY ordering is important, is relatively easy to model. Transition is relatively easy to model. But the idea of a space-time pattern remains too vague for my comfort. I don't know what to do about this but to wait until someone extracts yet another more clear level from it, and perhaps leaves nothing behind at all. Follow the bouncing ball.

Transition, which is basically derivatives, clearly doesn't require latching. Pure sequence perception, in which only ordering matters, clearly does require it. I don't know what that leaves for the "event" level except doom.

>It seems to me that, as the strength of some category
>perception grows, it becomes easier (more acceptable?) to fill
>in missing category-attribute perceptions by imagination. At
>some threshold, or seeming threshold, where imagined
>perceptions are integrated with real-time perceptions, it is as
>though the exemplar of the category is perceived as fully or
>truly present, whereas before there were only unsupported signs
>or symptoms that fostered a belief, readiness, or expectation.

Another phenomenon of categories, well known in experimental psychology, is the hysteresis effect. As a figure changes shape, say between a rectangle and an ellipse, there is a point where a person switches from one label to the other. When the change is carried out in the reverse direction, the switch-point is delayed so it occurs well into the region where the other label was used when the change was going the other way. This is explained by calling it "perseveration." The person who exhibits this phenomenon has perseverosia. I suppose that curing it would call for taking an antiperseverant.

If you had to dispose of the "event" label completely and subsitute another name, what would you call it? What seems to be the central phenomenon in the observation that

>speakers and hearers perceive words (morphemes) as (probably
>event-level) sequences of discrete tokens, where the types are
>sound contrasts established by social convention for their
>speech community.

?

Martin Taylor (921218.1530)--

>I have a problem with the specific detail of Bill's proposal.
>Not with the principle.

So do I. Same problems. That's why I won't seriously propose this arrangement until such problems are worked out.

The basic idea is that the upper level controls by sending its output signal into a model, which provides the perceptions that that level controls. In general, this means that copies of the output signal will NOT produce the correct real-world results when also sent to lower-level systems, because the model will be in some respects wrong. But if the lower-level error signals are also included in the upper-level perception, they will cause errors in the upper-level system, changing the outputs of the upper system until those outputs do have the proper effects. If now the error information can be used as the basis for slow changes in the model, eventually the model will converge to a form such that there are no lower-level error signals, and controlling the model provides outputs that do produce the wanted effects in the world.

Well, that's the basic spec for the system; now all we have to do is find a way to make a simulation work like this. In doing so, I'm sure that we will find that the spec itself is confused. So don't take any of this very seriously. This is just the germ of an idea. I have one way of doing it that works, and it doesn't work the way I just described. Remind me of it some day when there's a lull and I'll post it; for reference, I call it the "Artificial Cerebellum" (just to preserve a proper air of modesty).

Tom Bourbon (921218.1438 CST) --

>In the past, both of us wondered how, specifically, Shannon's
>ideas, or any of the major concepts from information theory,
>would improve any of the quantitative predictions we make with
>our simple PCT models.

This is the right question about information theory -- not "does it apply?" but "what does it add?" The basic problem I see is that information theory would apply equally well to an S-R model or a plan-then-execute cognitive model -- there's nothing unique about control theory as a place to apply it. Information theory says nothing about closed loops or their properties OTHER THAN what it has to say about information-carrying capacity of the various signal paths. All working models have signal paths of some sort, but only the control-system model puts those paths together in a way that results in control. And in a control model, the signals in the various paths normally carry far less information than the theoretical limits allow. If an auditory channel theoretically could represent a maximum frequency of 20 KHz, according to information theory, what do the theoretical limits matter when the system is controlling for a voice tone of middle C? The most you would get from information theory would be a prediction of the minimum amount of error to be expected. But information theory doesn't tell you there will BE an error signal.

I think that the problem here is that information theory can't distinguish between a model that works and one that doesn't; a model that represents a real behaving system and a model that represents a totally imaginary system. It's like the law of forces; all the forces balance in every bridge: bridges that carry traffic and bridges that collapse. Conservation of energy and momentum, in Newtonian mechanics, applies equally well to the spacecraft that gets to Mars and the one that misses it by a million miles. In all these cases, something has to be added to get a workable system. And I don't think that this something comes from the abstract principles involved, however convincingly one can prove that they apply.

Best to all, Bill P.

Date: Fri Dec 18, 1992 4:54 pm PST
Subject: Re: Martin to Rick on Shannon

[Martin Taylor 921218 18:30] (Tom Bourbon 921218 14:38)

>May I repeat and elaborate on an invitation I made about a year
>ago? I will send you one of my programs in which two systems (two
>people, the two hands of one person, two models, or a person and a
>model) interact and produce controlled relationships. ...

I guess I'd better try to describe, as I did a year or so ago, wherein information theory helps in the understanding of PCT. I didn't succeed in getting across then, and I'm not sure I'll do any better now. I should think that the prediction for your proposed system would be no better and no worse than you would get without it, because you are dealing with a transparent system of one control level. The understanding you get with information theory is not at the level of setting the parameters.

If I were to try to develop a model to make predictions in your experiment, I expect it would look essentially identical to yours, because the key elements would be the gain and delays in the two interacting loops.

Now consider the interchanges of a week or two ago about planning and prediction, continued in Bill's post of today to Allan Randall. In those, the situation is greatly different. The information required from the lower level for the upper level to maintain control through a hiatus in sensory acquisition depends greatly on the accuracy of control maintained at the lower level. Where does that come from, and where does it go?

We come back to the fundamental basis of PCT. Why is it necessary, and is it sufficient? Let's take two limiting possibilities for how a world might be. Firstly, consider a predictable world. PCT is not necessary, because the desired effects can be achieved by executing a prespecified series of actions. No information need be acquired from the world. From the world's viewpoint, the organism is to some extent unpredictable, so the organism supplies information to the world. How much? That depends on the probabilities of the various plans as "perceived" by the world.

At the other extreme, consider a random world, in which the state at t+delta is unpredictable from the state at t. PCT is not possible. There is no set of actions in the world that will change the information at the sensors.

Now consider a realistic (i.e. chaotic) world. What does that mean? At time t one looks at the state of the world, and the probabilities of the various possible states at t+delta are thereby made different from what they would have been had you not looked at time t. If one makes an action A at time t, the probability distributions of states at time t+delta are different from what they would have been if action A had not occurred, and moreover, that difference is reflected in the probabilities of states of the sensor systems observing the state of the world. Action A can inform the sensors. PCT is possible.

In a choatic world, delta matters. If delta is very small, the probability distribution of states at t+delta is tightly constrained by the state at t. If delta is very large, the probability distribution of states at t+delta is unaffected by the state at t (remember, we are dealing with observations and subjective probabilities, not frequency

distributions--none of this works with frequentist probabilities; not much of anything works with frequentist probabilities!). Information is lost as time goes by, at a rate that can be described, depending on the kinds of observations and the aspect of the world that is observed.

The central theme of PCT is that a perception in an ECS should be maintained as close as possible to a reference value. In other words, the information provided by the perception, given knowledge of the reference, should be as low as possible. But in the chaotic world, simple observation of the CEV provides a steady stream of information. The Actions must provide the same information to the world, so that the perception no longer provides any more information. Naturally that is impossible in detail, and the error does not stay uniformly zero. It conveys some of the information inherent in the chaotic nature of the world, though less than it would if the Actions did not occur. The Action bandwidth determines the rate at which information can be supplied by the world, the nature of the physical aspect of the world being affected, and the delta t between Action and sensing the affected CEV determines the information that will be given to the sensors (the unpredicted disturbances, in other words), and the bandwidth of the sensory systems determines how much information can be provided through the perceptual signal. Any one of these parts of the loop can limit the success of control, as measured by the information contained in the error signal.

So far, the matter is straightforward and non-controversial, I think. Think of a set of orbits diverging in a phase space. The information given by an initial information is represented by a small region of phase space as compared to the whole space. After a little while, the set of orbits represented by the initial uncertainty has diverged, so the uncertainty has increased. Control is to maintain the small size, which means to supply information to the world.

Things become more interesting when we go up a level in the hierarchy. Now we have to consider the source of information as being the error signals of the lower ECSs, given that the higher level has no direct sensory access to the world, and that all lower ECSs are actually controlling (both restrictions will be lifted later, especially the latter). Even though the higher ECSs may well take as sensory input the perceptual signals of the lower ECSs, nevertheless the information content (unpredictability) of those perceptual signals is that of the error, since the higher ECSs have information about their Actions (the references supplied to the lower ECSs) just as the lower ones have information about theis Actions in the world. The higher ECSs see a more stable world than do the lower ones, if the world allows control. (Unexpected events provide moments of high information content, but they can't happen often, or we are back in the uncontrollable world.)

What does this mean? Firstly, the higher ECSs do not need one or both of high speed or high precision. The lower ECSs can take care of things at high information rates, leaving to the higher ECSs precisely those things that are not predicted by them--complexities of the world, and specifically things of a KIND that they do not incorporate in their predictions. In other words, the information argument does not specify what Bill's eleven levels are, but it does make it clear why there should BE level of the hierarchy that have quite different characteristics in their perceptual input functions.

It is that kind of thing that I refer to as "understanding" PCT, not the making of predictions for simple linear phenomena. Linear models are fine when you have found the right ECS connections and have plugged in model parameters. I am talking about seeing why those models are as they are. Look, for example, at the attention and alerting discussions, which come absolutely straight from the Shannon theory. But the results of the (almost) a priori argument agree with the (despised) results of experiments in

reading that I discussed in our 1983 Psychology of Reading. had I known about PCT then, I could have made a much stronger case than I did, but only because of Shannon. The whole notion of Layered Protocols in intelligent dialogue depends on Shannon, and demonstrates the impossibility of simple coding schemes (which some people have claimed as the basis of Shannon information).

>I invite you to add to the PCT model any features of information
>theory that you believe must be there. If necessary, use features
>from information theory to replace those from PCT.

Have I done that to your satisfaction?

> If your changes
>improve the predictions by the model, there will be no argument and
>no complaint: You will have demonstrated that a person who does
>not understand Shannon or information theory does not understand PCT.

An electricity meter reader does not need to understand the principles of electromagnetism to get an accurate meter reading. This challenge is misdirected. If there are places where I think the prediction would be improved, they are likely to be structural, such as in the division of attention, monitoring behaviour or some such. What should be improved, in general, is understanding, not meter reading.

I said that if you don't understand Shannon, you won't understand PCT. I didn't say you won't be able to use PCT to make predictions.
==========
Having said all that, I might soften a bit, and take the analogy of the use of the mathematically ideal observer in psychophysics. The ideal observer is assumed to take whatever information is in principle available in the signal, and to use it to determine whether or not some specified class of even has occurred. Trained psychoacoustic listeners often perform rather like an ideal observer who is presented with a signal some 3 or 4 dB weaker than the actual one. We say they are within 3 or 4 dB of ideal.

Now we take the observer and add or eliminate possibilities for getting information about the event. For example, we may let the observer know what the waveform of the event would be if the event actually occurred, by presenting it to the other ear. They now approach the performance of an ideal observer who knows the waveform. Can they do this if the "cue" is delayed? It depends how much delay, and by looking at the performace over a variety of delays, we can tell something about what information the real observers are losing. Is it phase, frequency, or amplitude? Change the cue and do some more variation, and determine what the ideal observer might be capable of doing if it lacked this or that kind of information.

By analogy, it might be possible to make ideal controller predictions, given different kinds of disturbance or sensory prediction aids. An example comes to mind (not of an ideal controller). A submarine reacts very slowly to changes in its control surfaces, but in stable water there are reasonably simple algorithms to determine where it will be if nothing changes in the control surfaces over the next few minutes (the chaotic world doesn't provide much information under these circumstances). So it is possible to make a display that shows a line indicating where the submarine will go if the steerer does nothing. If that's where it should go, fine. Otherwise the steerer moves the controls until the line goes where the submarine should go. But there are currents and so forth (the world provides information), so the submarine does not go where the line said it would. However, the line still predicts where it would NOW go if nothing happens, so the helm can still control that future position to some extent. How far should the line go?

That depends on the information rate of the world. If the currents are swirling and unpredictable, probably it should not go very far, but if they are steady, they provide little information, and the line can compensate.

Too long. I must go home. I hope that this has been helpful.

Martin

Date: Fri Dec 18, 1992 5:32 pm PST
Subject: Re: Misstatements; Latin mottos; Knowel,ed

[Martin Taylor 921218 19:45] (Bill Powers 921218.1500)

Well, I just put a long posting to Tom Bourbon to bed, saying I have to go home, and as I was putting on my outdoor shoes, along comes this posting from Bill, demanding a response before the weekend...

(To Bruce Nevin)
>>It seems to me that, as the strength of some category
>>perception grows, it becomes easier (more acceptable?) to fill
>>in missing category-attribute perceptions by imagination. At
>>some threshold, or seeming threshold, where imagined
>>perceptions are integrated with real-time perceptions, it is as
>>though the exemplar of the category is perceived as fully or
>>truly present, whereas before there were only unsupported signs
>>or symptoms that fostered a belief, readiness, or expectation.
>
>Another phenomenon of categories, well known in experimental
>psychology, is the hysteresis effect. As a figure changes shape,
>say between a rectangle and an ellipse, there is a point where a
>person switches from one label to the other. When the change is
>carried out in the reverse direction, the switch-point is delayed
>so it occurs well into the region where the other label was used
>when the change was going the other way. This is explained by
>calling it "perseveration." The person who exhibits this
>phenomenon has perseverosia. I suppose that curing it would call
>for taking an antiperseverant.

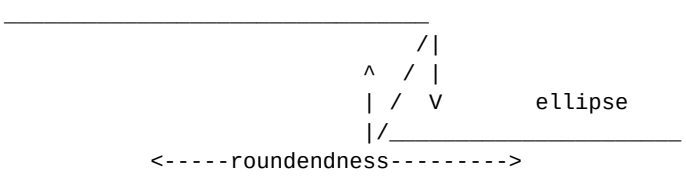
I had almost responded to this posting by Bruce, because it bears on a pretty fundamental issue that relates category perception to continuous perception (and to the information-theoretic ideas in my posting to Tom).

Before we knew about PCT, we had been developing concepts in cognition based on the requirement for quick but stable transitions among ideas (such as the recognition that a stimulus pattern represents a particular class of object). Also we were concerned with how people build on existing knowledge and how they develop analogies. All of this was within a geometric framework. I'm not sure whether I'll be any better at explaining this here than I have been at the information theory, but never mind. I wrote a 20-page draft of an introduction to the ideas a couple of years ago and quit because there was too much background to cover if the ideas were to be given justice. So...

The fundamental geometric object in our construction is a fold catastrophe. The reason is that things in the world can change continuously and (despite Bill's claim) most perceptions are based on very poor information--the SNR is very low. So if a structure

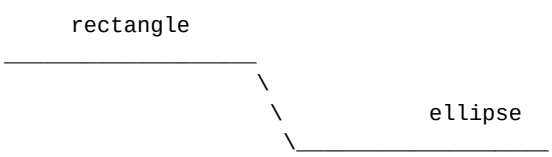
is near the boundary between two categories, it might oscillate between them if the boundary were just a threshold. The way around this is that category boundaries are not thresholds, but fold catastrophes. As the sensory data move along from, say, ellipse toward rectangle, the perception eventually changes to the rectangle branch of the fold. As the data move back the other way, the perception tracks along the rectangle branch beyond the original transition point until eventually it falls onto the other branch. Between the two, the perception is reasonably stable at whatever it had been (I say reasonably, because there is a second catastrophe function that causes a temporal hysteresis here as well, but I don't want to go into that).

Here's my best shot at an ASCII view



Now we have to add another dimension. In fact we do it twice, and once we call the added dimension "stress" and once "learning." The picture looks the same for each.

Consider the "roundedness" axis. If you don't know about ellipses and rectangles as categories, you could describe the object as "a kind of squarish circle" or something like that, with adjectives to describe the degree of squarishness. There's no fold catastrophe. If you begin to learn, a precise rectangle might be a "rectangle" and a clean ellipse an "ellipse", but in between, it wouldn't really be anything. You have something a bit like:



(I can't draw a shallowly sloping line, which would be the early stage I described. This is a later stage, when the categories are moderatly developed).

Under no stress to categorize, behaviour with respect to the pattern will be along the shallow slope, varying continuously as the roundedness varies. But under strong stress, either task dependent (rectangles give you money, ellipses don't) or context dependent (like alphabetic symbols rather than ellipses and rectangles), the behaviour varies on the fold pattern, The whole structure is a cusp catastrophe, in which the object is primarily a category to which adjectives can be applied when it is on the fold, or primarily an uncategorized entity when it is on the slope. The catastrophe allows a different kind of perceptual control--going up a level.

When the category is identified, the other attributes that go along with it are subordinated to the fold. What does that mean? The first thing to note about a catastrophe is that it requires some kind of POSITIVE feedback to maintain it. There must be a memory, of cross-feeds from related perceptual functions, or something like that. What this means is that teh existence of the category in part substitutes for sensory information. The effect Bruce mentions is a natural consequence, and what Bill

mentions is a simple description of a necessary phenomenon, of having the ability to form categories.
==========
Bill to Tom Bourbon (921218.1438 CST) --
>
>>In the past, both of us wondered how, specifically, Shannon's
>>ideas, or any of the major concepts from information theory,
>>would improve any of the quantitative predictions we make with
>>our simple PCT models.
>
>This is the right question about information theory -- not "does
>it apply?" but "what does it add?" The basic problem I see is
>that information theory would apply equally well to an S-R model
>or a plan-then-execute cognitive model -- there's nothing unique
>about control theory as a place to apply it. Information theory
>says nothing about closed loops or their properties OTHER THAN
>what it has to say about information-carrying capacity of the
>various signal paths.

You are right, but that "OTHER THAN" is a pretty big place to hide very important stuff. I had not previously realized that you wanted me to use Shannon to differentiate between S-R and Plan-then-execute. I think I did incidentally make that discrimination in my posting in response to the same posting by Tom. At least I think I showed how applying Shannon demonstrated that neither S-R nor Plan-then-execute could be viable. But we knew that already, so I didn't play it up.

> All working models have signal paths of
>some sort, but only the control-system model puts those paths
>together in a way that results in control.

Yes, you found the importance of that long ago without recourse to the necessity arguments that come from from information theory, but that doesn't make the arguments less relevant. Newcomen and Watt built steam engines of some power without worrying about the proper thermodynamic cycles. But we build more efficient ones nowadays.

>And in a control
>model, the signals in the various paths normally carry far less
>information than the theoretical limits allow.

Dubious. I would like to be able to figure out how to test that assertion. My intuition/prejudice about evolution is that it seldom wastes resources very badly, and I would be surprised if your statement is true about the real live control systems that have evolved (as opposed to control "models").

> But information theory doesn't tell you there will BE an error signal.

As I pointed out to Tom, it does.

>In all these cases, something has to be added to
>get a workable system. And I don't think that this something
>comes from the abstract principles involved, however convincingly
>one can prove that they apply.

Yes, that's absolutely right. Natural laws are no use without boundary conditions to describe particular situations. But if you understand the abstract principles, you can make better [bridges/kettles/radios/control systems].

Martin

Date: Sat Dec 19, 1992 2:09 am PST
Subject: Information theory vs control theory

[From Bill Powers (921219.0130)] Martin Taylor (921218.1830) --

Your remarks about the predictability of the world and PCT are mostly cogent. This is particularly true if you include the predictability of the output effectors as well.

I can quibble, however, about a number of points.

>In a choatic world, delta matters. If delta is very small, the
>probability distribution of states at t+delta is tightly
>constrained by the state at t. If delta is very large, the
>probability distribution of states at t+delta is unaffected by
>the state at t ...

It matters a heck of a lot more to a plan-then-execute model than it does to a control model. Remember that control of a variable depends only on the ability of the system to affect that variable, directly, in present time. It isn't necessary to produce an output and wait to see its future effects. If progress doesn't follow the intended path, correction occurs right away, after a short delta. So in properly-designed control systems, delta is always small. Even if the stated goal is far in the future, the path to the goal can be defined as a reference path, and control can assure that progress stays on that path. This, in fact, is the only practical way to control for long-delayed ends.

In circumstances where perceptions are uncertain and there are long delays in the control loop, control is simply going to be poor. Predictive control with a large delta is doomed to be lousy.

>The central theme of PCT is that a perception in an ECS should
>be maintained as close as possible to a reference value. In
>other words, the information provided by the perception, given
>knowledge of the reference, should be as low as possible.

I think you'd better take one that back to the drawing board. The reference in no way predicts the perception by its mere existence. The best control requires the widest bandwidth in the system, including its input function, up to the point where noise begins to become significant. I don't see how this is consistent with saying that the information provided by the perceptual signal should be as low as possible.

It may be that given an excellent control system, the state of the reference signal does predict the perceptual signal well, so in that case an observer will find knowledge of the actual state of the perceptual signal redundant. The information provided by the perceptual signal TO THAT OBSERVER is low. But that would have absolutely nothing to do with the design of the control system. That's a calculation by an external observer, not a property of the system. The only reason for which the perceptual signal provides low information to the observer is that it provides a great deal of information to the comparator. I still think that your analysis is simply descriptive, and has nothing to do

with the design of control systems. It may apply to a successful design, but it can't provide a successful design.

>In other words, the information argument does not specify what
>Bill's eleven levels are, but it does make it clear why there
>should BE level of the hierarchy that have quite different
>characteristics in their perceptual input functions.

>It is that kind of thing that I refer to as "understanding"
>PCT, not the making of predictions for simple linear phenomena.

Dennis Delprato, here is another addition to your list of myths about PCT: that we can predict only simple linear phenomena. Martin, have you looked at the Little Man? It is chock full of nonlinearities. We have actually tested it using a much more realistic muscle model -- in fact one that proved to be far more nonlinear (6th power) than the actual muscle (2nd power) owing to a misunderstanding of a published model (which tried to include the limits of limb travel in the same equation!). And the feedback path from output torques to visualized position of fingertip, and the method of depth perception for the target, are highly nonlinear.

We can't solve the nonlinear equations analytically (nobody can), but that is not a constraint on the simulations. In my psych review article so long ago, I showed tracking data with a cubic relationship between handle position and cursor position that actually reversed slope in the middle. The model handled it just the way the real subject did, by skipping across the region of positive feedback. Rick Marken's experiment with size control modeled one case in which the controlled variable changed as the square of handle position. No problem. We have tried all sorts of nonlinear functions. But there's no point in teaching control theory using nonlinear equations that nobody can solve.

>But the results of the (almost) a priori argument agree with
>the (despised) results of experiments in reading that I
>discussed in our 1983 Psychology of Reading.

What do you mean, they "agree?" Do you mean that you predicted the reading performance of every single subject with an error of five percent or so? Or even 20 percent? Come on, what are you calling "agreement?"

>I said that if you don't understand Shannon, you won't
>understand PCT. I didn't say you won't be able to use PCT to
>make predictions.

Why am I reminded of that poem about Hiawatha?

>>And in a control model, the signals in the various paths normally carry far
>>less information than the theoretical limits allow.

>Dubious. I would like to be able to figure out how to test that assertion.

It's easy. Most perceptions occur on a scale between 0 and maximum magnitude, and vary at a rate between 0 and some maximum cutoff frequency. To accomodate the maximum magnitude and frequency, the perceptual channel must have a certain information capacity. As perceptual signals can be controlled at any level within the whole range and can be varied at any rate up to the maximum, it follows that unless the perception is being controlled at maximum magnitude and the reference signal is changing at the maximum rate that still permits control, the actual information flow must be much less than the

channel capacity. Most perceptions are not controlled at their extremes; hence most perceptions must use less than the whole channel capacity.

>> But information theory doesn't tell you there will BE an error signal.

>As I pointed out to Tom, it does.

Show me where Shannon's theory says there must be a comparator, a reference signal, and a perceptual signal.

>Natural laws are no use without
>boundary conditions to describe particular situations. But if
>you understand the abstract principles, you can make better
>[bridges/kettles/radios/control systems].

This brings us right back to Tom's challenge. We have, for example, a simple tracking model that predicts behavior with an accuracy of better than 5%, measured as RMS error between model and real handle behavior divided by peak-to-peak real handle excursion (a signal-to-noise ratio of 20:1, by a standard measure in electronics). What can we do to this model, using information theory, that will make it predict any better?

I think that information theory is by its very nature a post-hoc description, not a model. You can't start with information theory and come up with a system design. Or so sez I.

Backing up to pick a sentence I passed over:

>The reason is that things in the world can change continuously
>and (despite Bill's claim) most perceptions are based on very
>poor information--the SNR is very low.

I think you're letting theory triumph over observation. I have just shown that we can predict behavior with an SNR of 20:1. We do this in a routine way. The SNR of the perceptual channel certainly can't be _lower_ than that. Do you consider 20:1 very low?

I come back to my basic statement about the levels I've defined in the hierarchy: they refer to the world you observe. The world you observe does not have a predominance of noise in it. I know that you have said, "Oh, you're talking about _conscious_ perception, which is a different matter." But I see no reason to suppose that conscious perception consists of anything but neural perceptual signals, the same signals that are there when we are using the same control systems unconsciously, as in standing erect. My theory of perception agrees with a largely noise-free experienced world; yours appears to predict a world in which perception barely stands out over the background noise. If your model were correct, precise control would be impossible. Yet we manage to control variables of all kinds with great precision, even variables like the form of an algebraic expression (as in proving trigonometric identities). We don't control ALL variables precisely, but the reason is usually not that there is some inherent imprecision in the process itself, but that we're attempting to control something we have conceived poorly, or that nature dictates is not amenable to control (like another person).

One last observation:

>The way around this is that category boundaries are not
>thresholds, but fold catastrophes.

That's a pretty fancy term for a Schmidt trigger. Anyway, saying that categories are fold catastrophes says nothing that my description of hysteresis didn't say. Categorizing categorizing doesn't tell us how it works. It doesn't work the way it does because it's a fold catastrophe. It's a fold catastrophe because of the way it works, which remains undisclosed.

Best, Bill P.

Date: Sat Dec 19, 1992 7:06 pm PST
Subject: Re: awareness of error/ignorance

[from Gary Cziko 921219.0440 GMT] Bruce Nevin (Fri 921218 13:24:41)

I enjoyed your:

>> Melius invenitur veritas ex errore quam ex ignorantia
>>
>>"It is easier to arrive at truth from error than from ignorance."

exchange with Martin Taylor. Your discussion reminds me of Socrates who when told that the oracle had said he was the wisest man in Athens set off to find wiser men. He discovered that whom he considred the wisest men thought they knew a lot but didn't really know anything while Socrates knew that he himself knew nothing but knew he knew nothing. So he finally agreed that the oracle was right after all.

I like the motto, but is your Latin good enough to make a revision? I would much prefer something like "get closer to" or "draw nearer to" than "arrive at." Would something like "advenitur" do it? This reflects my Popperian sympathies.--Gary

Date: Sun Dec 20, 1992 8:38 am PST
Subject: A Useful Insight at last

[Gabriel to NET 921220 10:01 CST]

Bill P and I have had an offline discussion that generated lots of light for me. So I want to share. We have two rather different views of Control Theory and hence of BCP. I think this serves to unify the two positions in exactly the technical sense of being the least general theory that includes both.

I'm going to avoid the word "Control" because it has so many different meanings. This does not signify any quarrel with BCP - just that there's a mathematical theory including BCP as a special case that I'm comfortable with.

The term "Reference Signal" is replaced by "Desired State (of the observed environment)" for similar reasons. The term "Reference" has Rock of Gibraltar like connotations, but I think we can all agree it's possible to change our desires from time to time, just as the BCP signal topology allows and encourages - i.e. the "desire" comes from within, not from without.

Thus we begin with two states of the external environment, Desired (D), and Perceived (P). For the unified theory it is sufficient for D and P to have "representations" in the mathematical sense, as points in a "metric space". This is a space where "distance" has a meaning, of one or more dimensions, each of which may be discrete or continuous. This

allows us to talk about the distance between D and P, and because the space is a metric space, we are guaranteed not to reach any absurd conclusions.

Arm and Man live in 6N dimensional metric spaces - 3 spatial coordinates for each degree of freedom, and 3 momenta for each degree of freedom. If you are doing rigid body kinematics, the 6 becomes a 12, but no matter.

Most of my concepts live in many dimensional spaces where the component of state in one of the dimensions has two or three possible values - TRUE/FALSE or TRUE/UNKNOWN/FALSE. If we increase the number of possible state values in a single dimension to say 256 or 1024 or 8192 .... we can approach a continuous state space as closely as we wish, we may exactly represent the digital computational models of Man and Arm, and in fact we may represent any physical system where there is noise, either thermal or quantum, within the precision of experimental observation.

A metric space has one other imporant property. Given two points say D and P we have a well defined distance between them. Distances MUST obey the triangular inequality, if A B and C are in the space, then

AB + BC >= AC

Now the central thesis of BCP can be stated.

People behave to move P closer to D, where this is possible at an acceptable cost.

Notice this is a constrained minimisation process. Also unlike the error P-R of BCP, DP is always positive, and may not always attain the desired minimum of zero. But the version of control theory used in BCP does in fact minimise DP for the cases considered in pratcice.

Notice also that D and P also have representations by data streams, i.e. by samples in the metric space at time intervals appropriate to the Nyquist criterion for lack of aliasing. P type data comes from sensors. D type data comes from desires.

In this case Shannon's Theory allows a metric space to be constructed (with a weighted Hamming metric) pretty much regardless of the details of the data streams, provided they are sequences of arrivals of symbols (including pulses of depolarisation across nerve membranes).

I think for the moment I need say little more. In both the continuous and discrete cases orthogonal function expansions are possible with arbitrary positive definite metric for the scalar products. These metrics have to do with power limits, or value systems.

This is why the detailed machinery of Control Theory does the job. Essentially the metric space allows Fourier Analysis and Laplace Transforms, which lead to the control formalism of BCP. But in some cases minimisation algorithms, taking account of the possible may be closer to the real world. All this also includes a lot of other things, like population genetics if you replace the machinery moving D around with Darwinian Selection. The story gets a little different but not very much.

Over and out. I'm exhausted. John

Date: Sun Dec 20, 1992 8:49 am PST
Subject: Signing out for a while

[Gabriel to Cziko 921220 10:43CST] Dear Gary

Please unsubscribe me for a while. I'm not taking my toys to play elsewhere permanently, but there is other work I MUST do. I'll rejoin in Spring or Summer '93.

Best to All, Peace, Joy, and many more years of happy and thoughtful purusit of Truth and Beauty.

John

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
>introducting 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: Sun Dec 20, 1992 3:17 pm PST
Subject: Gabriel on control

[From Bill Powers (921220.1330)] John Gabriel (921220.1001) --

I assume that Gary won't have unsubscribed you so quickly, but I'll copy this to you direct anyway.

>I'm going to avoid the word "Control" because it has so many
>different meanings.

Control seems to have more different usages than it actually has. People use this term in what seem to be quite different ways, but when you examine the circumstances in which they use the word, I think you often, maybe always, find that they have an incomplete understanding of the situation to go with the incomplete understanding of what control means. They're really assuming a complete control loop, but paying attention only to part of it and taking the rest for granted.

To see how common usages are incomplete, just ask a few questions organized around PCT.

Control is used in the sense of restraining or limiting. Consider controlling a dog by leashing it. Clearly you need an action, pulling on the leash. But suppose you weren't allowed to perceive any consequence of this action: you can't feel any pull on the leash and you can't see any dog on the end of it. This would clearly be an unsatisfactory kind of "control." You want to perceive that the dog is in fact on the end of the leash: see the dog and feel the pull. If you chain a dog in the yard, you must be able to see that the chain is attached between a stake and the dog and that the dog remains on the end of the chain and the stake remains in the ground. If you can't perceive those things, you feel uneasy about claiming that the dog is under control.

Control is used in the sense of a "control experiment." You do a control experiment by duplicating every condition except the one you manipulate for the real experiment. Suppose you were allowed to do a control experiment, but were not allowed to see the results. Would you still think of it as a control experiment?

Control is used to describe the relationship between an authority and a subordinate. Would the authority feel that commanding a subordinate's action amounted to control if there were never a report on or observation of the action or its effects?

Control is used in the sense of "determine." The temperature of reactants controls the rate of the reaction, because the reaction rate is a function of temperature. If you were not allowed to perceive either the temperature or the reaction rate, would you be able to claim that temperature was in fact controlling the reaction rate? More subtly, could you say that the reaction rate was controlled if you weren't both perceiving it and comparing it with some reference reaction rate?

A control is something like a steering wheel or a brake pedal. When a control is operated, it has an effect on something, like speed. But does the control by itself bring the speed to some specified value? Could you use the brake pedal as a control of the car's speed if you could not somehow perceive the car's speed?

I won't drag this out. In all the cases I find as synonyms of control in a thesaurus, the meaning would become irrelevant if perception of the consequences weren't allowed and if there weren't a preferred, expected, or desired state of the consequences. In all such situations, the speaker is taking for granted the perception and the reference level. Normal usages of control amount to synecdoche: referring to a whole by mentioning some part of it.

Even in the technical world, this sort of synecdoche happens. I have heard people talk about "open-loop control." But suppose you design an open-loop controller, and are

allowed to see only the input that you give it, not its output effects. You would be unable to calibrate it, and while it was operating you would not know (perceive) whether it was having the effect it was supposed to have (i.e., the reference condition).

I think that when people use the word control, there is ALWAYS a closed loop involved or implied even when it's not mentioned. In the background there is always someone perceiving the result and comparing it with an intended or desired result.

>The term "Reference Signal" is replaced by "Desired State (of
>the observed environment)" for similar reasons. The term
>"Reference" has Rock of Gibraltar like connotations, but I
>think we can all agree it's possible to change our desires from
>time to time, just as the BCP signal topology allows and
>encourages - i.e. the "desire" comes from within, not from
>without.

Reference signals in the HPCT model are continuously adjustable; they are the means by which higher systems act to control their perceptions. A reference signal is not supposed to connote something fixed. It is simply the momentary target toward which perceptions are being adjusted. We often say "desired state".

>Thus we begin with two states of the external environment,
>Desired (D), and Perceived (P).

This is right in terms of commonsense usage, but in the model we explicitly recognize that all the system knows about its environment is represented by its internal perceptual signals. So we would say that we begin with two states of perception, not of the environment: the actual perception, and the reference perception embodied as an adjustable reference signal.

You say that for the unified theory it is sufficient for D and P to have "representations" in the mathematical sense, as points in a "metric space". This presumes that we know what D and P "really are" outside the perceptual system -- that is, it assumes that we know how to take the inverse of the perceptual functions that lie between our perceptions and the world from which they are drawn. My view is that D and P are ALREADY representations in a metricized perceptual space at the time we become aware of them. The problem of perception is not to translate from real- world or objective variables to the perceptions that we assume to represent them in the brain, but to translate from the perceptions that we experience BACK to the hypothetical objective world which we assume to underlie the phenomena -- the world we imagine in terms of physical models. The real world is the inverse of an unknown function of the variables we experience.

Skipping ahead:

>Now the central thesis of BCP can be stated.

> People behave to move P closer to D, where this is
> possible at an acceptable cost.

The term "acceptable cost" hides the rest of the story: cost in terms of what variables, and relative to what reference states, and in whom?

I would generalize it more or less this way:

People behave to move P[i] closer to D[i], where this is does not cause the set of all P[k] to move significantly away from D[k], k <> i.

If you like, you can let k range over people as well as within people.

"Significance" of a deviation of P[k] from D[k] is evaluated in terms of error sensitivity: how much corrective action results from a unit of deviation. Thus we don't count a deviation as a cost if a person makes no effort to correct it.

In this way we get rid of the quasi-objective connotations of "cost," and cast the whole process in terms of the system's own operations and internal goals.

>Notice this is a constrained minimisation process. Also unlike
>the error P-R of BCP, DP is always positive, and may not always
>attain the desired minimum of zero.

"Minimization" of DP is not an effective mode of control, because a departure from minimum doesn't contain the information needed to direct action the right way. A system that simply "minimizes error" without regard to its sign would have to use some sort of hill-climbing strategy to get the sign of the output right. The error signal in BCP is signed for a good reason.

Furthermore, to say that DP is minimized doesn't pin down the state of D or of P at which this happens. In fact, during control it is normally not minimized. If I desire to be eating 3 units of ice cream and perceive myself eating 3 units of ice cream, the error becomes 0 but DP becomes 9. Actually the minimum of DP would occur in a state of large error: e.g., D = 3 and P = 0, or P = 3 and D = 0. Somehow I don't think that the system tries to minimize DP. Why would it try to do that?

>But the version of control theory used in BCP does in fact
>minimise DP for the cases considered in practice.

No, it minimizes |D - P|, which is not at all the same thing as minimizing DP. And it doesn't "minimize" anyway; that's a side- effect. The response of a control system to D - P is SIGNED, so there is no need for hill-climbing or other such ways of achieving minima. Naturally, if you make P approach D, then |D-P| is in fact minimized even though the system does not calculate that absolute value. But I can't think of any case in which DP would be minimized unless D = 0.

Enough for one post. Take a nap.

Best, Bill P.

Date: Sun Dec 20, 1992 6:08 pm PST
Subject: Perception and the environment

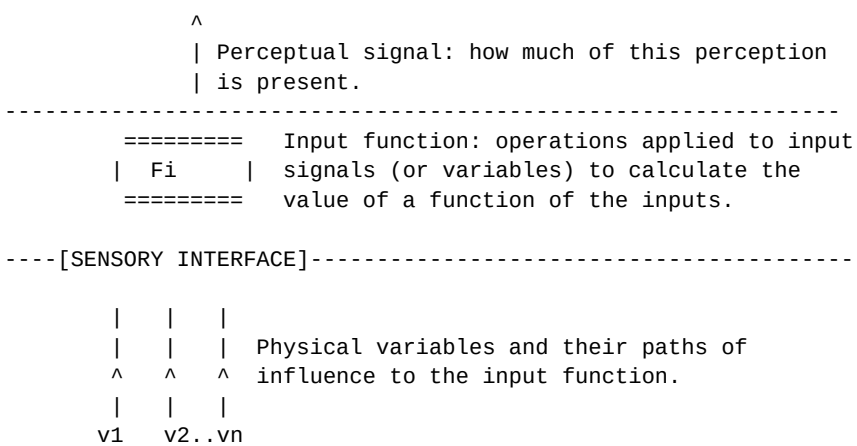
[From Bill Powers (921220.1745)] Wayne Hershberger (921219) --

I think I commented on that post. Anyway, here's what I have to say now. Brace yourself: I have a lot to say.

You say:

>I am saying that when taking something apart that works, one
>wants to keep track of all the working parts and to not mistake
>a limited set of parts for a complete set.

Here are some of the parts involved in perception as I model it.



Now, clearly the set of v's is a fixture of the model environment. For a given set of v's, any number of input functions Fi can be constructed (even in parallel) which produce perceptual signals that depend differently on the detailed behavior of the v's. Therefore the v's themselves should not be considered as a part of the perceptual process.

The form of the function Fi determines how the perceptual signal will change as the v's go through various detailed changes. The value of the perceptual signal will represent an aspect of the set of v's that will be invariant if the v's change in certain proportions, and variant if they change in any other proportions. Thus the magnitude of the perceptual signal represents the state of the v's as seen through a particular form of input function.

While the v's remain constant, it is possible to alter the form of Fi. Doing so will (in general) alter the value of the function, which is to say that the perceptual signal will change to a new value. If the v's then go through the same patterns of change as before, the perceptual signal will no longer be invariant for the same proportional changes as before, and it will not vary in the same way as before when the v's go through other patterns of change in other proportions. In short, the perceiving system will experience a new entity in the environment that obeys different laws even though the v's are changing in the same ways.

I don't know the advanced concepts behind all this, but it's clear that with n variables in the environment, we have an n-dimensional space, each axis being defined by one v. If there were two variables, an input function that computed weighted sums of powers of the individual stimulations at the sensory interface would create a two-dimensional family of curves which do not cross. These parallel curves would trace out ways in which the variables can change in v1-v2 space while producing a constant value of perceptual signal. If the environment changes so that the v's remain in the relationship defined by one of these curves, the input function will produce a constant signal: the system receiving the perceptual signal will experience a steady environment.

If the environment changes so as to move the v's from one curve to another parallel one, the perceptual signal will change according to the separation of the curves. This kind of

change, orthogonal to the "curves of indifference," is reported as a change in the perceptual signal.

The behavior of the v's is therefore perceived only along trajectories orthogonal to the curves of indifference. All such trajectories are equivalent in terms of the perceptual signal. The curves of indifference are created entirely by the input function; they are not a property of the v's, but of the perceptual apparatus.

It is perfectly possible that there are natural laws relating the v's. It might be true, for example, that (v1^2 + v2^2) = constant. In that case, the v's would always vary in such a way that the point v1,v2 lay on a circle on a plot of v1 against v2. This circle would intersect the lines of indifference created by the perceptual input function. As the v's varied, the point representing them would move around the circumference of the circle, and during one orbit the point would pass from one curve of indifference to another and back again.

The perception, however, would not represent the fact that v1^2 + v2^2 = constant. As the point moved uniformly around the circle, the perceptual signal would vary in some sort of distorted sine wave. The actual invariance represented by the natural law would not appear in perception at all.

In fact the behavior of the perception is related lawfully to the behavior of the point in v1-v2 space, but the law is due to the form of the perceptual function, not to the form of the natural law relating v1 to v2. The effect of the natural law constrains the way the perception will change, but that constraint is not evident in perception. All we see is the combination of the natural law and the law represented by the form of the perceptual function.

In adapting to a particular environment to get control of it, the brain reorganizes. Perceptual reorganization alters the curves of indifference, and thus alters the behavior of a given environment that will be perceived. The brain's problem is to find organizations of the input functions that will yield controllable variables, and then controllable variables that have a bearing on survival or well-being -- and it must do so without knowing anything about the v's except what is represented in the form of perceptual signals. All the criteria for selecting one perceptual function over another must be internal, in the end.

In trying to learn how the brain's control systems become organized, we have to try to figure out how it could settle on a set of perceptual organizations that will yield an adequate set of controllable perceptions. We already know that when multiple systems perceive and control the same collection of v's at the same time, there is a minimum-conflict arrangement in which the various input functions provide orthogonal representations of the external environment. This constrains only the whole set of systems that operate simultaneously, so we can't deduce a priori what the "axes" of each set of curves of indifference would be; all we can say is that all the curves, ideally, would cross at right angles where they intersect. Exact orthogonality isn't necessary unless we exhaust the degrees of freedom of the environment, which is highly unlikely to be a problem. But the more orthogonal the axes of control, the smaller all the error signals can be when all the reference signals are matched by their respective perceptual signals.

Obviously the brain manages to arrive fairly quickly at a very satisfactory set of control systems (although one can always ask, "compared to what?"). So whatever the trick is, it must be fairly simple. Perhaps it depends heavily on evolutionary preparation for

the rapid learning that occurs right after birth of a human being. Figuring out what is required from than angle could be complicated indeed.

At any rate, none of this answers the basic epistemological question as to whether the final set of perceptions comes to fit the environment in some special veridical way, or whether there is a large component of arbitrariness in it. We have no way of answering this question except to build a model of the brain that shows how the self-organizing process interacts with a hypothetical environment. Not having any way to look directly at the v's in the environment, we will never be able to verify our conclusions, whatever they are. The best we can hope to find, eventually, is a story with the virtues of being both simple and convincing. I don't think we are anywhere close to doing that.

I would like to know what you think of this argument. Do I need to worry that you won't tell me?

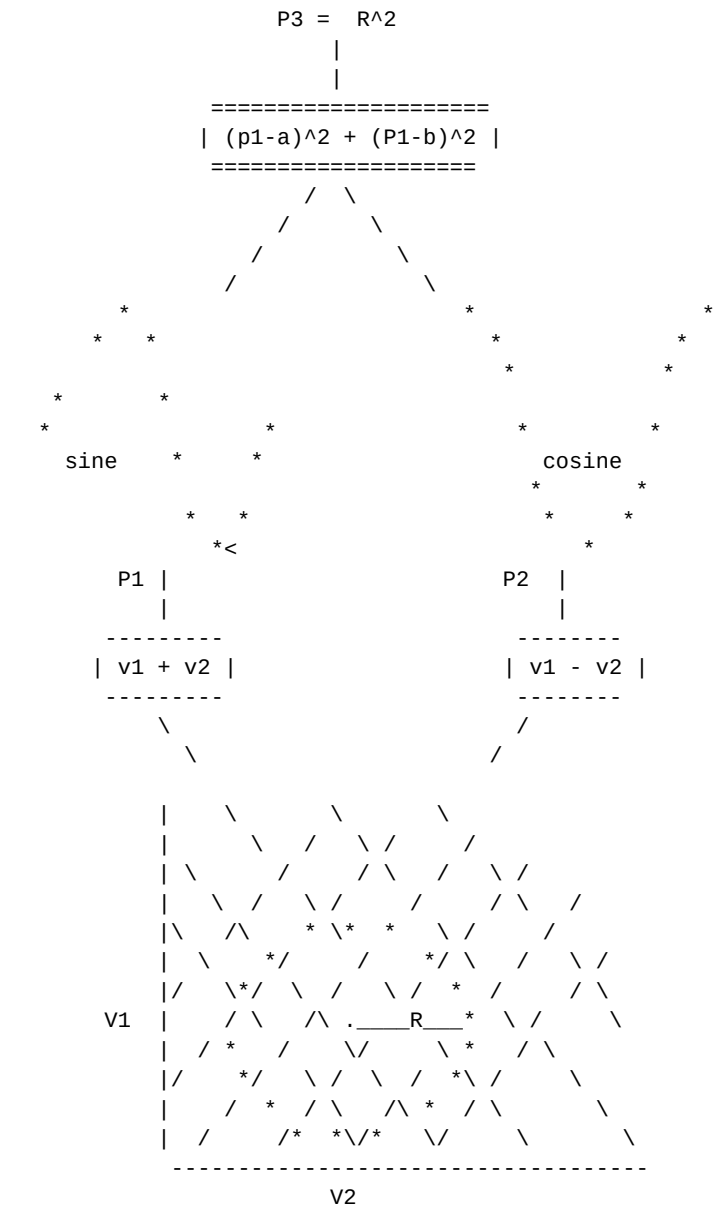
Best, Bill P.

Date: Mon Dec 21, 1992 5:36 am PST
Subject: More on perceiving invariants

[From Bill Powers (921221.0630)]

Morning thoughts on perceptual invariants.

The following is a diagram of the relationships in my post of 921220.1745. At the bottom is the v1=v2 space with a circle of radius R plotted in it, and two sets of "lines of indifference" due to two perceptual functions, p1 = v1 + v2 and p2 = v1 - v2. Some added thoughts about this were in my head when I woke up.



If v1 increases while v2 decreases by the same amount (lines slanting down and to the right), P1 remains constant. If v1 increases while v2 increases by the same amount (lines slanting up and to the right), P2 remains constant.

A point moving around the circle plotted with *** will generate a phase-shifted sine wave in P1 and a phase-shifted cosine wave in P2. As long as a point remains somewhere on the circumference of the circle, whether moving or not, it will generate values of P1 and P2 with amplitudes in a quadrature relationship (sine and cosine of the same angular variable).

If now a second level of perceptual function is added which computes the sum of the squares of P1 and P2 suitably offset by subtraction of constants, the resulting

perceptual signal P3 will have a magnitude proportional to the square of the radius of the circle in v1-v2 space. As the point in v1-v2 space moves around the circumference of the circle, P3 will remain constant. So all points on the circumference of the circle will produce the same magnitude of perception at level 2.

If, on the other hand, a point lies off the plotted circle, which is to say on the circumference of a larger or smaller circle, the perceptual signal P3 will become larger or smaller accordingly. We can therefore say that the perception P3 is a perception of the radial distance of a point from a center a,b without regard for the direction from that point. It can also be called the perception of size of a circle, as all points on a circle of one size will give rise to the same magnitude of P3, while points on different-sized circles will give rise to correspondingly different mangnitudes of P3.

Note that the location of the point from which all radial distances are computed is determined by the constants a and b in the perceptual function, not by the location of the plotted circle in v1-v2 space. To perceive an invariant with respect to direction from a center, the constants a and b must be adjusted to fit the location of the circle, or the circle must be shifted to the location specified by a and b. A shift in a and b might correspond to a control process by which attention (or gaze) is shifted to the centroid of a figure, with the effect of exposing any invariance with respect to rotation that might thus be created.

The lines of indifference are suggestive of the line-detectors found by Hubel and Wiesel. These line-detectors would represent the outputs of perceptual functions of the form P = k1*v1 + k2*v2. Implied is an underlying orthogonal v1-v2 coordinate system in visual perception (if vision is what we are talking about here) at the lowest level, where v1 and v2 are perceived.

These relationships must be richly suggestive to a mathematician. Unfortunately I am not a mathematician. Note that the space involved does not have to be visual space, nor do the coordinates have to be orthogonal in any geometric sense. All that is needed is that v1 be capable of varying independently of v2. And of course this treatment could be extended to spaces of any dimensionality.

All of this gives me a faint sense of encouragement about the idea that perceptions may have something fundamental to do with the universe for which they stand. On the other hand, for all I know they prove once and for all that the universe is totally defined by the way our peceptual functions are organized and we will never know its basic nature, if it has a basic nature. The mathematical reasoning required here simply exceeds my abilities.

Best to all, Bill P.

Date: Mon Dec 21, 1992 8:08 am PST
Subject: Re: Last thoughts; Auf Wiedersehen

Thank you Bill. I read yr previous post before this one. Let's keep in touch. My reply to previous is dead serious. I shall always have an enormous respect and admiration for what you have done.

You are absolutely right about minimisation and control being interchangeable. That's all a minimisation system is - a control system trying to make an objective function as small as possible.

The only things that give you trouble in going to a continuous control model are those cases, like Kanerva, where dist(P,R) does not have derivatives. And for those you need a control system that hops from point to point, instead of gliding smmothly along its' (as Bill C would say) "preordained" path.

But none of these things matter. YOU were there first, and all that I'm doing is a little bit of mathematician's nitpickery.

We do have different world views and perceptual histories. I always look for abstraction, and you most of the time, look for concrete examples and machinery. Doesn't matter, we are both necessary parts of the ecology.

John

Date: Mon Dec 21, 1992 9:52 am PST
Subject: neurosci, bye

Tom,
I principle I agree with you. I agree that we don't just Describe differently in PCT--ideally we would do research differently some of the time. But I think we can describe non-PCT research in PCT terms and I think that some of the research done is exactly how we would (or could) do it. I didn't say all. I am aware of the S-memory-R research paradigm you refer to, but I don't find myself exposed to this as much as you evidently do. Some forms of memory, such as priming, are in the Connections, not at all capable of being conceived of as between S and R. I'm even looking for such behavioristic paradigms and not seeing it in neuropsych literature (although I certainly did see it in cognitive psych lit). C'mon, you can't tell me that you would not do a delayed matched to sample task where a monkey has to remember under which bowl the food is that it is controlling for (poorly worded). What is wrong with that. Don't you want to know what areas are involved in this particular instance of control? I will be away from e-mail till Jan 1 but I would like to continue this discussion--if I am wrong I want to be convinced. You can send replies directly to me at m-olson@uiuc.edu

Have a great holiday!

Mark

Date: Mon Dec 21, 1992 11:07 am PST
Subject: Re: Information theory vs control theory

[Martin Taylor 921221 12:00] (Bill Powers 921219.0130)

Well, given last year's experience, I didn't expect my information-theory posting to be understood, and I wasn't disappointed in my expectation. Is it worth trying some more? I'll give it a little shot, and then give up if it still doesn't work (rather like CSG papers trying to get into conventional psychology journals, isn't it!).

>>In a choatic world, delta matters. If delta is very small, the
>>probability distribution of states at t+delta is tightly
>>constrained by the state at t. If delta is very large, the
>>probability distribution of states at t+delta is unaffected by
>>the state at t ...
>

>It matters a heck of a lot more to a plan-then-execute model than
>it does to a control model. Remember that control of a variable
>depends only on the ability of the system to affect that
>variable, directly, in present time. It isn't necessary to
>produce an output and wait to see its future effects.

The statement is completely independent of what is acting on or looking at the world. It has to do only with the rate at which the world supplies information that can be looked at. Of course it matters "a heck of a lot more to a plan-then-execute model than it does to a control model." Didn't I demonstrate that adequately in my posting?

But the delta between the output and observable effects on sensory inputs is important to the amount of information contained in the error signal and thus valuable for higher levels of control.

>>The central theme of PCT is that a perception in an ECS should
>>be maintained as close as possible to a reference value. In
>>other words, the information provided by the perception, given
>>knowledge of the reference, should be as low as possible.
>
>I think you'd better take one that back to the drawing board. The
>reference in no way predicts the perception by its mere
>existence. The best control requires the widest bandwidth in the
>system, including its input function, up to the point where noise
>begins to become significant. I don't see how this is consistent
>with saying that the information provided by the perceptual
>signal should be as low as possible.

The word "should" seems to be ambiguous. It refers in my posting to the results of having a good, properly functioning ECS. In your comment, you take it to refer to how a functioning ECS is to be designed, and that the perceptual bandwidth should be low. If the perceptual bandwidth is low, then the ECS will have difficulty matching the perceptual signal to the reference signal, and thus the error signal will have high information content. Now it is true that if the perceptual signal has lower bandwidth than the reference signal and the same resolution, then the error signal will in part be predictable, thus having lower information content than would appear on the surface. But I had the presumption that we are always dealing with an organism with high bandwidth perceptual pathways, so I forgot to insert that caveat.

>>It is that kind of thing that I refer to as "understanding"
>>PCT, not the making of predictions for simple linear phenomena.
>
>Dennis Delprato, here is another addition to your list of myths
>about PCT: that we can predict only simple linear phenomena.
>Martin, have you looked at the Little Man? It is chock full of nonlinearities.

I think you know from all my postings, including the one you are commenting on, that I don't subscribe to that myth. Sloppy wording. Sorry. But Tom specifically asked me to improve upon the numerical predictions made by a linear model, which is why I made the posting in the first place.

>>>And in a control
>>>model, the signals in the various paths normally carry far
>>>less information than the theoretical limits allow.
>>Dubious. I would like to be able to figure out how to test that assertion.

>
>It's easy. Most perceptions occur on a scale between 0 and
>maximum magnitude, and vary at a rate between 0 and some maximum
>cutoff frequency. To accomodate the maximum magnitude and
>frequency, the perceptual channel must have a certain information
>capacity. As perceptual signals can be controlled at any level
>within the whole range and can be varied at any rate up to the
>maximum, it follows that unless the perception is being
>controlled at maximum magnitude and the reference signal is
>changing at the maximum rate that still permits control, the
>actual information flow must be much less than the channel
>capacity. Most perceptions are not controlled at their extremes;
>hence most perceptions must use less than the whole channel
>capacity.

The last sentence is a non-sequitur. What follows the semicolon has no relation to what precedes it.

> My theory of perception agrees with a largely noise-free
>experienced world; yours appears to predict a world in which
>perception barely stands out over the background noise. If your
>model were correct, precise control would be impossible.

You place great store on the conscious impression of precise perception. This impressions really has nothing to say about whether evolution has worked well or not. Conscious impressions can be, and probably are built from many noisy samples which are used as rapidly as possible in the actual perceptual processes that are involved in control. Furthermore, if most of the control is done in the central part of the range, most of the channel capacity would be expected, in an efficient syste, to be devoted to accurate perception within that region.

>Show me where Shannon's theory says there must be a comparator, a
>reference signal, and a perceptual signal.

Show me where Euclid's axioms say that the sum of the squares on the two sides of a right-angled triangle equals the square on the hypotenuse.

I am not sure that the discrete individualized ECS is predicted by Shannon. What I did point out is that Shannon's theorems demonstrate that S-R and plan-then-execute will not work in a chaotic world, whereas perceptual control will work. The thesis is that if a structure is to be stable in the world, perceptual control is nessary, though it may not always be sufficient. I cannot prove the necessity, because it may depend on hidden assumptions (like the relatively high sensory bandwidth) that I have not seen. But the only other way I can see to make a stable structure is to have one in which the binding energies are high compared to the thermal regime in which the structure finds itself.

>I think that information theory is by its very nature a post-hoc
>description, not a model. You can't start with information theory
>and come up with a system design. Or so sez I.

There's usually an interplay between abstract principles and practical prototyping. If you understand the Carnot cycle, you know that superheated steam engines can be more efficient than ones operating at lower temperatures. James Watt didn't know that, but he came up with a principle for making steam engines. Could Sadi Carnot have built Watt's steam engine from first principles? I doubt it, and quite probably he wouldn't have

invented his cycle either, if Watt's engine hadn't been there. Yes, the Carnot cycle is a description, not a model. But it's useful.

Maybe this is an appropriate place to enter a reminder that we have a difference of opinion about there being a qualitative distinction between a description and a model. I deny it, whereas you think it important. I think Occam's razor is important, and that the difference between what you call a "description" and a "model" is that your "model" is a more precise description over a wider range than is your "description." Occam's razor thereby gives more credence to your "model" than to your "description." It's simply a question of what is nowadays called Kolmogorov complexity.

>One last observation:
>
>>The way around this is that category boundaries are not
>>thresholds, but fold catastrophes.
>
>That's a pretty fancy term for a Schmidt trigger. Anyway, saying
>that categories are fold catastrophes says nothing that my
>description of hysteresis didn't say. Categorizing categorizing
>doesn't tell us how it works. It doesn't work the way it does
>because it's a fold catastrophe. It's a fold catastrophe because
>of the way it works, which remains undisclosed.

Actually, I thought it did point out a necessary condition for it to work--positive feedback. You can't have category perception without some form of positive feedback, whether it occurs by cross-linking and mutual inhibition among perceptual functions at a given level, through some kind of modelling/imagination loop, or through temporal recurrence.

A Schmidt trigger provides a very specific kind of fold catastrophe, which loses all information other than the category. There's no need to lose that information, and as the fold approaches the cusp of the three-dimensional version (stress being the third variable), the "adjectival" information begins to dominate. What your description didn't say was that the categorical aspect is of variable importance, and that the degree of overrun is affected by the amount of stress. The cusp catastrophe, of which the fold is a cross-section, does say that.

And yes, it is "only" a description, with a mechanism.

Martin

Date: Mon Dec 21, 1992 11:38 am PST
Subject: RE: neurosci, bye

Mark,
   By all means, let's keep up the discussion! Not so I can "prove you are wrong," but because it is on a topic not often aired in neuroscience. When I posted that I was happy you had raised the topic, it was for just that reason.

Enjoy the holidays. See you in January. Tom Bourbon

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

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

Make a simple offer ...!

****

   Portions of Tom's original message to Martin:
May I repeat and elaborate on an invitation I made about a year ago? I will send you one of my programs in which two systems (two people, the two hands of one person, two models, or a person and a model) interact and produce controlled relationships. ...

I invite you to add to the PCT model any features of information theory that you believe must be there. If necessary, use features from information theory to replace those from PCT.

If your changes improve the predictions by the model, there will be
no argument and no complaint: You will have demonstrated that a
person who does not understand Shannon or information theory does
not understand PCT.

****

   Martin to Tom, after lengthy discussion:
Have I done that to your satisfaction?

****

   Tom to Martin (in the present);
No.

****

   More from earlier sections of Martin's reply to Tom:
I guess I'd better try to describe, as I did a year or so ago, wherein information theory helps in the understanding of PCT. I didn't succeed in getting across then, and I'm not sure I'll do any better now. I should think that the prediction for your proposed system would be no better and no worse than you would get without it, because you are dealing with a transparent system of one control level. The understanding you get with information theory is not at the level of setting the parameters.

If I were to try to develop a model to make predictions in your experiment, I expect it would look essentially identical to yours, because the key elements would be the gain and delays in the two interacting loops.

****

   Tom to Martin (in the present): All you need do in the case of coordination between two systems is show me how, working back from Shannon's principles, you end up with two interacting PCT systems, each with the features Bill postulated for a single system. If the models that emerge from Shannon's principles are identical to those presently envisioned in PCT, your point is made. But I expect to sit down in front of a PC, grasp a control device, and interact with a Shannon-system in real time, with results at least as good as those when I coordinate with a PCT model. And when, without warning to my virtual partner, I alter my intentions in mid-run, I expect the Shannon-system to do the things another person or a PCT model would do. If the situation for modeling coordination is all that transparent, the task of working from first principles and creating the Shannon- controller should not be terribly difficult.

As for the single control level, that is merely the form in which I have published on interacting systems: In several other programs my interacting systems are hierarchical (two levels). In ARM, Bill and Greg have programmed a much more elaborate coordinated system, with a hierarchy of PCT loops and with loops in parallel.

****

Martin continuing his reply to Tom:
An electricity meter reader does not need to understand the principles of electromagnetism to get an accurate meter reading. This challenge is misdirected. If there are places where I think the prediction would be improved, they are likely to be structural, such as in the division of attention, monitoring behaviour or some such. What should be improved, in general, is understanding, not meter reading.

I said that if you don't understand Shannon, you won't understand PCT. I didn't say you won't be able to use PCT to make predictions. ==========

****

Tom to Martin (in the present):
Let me be sure I follow you correctly: Not only do PCT modelers not understand PCT (your original claim); now modelers are akin to meter readers and have no need to understand PCT.

****

More from Martin's reply to Tom (after a long discussion):
It is that kind of thing that I refer to as "understanding" PCT, not the making of predictions for simple linear phenomena. Linear models are fine when you have found the right ECS connections and have plugged in model parameters. I am talking about seeing why those models are as they are.

****

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) 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.)

****

Martin replying to Bill Powers' reply to Martin's reply to Tom:
(TB: What tangled webs we weave ...!
[Martin Taylor 921218 19:45]
Bill to Tom Bourbon (921218.1438 CST) --

****

Bill quoting Tom:

In the past, both of us wondered how, specifically, Shannon's ideas, or any of the major concepts from information theory, would improve any of the quantitative predictions we make with our simple PCT models.

****

Bill replying to Tom:>

This is the right question about information theory -- not "does it apply?" but "what does it add?" The basic problem I see is that information theory would apply equally well to an S-R model or a plan-then-execute cognitive model -- there's nothing unique about control theory as a place to apply it. Information theory says nothing about closed loops or their properties OTHER THAN what it has to say about information-carrying capacity of the various signal paths.

****

Martin replying to Bill:

You are right, but that "OTHER THAN" is a pretty big place to hide very important stuff. I had not previously realized that you wanted me to use Shannon to differentiate between S-R and Plan- then-execute. I think I did incidentally make that discrimination in my posting in response to the same posting by Tom. At least I think I showed how applying Shannon demonstrated that neither S-R nor Plan-then-execute could be viable. But we knew that already, so I didn't play it up.

****

Tom to Martin (in the present):

Perhaps Bill and I both missed it, but I did not see where you used Shannon to demonstrate that neither S-R or Plan-then-execute models would be viable. Your remark leads me to think that you also used Shannon to demonstrate that a PCT model would be viable. Was that the case?

****

More of Bill replying to Martin's reply to Tom:

In all these cases, something has to be added to get a workable system. And I don't think that this something comes from the abstract principles involved, however convincingly one can prove that they apply.

****

Martin replying to Bill:

Yes, that's absolutely right. Natural laws are no use without boundary conditions to describe particular situations. But if you understand the abstract principles, you can make better [bridges/kettles/radios/control systems].

****

Tom to Martin (in the present):

Your concluding remark brings us back to where we started. That is what I asked you to do in my original post: Use Shannon's natural laws to build a better PCT model of coordination among independent systems acting in an environment where conditions change in ways the systems cannot predict. Do that and let me interact with one of the systems in real time, then I might change my mind.

Until later, Tom Bourbon

Date: Mon Dec 21, 1992 12:12 pm PST

Subject: One whole second

From Greg Williams (921221)

Mainly to Tom B. and Bill P., but anyone can join in.

What would a PCT model look like for a step tracking task? Would it be the same as the model used so successfully for continuous tracking? Would its parameters change with the amplitude of the step and/or the speed of the step's rise (actually a ramp, in this case)? Maybe Tom could come up with an answer after a few quick experiments. I'm interested in the possibility of accurately determining the nonlinearities (if there) in the human control task -- which the Air Force tracking people have identified. Maybe step (ramp) tracking studies would be useful in this regard. One would be looking for accurate prediction over only a second or so (but with varying ramp rise times and amplitudes and directions (up or down)), especially with regard to delay of movement onset, oscillations, overshoot -- should be easy for somebody who can predict for One whole minute, right?

As ever,   Greg

Date: Mon Dec 21, 1992 12:55 pm PST
Subject: BBS on Movement Control

[from Gary Cziko 921221.1950 GMT]

The December issue of _Brain and Behavioral Sciences_ just arrived with the title "Controversies in Neuroscience I: Movement Control." Articles by Bizzi et al; Gandevia & Burke; McCrea; Robinso;, Alexander et al., Bloedel; Fet; and Stein plus lots of peer commentary.

Can't find a single mention of PCT (or anything like it, except in the Robinson article which is more interesting for other reasons--his claim that neuroscientists are wasting their time looking at individual neurons) and not a single reference to Powers.

Bill, I would be happy to send you my copy if I eventually get it back and if you are willing to share your thoughts about it with us on CSGnet.

Gary

Date: Mon Dec 21, 1992 2:11 pm PST
Subject: auditory configurations

[From: Bruce Nevin (Mon 921221 15:54:55)]

Bill,

Apologies for being such a sketchy presence. Hopefully, things will lighten up.

Harris (_A Theory of Language and Information_) suggests that both in language acquisition and in the evolution of language a relatively small stock of words comes first, then later the system of shared, socially instituted contrasts, which in turn support acquisition and control of a larger and more complex vocabulary and (in the case

of acquisition) the distinguishing of morpheme boundaries in more complex heard utterances (by the stochastic dependence that we have discussed).

The initial vocabulary is probably mostly monosyllables. It strikes me that a syllable could be an auditory configuration perception. The tricky issue is that there must be duration for auditory perception. From an analytic point of view adopted *after* acquisition of the contrasts, the duration of a syllable means sequence or the seeming sequence of the event level. But if duration (without analysis into sequential components) is necessary for auditory configuration perception, then no event level of perception is required to account for it.

Perhaps the same argument could be extended to bisyllabic or even trisyllabic morphemes (and a fortiori monomorphemic words).

How well does it transfer to other sensory modalities? Is some duration implicit in other kinds of configuration perceptions?

The issue of configurations of configurations, which still I have not properly digested, provides a related angle on all of this.

Sorry to be so brief. Got to sprint for the train now.

Be well, Bruce bn@bbn.com

Date: Mon Dec 21, 1992 2:14 pm PST
Subject: Re: Martin to Rick on Shannon

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

> Tom to Martin (in the present:
>All you need do in the case of coordination between two systems is
>show me how, working back from Shannon's principles, you end up
>with two interacting PCT systems, each with the features Bill
>postulated for a single system. If the models that emerge from
>Shannon's principles are identical to those presently envisioned in
>PCT, your point is made.

Is that what you want? I had misinterpreted you to mean that you wanted numerical predictions that came from a different source but were as good as your predictions. I said that was an inappropriate challenge because your model would be at least as good, coming as it does from a presumably correct structure, as would a model of the same structure derived from an information-theoretic background.

The challenge you now pose is worth trying. And it might help shed some light on the arguments that were going on a month or two ago between Bill and Greg about social control.

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> Martin replying to Bill Powers' reply to Martin's reply to Tom:
>(TB: What tangled webs we weave ...!

It happens when we get a discussion of more than two people. But no-one here is trying to deceive, as far as I can see. If they are, it's pretty well done... I much prefer multi-party discussions to discussions by X + Bill P.

==========
>   Tom to Martin (in the present):
>Perhaps Bill and I both missed it, but I did not see where you used
>Shannon to demonstrate that neither S-R or Plan-then-execute models
>would be viable. Your remark leads me to think that you also used
>Shannon to demonstrate that a PCT model would be viable. Was that
>the case?

Yes. And in trying your challenge, I suspect I will have to go over
that same ground again, so maybe I will be able to make it intelligible.

What I think I will have to do is to write what amounts to a serious paper rather than a set of impromptu come-backs on-line. I have not seriously considered the two-person version in light of information theory hitherto, so it will take a little thought to get it right. I was dealing only with why PCT was both necessary and (usually) sufficient for a living organism. ("Usually", because accidents can kill.) Now we must deal with another controller. I don't know whether this will change the analysis or not.

>I said that if you don't understand Shannon, you won't understand
>PCT. I didn't say you won't be able to use PCT to make
>predictions. ==========
>
>**********
>   Tom to Martin (in the present):
>Let me be sure I follow you correctly: Not only do PCT modelers
>not understand PCT (your original claim); now modelers are akin to
>meter readers and have no need to understand PCT.

I should know better than to tease people with serious agendas (can one pluralize a plural knoun?).

But yes, when the meter has been designed and built, the meter reader doesn't need to know how Maxwell's equations work. When Bill has designed the PCT structure, it doesn't take genius to fill in the parameters without understanding the beauty and power of the theory.

I retract my original statement, which was mainly intended to provoke Rick (he provokes me often enough that I felt entitled). There are many kinds of understanding, and one approach will suit one person while being quite obscure to another person. For me, Shannon theory explains unambiguously why PCT works and why higher levels work more slowly on average than lower levels, etc., etc. It provides a rationale based on fundamental facts of nature, rather than in the finding that PCT works, or that we can see how evolution could have produced control hierarchies.

I like (Occam's razor) things that are consequences of other things we know about nature better than things that stand off on their own, needing new basic foundations. It is in this sense that I say that one needs Shannon if one is to understand PCT. But there may be other ways of tying PCT to fundamental natural laws. If so, I would say that one could not understand PCT without understanding those ties, either.

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>   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.

Mea Culpa. As I replied today to Bill, my problem was more of sloppy wording than of poor understanding about nonlinearity. You challenged me with a linear problem, so I was thinking in those terms. As you can tell from reading that and other postings, I know very well that nonlinearity is essential in the input (at least of a multilevel control system) and not very relevant in the output.

>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?

I can guess. It is because every formal didactic presentation uses linear equations to describe the behaviour of an ECS. The methods used to solve the equations and demonstrate control would not work in a non-linear system. It is a natural step, for someone who is introduced to PCT through the equations, to believe that PCT works only in linear systems. The ideas you get first are the hardest to dispel, but demos and the like can help get rid of this one.

I'll think about your challenge. Martin

Date: Mon Dec 21, 1992 2:48 pm PST

Dr. Arkin,

Regarding intelligent buildings, I think there was a book published by MIT Press in the early-mid 1970's called The Adaptive House, which discussed user-modifiable structures and (if my memory is correct) self-adapting structures and materials. I think there are still quite a few visionaries left in the Architecture and Urban Studies departments at MIT, so you might try to get in touch with them.

There are also a small number of people concerned with Intelligent Materials, which are essentially adaptive materials that change their properties in response to changing environments (like the bones of most vertebrates). I don't know if this group includes architects and builders; unfortunately I don't have a good reference or contact for them.

Peter Cariani

Date: Mon Dec 21, 1992 3:15 pm PST
Subject: Re: Rick on (off) Shannon

[From Rick Marken (921221 1300) Martin Taylor (921216 17:40) --

>The argument was roughly (as seen from my side): if there is an intrinsic
>variable (i.e. a variable outside the main hierarchy, in a Bill-P-type
>reorganization hierarchy) with a reference level R and perceived level P,
>giving error E, then I assert that the controlled variable is P. Rick
>asserts that it is d(E^^2)/dt.

If this were the argument then there would have been no argument. Remember, I'm the guy who posted that little proof that the ERROR VARIABLE IN A CONTROL LOOP is not controlled. Why the hell would I have argued that error IS controlled? Unfortunately, I have saved none of our private posts but I remember the argument quite differently -- I thought that you were saying that intrinsic error (or some transform thereof) was controlled in reorganization. Now you say it the perception of the intrinsic variable that is controlled -- which (of course) is correct and I agree.

If you have saved the private posts, Martin, then you could post them and clear this all up. But even without them it is clear from my post to the net that I NEVER suggested that 'd(E^^2)' was controlled -- where E (in your comment above) is the error in the system controlling the intrinsic variable, P. When I said that d(e^2) is controlled, I made it quite clear that this was a perceptual variable -- representing perception of error in OTHER control systems. And I never made a big deal about what the form of the function was -- it's a perceptual function and determining it is best left to research and modelling.

>Now what might be an intrinsic variable. I used blood CO2 concentration,
>but I'm quite happy with overall error in the main hierarchy. But if
>overall error is to be used, Rick's formula won't work.

Well, I won't concede that until I see it modelleed. But I never made an issue of what the damn function is that transforms overall error into a perceptual measure of error -- I was just saying that 'error in the main hierarchy' can be a controlled intrinsic variable. Apparently you are happy with that; if you had written the above paragraph N posts ago we could have saved all this useless blather.

>Rick was talking
>about error in the hierarchy AS an intrinsic variable, but using an
>expression that couldn't work, whereas when I used the word "error" I
>was referring to error IN an intrinsic variable (which might well be
>overall error magnitude in the main hierarchy).

Well, then why did you just now (see above) say 'Rick asserts that it is d(E^^2)/dt.' that is controlled; where E is error in the control system controlling the intrinsic variable (according to your notation)' If you don't want to have confusion, don't be confusing.

I find this little misunderstanding (if that's what it was) particularly annoying because it led Bill Powers to write:

>I think I have to side with Martin Taylor on this one, Rick. In a
>simple reorganizing model, e^2 might be a suitable driving signal
>for the rate of reorganization. But that isn't the controlled
>variable. It's the error signal.

Again -- I'M THE ONE WHO WROTE THE DAMNED PROOF that error is not controlled; of course e^2 is not controlled -- unless it is PERCEIVED by another control system which can act in ways that have systematic effects on that perception. Why in the hell would anyone imagine that I was arguing that an error signal in a control loop is controlled??? How could you imagine that this was my argument, Martin????

Martin Taylor (921217 10:40)

>I really can't let Rick's latest non-sequitur go unchallenged:

>Every piece of the loop is constrained by Shannon's observations/theorems.
>If (like Rick) you don't understand them, you are doomed not to understand
>PCT.

Well, you got me there Martin (I think I understand Shannon's observations/theorems ok, though not nearly as well as you). But since you understand PCT so well (thanks to your understanding of information theory) why haven't I seen any of your PCT research or modelling work in the last decade or so, while I've been working on PCT? About the only work I ever found was done by Bill Powers. I did find some stuff by Carver/Scheier types who didn't know the difference between a controlled variable and a reference signal.I also saw lots of tracking studies by people who's goal was to figure out how target inputs cause response outputs. But I never found any research (other than Bill's) that was based on a deep understanding (like yours) of PCT. In the last few years I've seen some great stuff done by people like Tom Bourbon (and his students), Clark McPhail, and Chuck Tucker and a couple others. But I never ran across anything of yours. Since you REALLY understand PCT I bet your stuff is great. I'd really like to see it; especially the stuff that shows how important information theory is for doing PCT research and modelling.

Martin, I count a statement like this:

>If (like Rick) you don't understand them, you are doomed not to
>understand PCT.

as a personal insult. It is a disturbance to the level at which I like to maintain the perception of my own self esteem. I'm sure you will say that it was not meant as an personal attack -- and I'm prepared to believe that; but I don't really care -- a disturbance is a disturbance, regarless of the intent of the source; so here is a little compensating action (in lieu of going up a level):

If (like Martin) you don't undertand PCT, DON'T TRY TO TEACH IT. Those of us who do understand it -- and have done the grunt work necessary to grasp the fundementals -- find condescending tutorials on PCT to be cloying.

Best regards

Rick (what else would you expect from a loose canon?) Marken

Date: Mon Dec 21, 1992 3:41 pm PST
Subject: Re: Rick on (off) Shannon

[Martin Taylor 921221 18:00]   (Rick Marken 921221 1300)

Rick,
Welcome back from your trip. I'm sorry it didn't go well, and left you in a bad mood, when you saw my little twitting of you. I didn't intend you to take it as an insult, any more than you presumably intended your comment to me a couple of weeks ago as an insult, when I took it badly. Sorry you took it that way.

I think I posted a reasonable summary of the interactions, but you seem to think I misled the other readership. I had asked you to post everything, to avoid that kind of problem. Now it seems that to preserve honour (a rather silly concept, but we tend to have

reference levels for that kind of thing), I have to waste net bandwidth by reposting the discussion entire.

Either I still misread what you posted, or you misread what I said. It still seems to me that you were arguing for precisely what I claimed in my summary, which you now claim is ridiculous.

>If (like Martin) you don't undertand PCT, DON'T TRY TO TEACH IT. Those
>of us who do understand it -- and have done the grunt work necessary to
>grasp the fundementals -- find condescending tutorials on PCT to be cloying.

I have a feeling it is my various attempts to look for the fundamentals that you (Rick) find cloying (should I guess at threatening?). I know very well that I don't understand PCT. I don't think anyone on the net claims to except you, least of all Bill P. I'm sorry that you don't want to look beyond the behaviour of control hierarchies to see how and why they work in addition to what they do. I hope that "those of us who do understand it" is not a large group who find my postings "cloying."

One of the best ways of learning is to try to teach. But I don't think that I have ever been trying to do that (except locally here at DCIEM). What I have tried to do is to develop the implications of PCT, to enquire as to problems that seem to arise, to get at the basics of PCT. If you don't like it, I'll quit, and just work with our group here at DCIEM. There's no benefit to me in finding an automatic rejection of my ideas when they are proposed to the net. It is really Bill P's encouragement more than anything else that keeps me going--that, and my belief that PCT is the Newtonian revolution in psychology. But the latter part is something I can develop on my own, or in private communication with Bill P. and my local collaborators.

Here are the postings, so far as I still have them.

Martin

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(I don't seem to have the first mail, to which this is Rick's response)
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From Marken@courier4.aero.org Thu Dec 10 14:30:23 1992
Subject: Re: Rick's proof: error not controlled

Martin

You seem to have sent this to me personally -- so I am sending it back to you personal (I think; it's often hard for me to tell where mail came from when I'm using this mail system). But I'd be happy to have the put on the net -- as usual.

>I have no complaint about your proof. Different people see things better
>from different viewpoints. I thought I was buttressing, not caviling.

OK. What made me think that you disagreed with the proof was the following:

>>Where in the world (take the literal meaning of that expression) is the
>>possibility for disturbing the error in an ECS?

Now you say:

>A disturbed error signal is not an error signal as defined.

If you cannot disturb error (and again I point to Bill's diagram to show that you can) then my proof is invalid because it is based on the idea that o = k(r-p+de) -- were de is the disturbance to error.

As I mentioned in my post with the equations, if the perceptual signal were disturbed in the same way (by having a disturbance added to the neural signal itself) the perceptual signal would still be controlled (made to equal the reference -- ie. the disturbance would have no effect. So again, while I agree with your conclusions, I disagree with the arguments you use to come to them.

>I suppose I would agree that error could be controlled if it were to be
>provided as input to the perceptual input function of a control system that
>maintained the error at some reference level.

As you will see in a reply I plan to make to Greg, there is no supposing necessary in this case. The error signal (which is now a perceptual input to a control system) would DEFINITELY be controlled.

> But what on earth would that
>do other than enforce a situation in the ECS whose error was being controlled
>that would ordinarily arise only through unresolvable conflict. I can't
>see why any hierarchy would include such a mechanism--but I suppose there
>might be a reason somewhere.

But this is precisely the way reorganization works. It controls errors by changing properties of the control system itslef (like its perceptual function) -- properties which are presumably responsible for the conflict that is creating the chronic error. Reorganization is control of error (where error is the input perceptual variable to the reorganization control system) -- and control is effected by acting on properties of the control systems themselves. Since there is no way to know HOW to change the control systems in order to reduce error, there must be a random componenet to this kind of control -- Bill has modelled as a control system where the RATE at which random changes occur is inversely proportional to the perceived value of error.

Best

Rick
==========
From mmt Thu Dec 10 16:45:11 1992
To: Marken@courier4.aero.org
Rick,

As usual, we were talking a little at cross purposes. There's no real argument. You were assuming that there might be some way that a disturbance could be applied to the error within an ECS, and when you mention electrodes, that can be true. I was working within the normal hierarchy, in which the only inputs to an ECS are through its perceptual input function and its reference inputs, and within that frame, there's no way to apply a "de". That doesn't argue against your proof at all. If(x) then (y) is the proof. Not(x) does not say not(y).

On a different topic, you say:

>> But what on earth would that
>> do other than enforce a situation in the ECS whose error was being
>>controlled
>>that would ordinarily arise only through unresolvable conflict. I can't
>>see why any hierarchy would include such a mechanism--but I suppose there
>>might be a reason somewhere.
>
>But this is precisely the way reorganization works. It controls errors by
>changing properties of the control system itslef (like its perceptual
>function) -- properties which are presumably responsible for the conflict
>that is creating the chronic error.

Reorganization may well work this way. (All we know experimentally is that Bill says that my approach to reorganization is the only one he knows to work by simulation, and then only in respect to gain functions; even if we eventually discover various ways in which it COULD work, we won't know that it DOES work this way). But the thing I was saying was that however reorganization works, it seems unlikely to develop stable systems that do useless or obstructionist things.

> Reorganization is control of error
>(where error is the input perceptual variable to the reorganization control
>system) -- and control is effected by acting on properties of the control
>systems themselves. Since there is no way to know HOW to change the
>control systems in order to reduce error, there must be a random
>componenet to this kind of control -- Bill has modelled as a control
>system where the RATE at which random changes occur is inversely
>proportional to the perceived value of error.

Actually, what Bill has done seems to be a little more complex. He has found that the derivative of the squared error is a better criterion. In our experiments, we have been planning to use K1(e^^2)+K2(e*de/dt), where de/dt is the derivative of error (2e*de/dt is the derivative of e^^2). We think that both error and the rate of increase of error are important. It shouldn't matter if the error is momentarily large, provided it is decreasing well. When I say "criterion" I mean the Poisson rate of reorganization events.

Does this help to reduce the cross of our purposes?

Martin

==========

From Marken@courier4.aero.org Tue Dec 15 18:54:38 1992
Subject: Re: Rick's proof: error not controlled

Martin

>Actually, what Bill has done seems to be a little more complex. He has
>found that the derivative of the squared error is a better criterion.

You mean, as the variable controlled by the reorganization system, right?

>In our experiments, we have been planning to use K1(e^^2)+K2(e*de/dt),
>where de/dt is the derivative of error (2e*de/dt is the derivative of
>e^^2). We think that both error and the rate of increase of error are
>important. It shouldn't matter if the error is momentarily large, provided
>it is decreasing well. When I say "criterion" I mean the Poisson rate of

>reorganization events.

Now I don't understand. The "Poisson rate.." is a criterion? It sounds like a variable. Why would your model be concerned about the "Poisson rate of reorganization events"? It's not controlling that rate, is it? Bill's model controls the derivative of the squared error(you are correct about this, I believe); it controls this variable by changing some parameter of the systems that are experiencing the error; the time between such changes increases as the difference between the derivative of the squared error and zero decreases. As a side effect the Poisson rate of reorganization events changes during the course of reorganization.

At least, that was my impression this summer in Durango.

Regards

Rick

==========

From mmt Tue Dec 15 20:34:04 1992
To: Marken@courier4.aero.org
Subject: Re: Rick's proof: error not controlled

Rick, It's words again getting in the way...

Bill's model doesn't control the derivative of the squared error, in any normal sense. That derivative has more the function of "error" in a standard ECS, in that it determines the output of the reorganizing function. That output is a reorganizing event that occurs from time to time in the main hierarchy. The higher the value of the derivative of the squared error, the more likely a reorganization even is to occur. I don't know whether in Bill's simulations the rate was determinate or Poisson. Ours is Poisson, which means that the probability of a reorganization event happenning in a small delta-T is proportional to the value of the criterion (derivative of squared error in Bill's case, unless he was using a determinate time-interval between events).

Now what is controlling what? I think what is being controlled is the value of some intrinsic variable. It has some error, and all this stuff about derivatives and Poisson rates are attributes of the output function of the control system for that intrinsic variable. As with any control system, whether in the main hierarchy or in the reorganizing system, the actual outputs are not controlled in themselves. Outputs are, shall we say, blind. So the "criterion" is not controlling the Poisson rate. It is just a component of the output function of a controller that controls an intrinsic variable.

(Bill and I have an unresolved, and perhaps unresolvable, disagreement here as to what might constitute an intrinsic variable, but that disagreement is minor and not germane to this discussion.)

Martin

PS. It feels great to be back on the air. I hope the link stays in action for a while.

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From Marken@courier4.aero.org Wed Dec 16 11:59:39 1992
Subject: Re: Rick's proof: error not controlled

Martin Let's put this on the Net.

>Bill's model doesn't control the derivative of the squared error, in any

>normal sense.

It does. I'll explain on the net.

>That derivative has more the function of "error" in a standard ECS

Because the intrinsic reference (for de^2) is 0.

>Now what is controlling what? I think what is being controlled is the value
>of some intrinsic variable.

de^2 is the intrinsic variable being controlled relative to an intrinsic reference which happens to be 0; it could be something else (maybe evolution wants the system to operate with de^2 at just a tad above zero -- keeping you on your toes).

de^2 is controlled because it is both a cause and a result of reorganization; and the sense of the feedback is (or should be) negative.

Regards

Rick

==========

From mmt Wed Dec 16 14:10:23 1992
To: Marken@courier4.aero.org
Subject: Re: Rick's proof: error not controlled
Cc: ./marken
Status: RO

Rick,
I don't mind putting the discussion on the net, but I don't think it worthwhile. There are too many long wavelength fish involved.

>Because the intrinsic reference (for de^2) is 0.
>
>>Now what is controlling what? I think what is being controlled is the value
>>of some intrinsic variable.
>
>de^2 is the intrinsic variable being controlled relative to an intrinsic
>reference which happens to be 0;

This makes no sense to me. In words, you are saying that whatever the error is in the intrinsic variable (say blood CO2 level, for example), what is controlled is that this error should not change. To me, the only thing that makes sense is that there is some reference level for blood CO2, and if this is too high or too low, and particularly if it is moving in the wrong direction, then the main hierarchy needs reorganizing. The reason for using the derivative of the squared error in the intrinsic variable as part of the output function is that it is a short way of ensuring that the error is both large and increasing, whichever sign it has. Other functions having the same properties would also be useful. But e*de/dt is not a controlled variable, any more than (in fact less than) is the error in a normal ECS.

Another way of seeing that d(e^^2)/dt is not controlled is to note that zero is not a reference level for it. Negative values are even better, becasue they show that the intrinsic variable is really being controlled. A zero value is neutral in this respect, in that the error in the intrinsic variable may be large but unvarying, or the error may

be small while changing wildly. Neither indicates good control, but both are compatible with good control (momentarily).

If you want to put it on the net, collect all the postings and send them out as one. But I don't think it worthwhile. I haven't seen anyone else that seems to be interested in the matter (except presumably Bill, by inference)

Martin

Date: Mon Dec 21, 1992 4:54 pm PST
Subject: Info theory; nonlinear models

[From Bill Powers (921221.1500)]   Martin Taylor (921221.1200)

Martin, our discussion of information theory and PCT seems to be flying apart into very strange pieces. I don't follow your reasoning about information flow or channel capacity in a control system at all. If you want me to understand, you're going to have to do a lot more specific spelling-out of what you mean.

In my last post and your answer the following exchange occurred:

You:
>>The central theme of PCT is that a perception in an ECS should
>>be maintained as close as possible to a reference value. In
>>other words, the information provided by the perception, given
>>knowledge of the reference, should be as low as possible.

Me:
>I think you'd better take [that one] back to the drawing board.
>The reference in no way predicts the perception by its mere
>existence.

You seem to be taking the position of an external observer who has one probe on the reference signal and another on the perceptual signal. Knowing that a good control system is acting, the observer knows that the perceptual signal will track the reference signal closely, and so is predicted by the reference signal. I understand this to imply that the perceptual signal adds little information to what this external observer is already getting from the reference signal. The same could be said the other way around: observing the perceptual signal, the observer knows essentially what the reference signal is doing, and so the reference signal adds little information to what the perceptual signal is already supplying.

But the receiver of the information in either case is external to the behaving system. What does that external receiver's information input have to do with the properties of the system being observed? Why should it make a difference in the behaving system if the external observer uses the reference signal to predict the perceptual signal, or the perceptual signal to predict the reference signal? Does the information being carried in a channel depend on what the external observer is paying attention to?

If the reference signal and the perceptual signal are both varying in a pattern that requires a bandwidth of, say, 2 Hz, doesn't this mean that both signals are carrying information at a rate corresponding to that bandwidth?

If you want me to understand this, you're going to have to take it slow and simple. I'm not following you. Today's post just made the whole thing more baffling to me.

Martin Taylor (various posts) and Tom Bourbon (921221.1015) --

Tom, I think that the meaning of your challenge isn't completely clear to Martin: that is, what you think of as a demonstration and what he thinks of as one are very different. What you (and I) want is a program, or at least the design of a simulation that we could program and run on a computer, which would generate behavior that can be compared with real behavior. What Martin seems to think of as a demonstration is showing that a specific behavior is an instance of a more general class of behavioral phenomena.

We have to be very careful here not to ignore Martin's complaint, that it is as hard to get PCTers to listen to information theory as it is to get conventional journals to listen to PCT. Perhaps in learning how to understand what Martin is trying to say, we can also learn something about why we have difficulties in getting mainstream psychologists to listen to us. I recommend patience here, and not leaping to conclusions.

Martin, the difference that Tom is talking about, I believe, is between a descriptive model and a generative model. A descriptive model provides a general picture of which a specific behavior is only one example. A generative model actually generates (simulated) behavior for direct point-by-point comparison with real behavior. So conceptually, the arrangement from most to least detail is

generative model ==> observed behavior ==> descriptive model

I think the different relationships of the two kinds of model to observed behavior is the source of much of our mutual difficulties. The generative model is a proposed system design; it connects components with physical properties (mathematically represented, but close to the component level) into a system that behaves as it must according to the design. If the system design is successful, it will behave like the real system: that is, its variables will change through time as the same variables do in the real system.

The descriptive model, on the other hand, is a generalization drawn from classes of behaviors. It attempts to extract general principles and laws from the details of behavior. It looks for truths about behavior that are more general than any specific behavior.

If these truths are true of observed behavior, they are also true of the behavior of a generative model that can mimic observed behavior. That is, for example, if Ashby's law of Requisite Variety can be shown to encompass certain control behaviors, then it will also encompass the behavior of a successful simulation of those behaviors.

I think our problems arise when we try to make one kind of model work in place of the other. The concept of information is a generalization, not an explanation. If we begin with the phenomenon of messages passing between behaving systems, we can show that those messages carry a certain amount of technically defined information, dependent on what the receiver wants from the message. But this tells us nothing about HOW those messages are generated and received. We can't use information theory to provide a system design, a generative model, because it is on the wrong end of the scale of abstraction. Neither can we use the generative model to provide an analysis of information flow; the generative model handles physical signals and quantities, and its specifications say nothing about information.

The PCT model is fundamentally a generative model. As such it is only partly successful. It will become more successful as we become able to simulate more and more complex behaviors, thus showing that the structure of the model is plausible. What we may guess about higher levels of organization is largely irrelevant now to the modeling process.

The applications of information theory that are relevant depend to some extent on the model that is assumed. Given an assumed model, its behavior or hypothesized behavior can be found consistent with information theory, and perhaps information theory will be able to explain why some designs work better than others. But information theory can only specify requirements -- for example, adequate bandwidth, or decreasing bandwidth at higher levels. It can't supply the system design at the generative-model level that will meet those requirements. It can't specify what information is needed in the generative model, or how signal paths should be arranged, or what functions should be applied to the signals.

In the end, the generative model will explain behavior, while
descriptive models show that the behavior thus explained and the
structure of the successful model are consistent with general
laws.

Greg Williams (921221) --

>What would a PCT model look like for a step tracking task?
>Would it be the same as the model used so successfully for
>continuous tracking? Would its parameters change with the
>amplitude of the step and/or the speed of the step's rise
>(actually a ramp, in this case)?

I'm working on an experimental setup (for David Goldstein) that will partly answer this question. By using a little control system, the program adjusts the difficulty of a task (by varying the speed with which a table of disturbance values is scanned) until a specific amount of RMS tracking error is produced by the participant. This amount of error is then maintained quite well in a subsequent one-minute tracking task. The purpose is to measure parameters of control at standard levels of tracking error, and also to monitor long-term changes in tracking skill. In experimenting on myself, I find that as the amount of mean tracking error increases from one run to another, the integration factor in a best-fit model decreases. This is a crude way of measuring nonlinearity in the overall system response. With large mean errors, the slope of the output curve flattens out, as we would expect on neurological grounds (signal saturation).

It may be possible to estimate this nonlinearity and build it into the output function of the model. Then step-disturbances could be tried to see if the model approximates real behavior better than the linear model does.

The chief difference between continuous and stepped disturbances, which I have looked at, is that the stepped disturbances entail a transport lag much longer than the lag used to fit continuous behavior (twice as long). This could be due to nonlinearity in the output function, or (more likely, I think) to higher-level control systems being involved. After a relatively long period of no disturbance, a sudden step disturbance seems to surprise the system, so it doesn't start tracking right away. If you begin even a continuous-disturbance run with a few seconds of zero disturbance, there is a longish lag, 250 milliseconds or so, before tracking actually starts after the first significant amosunt of disturbance appears. But once it has started, the lag drops to 100 milliseconds or so. So perhaps the 250 milliseconds includes the normal 100 milliseconds, plus another 150 for a higher-level system to turn the tracking system on.

>Should be easy for somebody who can predict for One whole minute, right?

Yeah. When are you going to do it?

Best to all, Bill P.

Date: Mon Dec 21, 1992 7:24 pm PST
Subject: Steps and such

From Greg Williams (921221 - 2) >Bill Powers (921221.1500)

>The PCT model is fundamentally a generative model. As such it is
>only partly successful. It will become more successful as we
>become able to simulate more and more complex behaviors, thus
>showing that the structure of the model is plausible.

This is the direction in which I was trying to point when I asked earlier today about modeling step tracking performance. Given good prediction of tracking with low error over most of the duration of a trial, I think the logical next step is to attempt prediction when error is higher over most of the duration of a trial. Also, because good control obscures system parameters, I think higher error situations will make it easier to distinguish among candidate models with regard to their predictive abilities, and to identify nonlinearities in system components.

>By using a little control
>system, the program adjusts the difficulty of a task (by varying
>the speed with which a table of disturbance values is scanned)
>until a specific amount of RMS tracking error is produced by the
>participant. This amount of error is then maintained quite well
>in a subsequent one-minute tracking task. The purpose is to
>measure parameters of control at standard levels of tracking
>error, and also to monitor long-term changes in tracking skill.

Sounds great -- in some ways better than steps/ramps, but I suspect figuring out which types of nonlinearities to try in the models might be easier with steps/ramps. Also, be sure to look at a broad range of disturbance bandwidths, to make sure the "good" models aren't biased for a notch which you are gradually moving up the spectrum. I'll be interested to see your results. What's the timetable?

>>Should be easy for somebody who can predict for One whole
>>minute, right?

>Yeah. When are you going to do it?

I don't need to, since you already are.

As ever, Greg

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 a 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

9212D

Command: Date: Tue Dec 22, 1992 12:45 am PST
Subject: Truth from error?

[from Ray Jackson (921222.0145)]

Gary Cziko, Bruce Nevin and others (various times)

>> Melius invenitur veritas ex errore quam ex ignorantia
>>"It is easier to arrive at truth from error than from ignorance."

> ....make a revision? I would much prefer something like "get closer
>to" or "draw nearer to" than "arrive at." Would something like
>"advenitur" do it? This reflects my Popperian sympathies.--Gary

What about the quote by Francis Bacon (in English): "Truth emerges more readily from error than from confusion."

Regards, Ray

Date: Tue Dec 22, 1992 1:55 am PST
Subject: Re: Info theory

[Martin Taylor 921221 20:00] (Bill Powers 921221.1500)

Bill, your posting just arrived, as I was on the way out to go home, so this must be short (our phone lines have got very bad recently, and I can no longer try to do it from home).

As usual, you are an acute observer, especially in relation to the question of model types. But I really do think that I can use information theory to identify that the PCT

structure was correct, at least feasible. When you put in the appropriate perceptual input functions, gains, and delays, you get the same model that you and/or Tom would produce without information theory, so it should make the same predictions in any specific case. So why should I try to do better, when I anticipate the result being identity?

What I do want to do is to get some deductions about the structure and its behaviour that are not obvious, even though they may (should) agree with what you have found to work in practice. I find that it makes much more sense to me to have a good theoretical underpinning that allows me to generalize from a practical result than just to see the practical result and wonder what might happen if some little thing were changed.

>Martin, the difference that Tom is talking about, I believe, is
>between a descriptive model and a generative model. A descriptive
>model provides a general picture of which a specific behavior is
>only one example. A generative model actually generates
>(simulated) behavior for direct point-by-point comparison with
>real behavior.

Yes, I understand. I have a bit of a problem with limiting myself to either kind of model exclusively, though, and it is a problem that has been with me since undergraduate days. If a generative model does predict reality well, without excessive use of parameters, then it produces strong evidence of the plausibility of the theory that underlies it. But if the generative model fails, it does not give evidence against the underlying theory, because the failure could have been only in the choice of parameters. So the generative model is a one-sided kind of support.

On the other hand, the theory by itself is only plausible unless it can be shown to predict reality, and that can be done only through generative models or mathematical analysis. In the case of your and Tom's models, the prediction is very good. So I see little point in trying to create generative models from what I see as a theoretical support for the same structure on which your models are based. It is conceivable that in some situations the information-theoretic approach might produce numerical statements of more precision or using fewer parameters, but those situations probably will not be easy to find. They will be at higher levels in the hierarchy, most probably. I'm not even going to look for them at present, at least not until I can see some problems with your practical approach that are resolved in the information-theoretic approach.

>In the end, the generative model will explain behavior, while
>descriptive models show that the behavior thus explained and the
>structure of the successful model are consistent with general laws.

Wise.

I obviously must either drop the information-theory thread or take it much further back to first principles. In response to what Tom seemed to want in his revised version of his challenge, I anticipated doing just that. But perhaps it would help if I contradicted one of your assumptions:

>You seem to be taking the position of an external observer who
>has one probe on the reference signal and another on the perceptual signal.

No. Throughout, I am trying to take the position that the only probabilities that can be observed are based within the observing entity. Sometimes I slip, I acknowledge. But that's a simple mistake, not a failure of principle. In this case, the reference signal

and the perceptual signal are both known within the ECS. If you remember a long way back, this came up. There is no need for an external evaluation of the probability distribution, any more than there is a need for an evaluation of a neural current that is based on a rate of neural impulses. I suppose it might be possible for an external observer with a probe to make the analyses, and sometimes it is didactically easier to posit such an observer. But in practice there isn't one, and it is not necessary to think of one.

One didactic problem is that WE are external to the ECS in question, so that WE externally observe (imagine) what is going on. But we have to try to imagine ourselves being in the ECS. It's not easy.

As you can see, we do not
>begin with the phenomenon of messages passing between behaving systems.

I've been playing with information at an intuitive level for as long as you've been playing with control systems. It's hard for me to get back to basics (or even to exact formulae, since I don't use them much), but it will be a good exercise for me to try.

>If the reference signal and the perceptual signal are both
>varying in a pattern that requires a bandwidth of, say, 2 Hz,
>doesn't this mean that both signals are carrying information at a
>rate corresponding to that bandwidth?

They could be, but they need not be. The problem is in thinking of information as being carried, as if an external observer could see it. That's the same problem as the "codingism" problem. The information arriving at a receiving point depends on the probabilities of the received pattern as believed by the receiver, which are unknown to any other party. The information the originator thinks is being sent is based on the originator's beliefs as to the probabilities held by the receiver. In the 2Hz case, the presumption is that all these probabilities are flat (actually Gaussian) distributions, maximizing the information that could be transmitted.

Anyway, I'll try to put together a discussion of this, but it may take some time. I'm sorry I annoyed Rick with my comment. Information theory clearly isn't as easy to understand as I thought, but in any case I had no right to say soemthing that could be perceived as insulting. I'm afraid I may have done it again, in a somewhat hasty reaction to Rick's riposte. Rick--if you are reading this--let's try to keep things more technical, if we can.

Date: Tue Dec 22, 1992 8:33 am PST
Subject: different kind of fog

[From: Bruce Nevin (Tue 921222 10:44:42)] (Rick, Marten) --

Gee, I wish you guys wouldn't huff and puff so much, it gets the windows all fogged up.

And I hope you will keep on including elementary restatements of what may seem obvious to you, at the risk even of seeming patronizing in a 1-1 conversation. First, when the private conversation is reposted for the rest of us (as I hope such conversations generally always shall be) what is obvious to you may be essential tutorial to less practiced folks like me. There are a lot of different levels of expertise present, in a lot of fields that intersect variously with PCT. Secondly, and I doubt this process is unique to me, I find that the effort to state clearly what seemed obvious often discloses

things that were not obvious. I think this has to do with participating in a revolution with respect to fundamentals.

It seems to me that taking offense generally causes as much trouble as giving it. Must be a social-PCT way of putting that.

I am extremely grateful for the forebearance and tact with which my efforts at participating here have been met. Others have commented on this quality, which is very rare indeed in the world of electronic mail fora. Bill attributes it to a change in perspective brought about in each of us by understanding the nature of control. The jury is obviously out on that. But whatever the reasons for it, I hope we continue always so.

Bruce bn@bbn.com

Date: Tue Dec 22, 1992 8:57 am PST
Subject: Misc replies

[From Bill Powers (921222.0800)] Rick Marken & Martin Taylor (various posts)

Your argument is based on a misplaced reference. When Rick said that the squared error was a controlled variable, he was referring to error signals in the hierarchy being sensed as intrinsic variables and being controlled by the reorganizing system. When I corrected Rick, I was influenced by the same misreading; on hasty reading it was possible to confuse the squared error in the hierarchy with the reorganizing system's own error signal. So all this has been just a verbal misunderstanding. It's made more confusing by the fact that in the reorganizing system, the rate of reorganization seems best based on a function of the absolute error and derivative of absolute error in the reorganizing system (sign of error doesn't matter when the output is a rate of application of a random process). Whether this absoluteness is achieved by an absolute value function or by squaring or by RMSing is a detail; I happened to use squaring as a way of getting absolute value, which totally confuses everything.

Greg Williams (921221.1500) --

>I suspect figuring out which types of nonlinearities to try in
>the models might be easier with steps/ramps. Also, be sure to
>look at a broad range of disturbance bandwidths, to make sure
>the "good" models aren't biased for a notch which you are
>gradually moving up the spectrum. I'll be interested to see
>your results.

The problem with using any kind of disturbance with predictable properties is that higher-level systems get into the act. It's been known for a long time among engineers and engineering psychologists that tracking unpredictable waveforms produces "deterministic" behavior, while tracking any predictable waveform like a sine wave or square wave or ramp produces "nondeterministic" behavior. Any regularity YOU can see in the situation, the person doing the tracking can also see. So you can't do the standard kinds of tests that are done with an artificial system that is capable of behaving at only one level.

As I interpret these terms, deterministic behavior involves a clear lag so that cause and effect can be separated in time. With deterministic behavior it's possible to derive a transfer function that fits the behavior quite well. Non-deterministic behavior, on the other hand, involves spontaneous generation of output in a way that can lead the

disturbances. When a sine-wave disturbance is used, the skilled participant adjusts a spontaneously-generated sine wave output so its amplitude, frequency, and phase match the disturbance (this works best for pursuit tracking, I presume, where there is a representation of the disturbance in the form of the target movements). The movements of the cursor can actually get ahead of the movements of the target, and there is no longer any simple transfer function that can express the relationship.

I would not use terms like deterministic and non-deterministic; I think that the difference is one of level of controlled variable. One of the portable demonstrations from the 1960 paper by Powers, Clark, and MacFarland shows this "non-deterministic" kind of behavior as a demo of control of sequence (as we called it then). The experimenter moves a finger up and down in a regular sine wave while the participant tracks it with a finger. The frequency of the sine wave is made rather fast, around 1 to 2 Hz. At some unexpected time, the experimenter abruptly stops moving the finger and holds it still. The participant's finger continues to move, going almost a foot past the experimenter's finger before any correction starts even if the halt is at a zero-velocity point like the peak of the sine wave. This also works for a continuous movement of the target finger in a circle, or in any other simple predictable pattern. The reaction time to the sudden break in the regular pattern is quite long, indicating to me that higher order control is involved.

Now, of course, we can set up an experiment with continuous control of such a pattern and measure the parameters. If PCT were being taught as an experimental science in universities, this would be a part of a nice experiment for a Master's thesis. Unfortunately, all the real experimental investigation of control behavior is being done by a small handful of people with no funding or assistance, who are either retired and decrepit or working full time at something else to make a living.

>What's the timetable?

The timetable is the same as always: when the queue gets emptied, or when someone else decides to take on the project.

The proper way to do this kind of work is to analyze the lowest levels of control first and get a model to fit them accurately, then introduce variations on the experiments that supposedly bring in higher-level variables, and model them USING THE MODEL FOR LOWER-LEVEL BEHAVIOR OF A PARTICULAR INDIVIDUAL AS PART OF THE OUTPUT FUNCTION IN A MODEL OF THAT INDIVIDUAL.

I've known for a very long time that this was the way to go, but it wasn't until 1974 that I had even rudimentary computing equipment to start doing this, and then it took me years to find a simple model that really worked (and for the speed of computers I could afford to increase enough to do the experiments right). I did everything the hard way first.

It's been less than two years since I ceased to have to earn a living by doing rather demanding things of no interest except to my employers. Remember that since 1960 I have produced major advances in low-light-level television astronomy including designing and building three generations of astronomical television cameras and a whole semi-automated observatory to do a supernova search, a control system for ruling the best diffraction gratings in the world, and numerous microcomputer projects for a newspaper including one that took stock market data off a satellite signal, organized it, typeset it, and sent it to the APS typesetter to produce camera-ready copy in time for the Bulldog edition (which went to press 15 minutes after the last table was transmitted). The time and mental

energy available for developing PCT in the laboratory has been very limited. Now I have time, but less mental energy (whatever that is, but you know it when you see it).

The timetable depends, therefore, on the person-hours available and the facilities for doing the research, including availability of human subjects. As long as the list of people actually devising and carrying out experiments and modeling is limited to Rick Marken, Tom Bourbon, and me, the queue of possible experiments with HPCT is going to grow while the actual work done trudges along at a slow pace.

When I last looked, there were 132 subscribers to this list. Permit me a moment of impatience: when are some of you people going to get out of your armchairs?

Gary Cziko (921222.0430) --

>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.

Literal prediction doesn't happen at the lower levels. When I referred to "predictable" waveforms in the reply to Greg, above, I didn't mean that the control systems themselves do any literal predicting. It's just that waveforms with enough regularity for a bystander to predict them can be perceived directly and controlled. A frequency detector can put out a signal representing the mean frequency of a sine wave without looking ahead to compute sine(now + tau).

Think of "prediction" as a proposal for a model. It is always possible to fit some mathematical form to a process, and compute its value for some time in the future. The question is whether we are to propose that the real system actually does it that way. The equivalent of prediction can often be carried out by a simple analogue process. For example, a sensor that strongly exaggerates changes in stimulation produces a first-derivative component of perceptual signal, which permits the action of the control system to slow down before the error is actually corrected fully. This can be interpreted as a prediction that if the present speed of change kept up, the target would be overshot. But no such cognitive process is really going on -- to carry it out that way, literally, would require tons of machinery to do what a single neuron easily does without any thought or "looking ahead" at all. You have to ask whether the term prediction is meant literally or metaphorically.

Maybe my remarks to Greg above will help.

Martin Taylor (921221.2000) --

>If a generative model does predict reality well, without
>excessive use of parameters, then it produces strong evidence
>of the plausibility of the theory that underlies it.

The control system model we use predicts detailed handle movements within 5 to 10 percent with a single adjustable parameter, the integration constant. By adding a transport lag, we can approximately halve that error. So at most we need two parameters to reduce prediction error to 2.5 to 5 percent. If we introduce nonlinearities, we might get that down to 2 to 4 percent. But diminishing returns will set in pretty quickly. I figure that we are pretty near the noise level set by the discrete nature of neural impulses.

>In this case, the reference signal and the perceptual signal
>are both known within the ECS. If you remember a long way
>back, this came up. There is no need for an external
>evaluation of the probability distribution, any more than there
>is a need for an evaluation of a neural current that is based
>on a rate of neural impulses.

I think you're going to have to take us back to a more elementary level. I still don't understand why a perceptual signal that is maintained in a match with a changing reference signal is said to have a LOW information content.

>The information arriving at a receiving point depends on the
>probabilities of the received pattern as believed by the
>receiver, which are unknown to any other party.

What does a simple comparator, which subtracts the perceptual signal's magnitude from that of the reference signal "believe" about either signal? What does "believe" mean in this context?

>The information the originator thinks is being sent is based on
>the originator's beliefs as to the probabilities held by the receiver.

What does a sensor "believe" about the probabilities held by a comparator?

>In the 2Hz case, the presumption is that all these
>probabilities are flat (actually Gaussian) distributions,
>maximizing the information that could be transmitted.

Who is doing this presuming in the system? I asked why the perceptual and reference signals should not both be considered to carry an information flow appropriate to a signal varying within a 2Hz bandwidth. You didn't answer that.

Best to all,   Bill P.

Date: Tue Dec 22, 1992 10:31 am PST
Subject: Re: Information theory

From: Tom Bourbon (921222 10:10 CST)

[Martin Taylor 921221 20:00]   (Bill Powers 921221.1500)

****
Martin:
Bill, your posting just arrived, as I was on the way out to go home, so this must be short (our phone lines have got very bad recently, and I can no longer try to do it from home).

As usual, you are an acute observe, especially in relation to the question of model types. But I really do think that I can use information theory to identify that the PCT structure was correct, at least feasible. When you put in the appropriate perceptual input functions, gains, and delays, you get the same model that you and/or Tom would produce without information theory, so it should make the same predictions in any

specific case. So why should I try to do better, when I anticipate the result being identity?

****
Me (now):
Martin, I think this is the heart of the misunderstanding that seems to emerge in a regular cycle on csg-l, concerning your ideas. You are convinced that you can work from first principles in information theory and (necessarily?) arrive at an architecture identical to that in the PCT model. Further, you say that your model would behave identically to a PCT model. I certainly would not question or challenge your convictions -- they are yours and you undoubtedly have good reasons to hold them. What I would like to see is a demonstration that things work the way you believe they do.

I am not saying you are wrong. I am not even offering a challenge, although that is the way my offer has been characterized on the net, just as it was when I first made it over a year ago. Then and now, my posts were motivated by concern that the discussion about information theory and PCT was becoming supercharged and an opportunity for clearer understanding on both sides might be slipping away. I thought of my posts more as requests, or offers, or attempts to encourage you to try a different style of presentation.

Obviously, on csg-l part of the group with which you converse relies heavily on modeling and simulation as strategies to test ideas about behavior and perception. The emphasis is clearly on generative models, not descriptive ones. For a variety of reasons, the generative modelers on the net think of information theory and signal detection theory as descriptive. In contrast, you frequently appeal to both of those theories and say they provide you a deeper understanding of PCT. As you said in your post:

"What I do want to do is to get some deductions about the structure and its behaviour that are not obvious, even though they may (should) agree with what you have found to work in practice. I find that it makes much more sense to me to have a good theoretical underpinning that allows me to generalize from a practical result than just to see the practical result and wonder what might happen if some little thing were changed."

Who could possibly see a problem with those thoughts? The difficulty arises when, probably through misinterpretation by your readers, you seem to argue that information theory *obviously* offers a superior, or clearer, understanding of the phenomenon of control and that readers on the net who do not see that fact, on their own, have an inferior understanding of control. Whether their interpretation of your intent is right or wrong, it is clear that some readers begin to take those remarks personally. That is why I offer this suggestion: Demonstrate that you can work from first principles in information theory and (necessarily?) arrive at an architecture identical to that in the PCT model. If the model you derive is identical to the PCT model, you are right; there is no need to simulate it -- to run it. But if it differs in any details, run it, to confirm that it behaves as you think it will. That step should satisfy any questions, doubts or criticisms I have seen directed toward your posts about information theory and PCT.

The following exchange between Bill and you leads me to doubt that you will try the approach I suggest.

****
Bill to Martin:
Martin, the difference that Tom is talking about, I believe, is between a descriptive model and a generative model. A descriptive model provides a general picture of which a

specific behavior is only one example. A generative model actually generates (simulated) behavior for direct point-by-point comparison with real behavior.

****

Martin replies to Bill:
Yes, I understand. I have a bit of a problem with limiting myself to either kind of model exclusively, though, and it is a problem that has been with me since undergraduate days. If a generative model does predict reality well, without excessive use of parameters, then it produces strong evidence of the plausibility of the theory that underlies it. But if the generative model fails, it does not give evidence against the underlying theory, because the failure could have been only in the choice of parameters. So the generative model is a one-sided kind of support. On the other hand, the theory by itself is only plausible unless it can be shown to predict reality, and that can be done only through generative models or mathematical analysis. In the case of your and Tom's models, the prediction is very good. So I see little point in trying to create generative models from what I see as a theoretical support for the same structure on which your models are based. It is conceivable that in some situations the information-theoretic approach might produce numerical statements of more precision or using fewer parameters, but those situations probably will not be easy to find. They will be at higher levels in the hierarchy, most probably. I'm not even going to look for them at present, at least not until I can see some problems with your practical approach that are resolved in the information-theoretic approach.

****

Me (now):
I believe a major question that is unresolved for some of the modelers is whether you would necessarily arrive at the PCT structure. Couldn't you just as easily arrive at other, sometimes implausible, structures? I have seen information theory used to justify or explain many varieties of theory in behavioral and cognitive science. Why should one person arrive at a PCT structure, when so many others did not? I am not saying that you will not, just that I do not see the necessity that you will.

Also, the act of producing and simulating a model does not require that the modeler limit herself or himself to that model over some other(s). Modeling and simulating are tests, nothing more. In "Models and their worlds," when Bill and I constructed and ran a S- R model, a plan model and a PCT model, we did just that. The act itself need say nothing about the preferences or beliefs of the modeler. (Of course, our reviewers thought otherwise!)

****

Martin:
I've been playing with information at an intuitive level for as long as you've been playing with control systems. It's hard for me to get back to basics (or even to exact formulae, since I don't use them much), but it will be a good exercise for me to try.

****

Me (now):
Try it! You might like it! Even should the task prove daunting (which, for you, I doubt), the least you can expect is that your personal insights about PCT and information theory will be more easily digested by readers on the net -- the discussion will be much more likely to remain on a technical level.

Just to clarify my "challenge" .... For me to more easily understand your personal insights about PCT and information theory, I would like to see whether principles in information theory necessarily imply and produce a generative control model and whether a

model so produced can assume the role of a person or a PCT model in a real-time interactive task. The person who accomplishes that demonstration will have made clear a relationship that is not obvious to some people on the net, including me. I am willing to be taught.

Until later,

Tom Bourbon

Date: Tue Dec 22, 1992 10:49 am PST
Subject: The cure for frustration?

From Greg Williams (921222)

>Bill Powers (921222.0800)

>Unfortunately, all the real experimental investigation of control
>behavior is being done by a small handful of people with no
>funding or assistance, who are either retired and decrepit or
>working full time at something else to make a living.
>The timetable depends, therefore, on the person-hours available
>and the facilities for doing the research, including availability
>of human subjects. As long as the list of people actually
>devising and carrying out experiments and modeling is limited to
>Rick Marken, Tom Bourbon, and me, the queue of possible
>experiments with HPCT is going to grow while the actual work done
>trudges along at a slow pace.
>When I last looked, there were 132 subscribers to this list.
>Permit me a moment of impatience: when are some of you people
>going to get out of your armchairs?

I suppose that most of us are either retired and decrepit or working full time at something else to make a living, AND ALSO (instead of doing PCT experiments) doing other things we think are more important TO US than doing PCT experiments. All of that needn't prevent some of us from suggesting possible PCT experiments to you and Rick and Tom; you might not have thought of doing them, and, on occasion, you might actually decide to give them higher priority FOR YOU than the experiments (and other PCT-related activities) you've been working on. My notion in suggesting step-tracking modeling by you was that it would be interesting to see how general the (low-error) continuous tracking model is, and that you and Tom are the ones who are set up to do it (with software and hardware -- I don't even have a working joystick right now -- my kids are primo joystick torturers). Maybe more netters would be moved to become experimenters if they could get the tracking software you use from you or Tom (that would save them a LOT of development time). Maybe you could even give a higher priority to preparing a tracking experiment lab kit for students. If I had a "plug-and-play" tracking lab, I'd know I'd buy a new joystick. So here's a middle way: you and/or Tom come up with a "tracking experiments for nonprogrammers" disk, and I bet several of us will get out of our armchairs. Scolding us isn't a sufficient influence to make ANY of us learn C or Pascal, I'll bet.

As ever, Greg

Date: Tue Dec 22, 1992 12:28 pm PST
Subject: RE: The cure for frustration?

From Tom Bourbon (921222 13:58 CST) --
Greg Williams (921222) gave his reasons for not performing experiments and modeling to test his ideas in PCT. I can hardly believe that he would consider the roof over his family's head (literally) more important than sitting down to a weekend ofprogramming in Pascal or C! Still, Bill's point is well taken: Far too few of us are engaged in the experimental science side of PCT. Bill listed three, and by my own admission I am the least accomplished programmer in the lot. If three more people begin working on experiments and modeling, I will immediately fall to sixth on the chart.

But I remember the day when, without Bill saying a word to me, I realized I had spent a year or more asking him to do things for me -- tweek a program here, add a condition there, and so on. I took some of the source code he so generously gives away, printed it out, spread it across the floor of my lab and started to learn. Of course, I was doing it all backwards, but even that clumsy approach has let me do SOME of the things I want to do with PCT.

As Greg says, most people are busy doing what sems most important to them. So am I. It is just that when I got off of my duff and started hacking away at my code, the things that seem most important to me began to change.

Like it or not, unless more people become active in experimental work on PCT, all of the discussions and hand wringing in the world, over how to present PCT more effectively, will lead to nothing. That is not meant as a harangue -- just the facts.

Until later,   Tom Bourbon

Date: Tue Dec 22, 1992 1:56 pm PST
Subject: On the lighter side

Holiday greetings--

The following exam is provided for those unable to relax over the holidays.
It consists of 4 short answer questions and 1 essay.
_____

How many PCTers does it take to

1. Change a light bulb if the light bulb controls for change? _____

2. Change a light bulb if the PCTer(s) control(s)s for change and the light bulb doesn't care? _____

3. Change a light bulb if the PCTer(s) control(s) for change and the light bulb controls for no change. Your answer should discuss loop gain of appropriate systems, potential for layered protocols to reach mutually acceptable solution, whether information theory is relevant to either control system. Provide the names of 3 reviewers who agree with you answer and a working model in ADA or COBOL. Cite references in three languages, at least one of which is not alphabetic.

4. Propose a means whereby two PCTers might determine whether a light bulb found on table might control for no change. HINT: flirting won't do it.

5. Translate and provide correct grammar for the following:

"De apibis et filibus apiarem semper dubitandum est."

The very best holiday wishes to all. May all your perceptions be closely alligned with your higher level control system, this day and forever more.

Bill Cunningham

Date: Tue Dec 22, 1992 3:50 pm PST
Subject: Re: Misc replies

[Martin Taylor 921222 18:15] (Bill Powers 921222.0800)

> As long as the list of people actually
>devising and carrying out experiments and modeling is limited to
>Rick Marken, Tom Bourbon, and me, the queue of possible
>experiments with HPCT is going to grow while the actual work done
>trudges along at a slow pace.
>
>When I last looked, there were 132 subscribers to this list.
>Permit me a moment of impatience: when are some of you people
>going to get out of your armchairs?

Well, we are trying the Little Baby experiments, and are doing preliminary work for Genetic Algorithm experiments, both to study reorganization. Does that count?

==========

>Martin Taylor (921221.2000) --
>
>>If a generative model does predict reality well, without
>>excessive use of parameters, then it produces strong evidence
>>of the plausibility of the theory that underlies it.
>
>The control system model we use predicts detailed handle
>movements within 5 to 10 percent with a single adjustable
>parameter, the integration constant. By adding a transport lag,
>we can approximately halve that error. So at most we need two
>parameters to reduce prediction error to 2.5 to 5 percent. If we
>introduce nonlinearities, we might get that down to 2 to 4
>percent. But diminishing returns will set in pretty quickly. I
>figure that we are pretty near the noise level set by the
>discrete nature of neural impulses.

I intended to imply that PCT had that kind of requisite support. It's one reason why I think it worthwhile working on the foundations. But I'm equally impressed with the argument from evolution and other more abstract arguments of necessity and possibility. They all support one another.

==========

>I think you're going to have to take us back to a more elementary
>level. I still don't understand why a perceptual signal that is

>maintained in a match with a changing reference signal is said to
>have a LOW information content.

...

>>In the 2Hz case, the presumption is that all these
>>probabilities are flat (actually Gaussian) distributions,
>>maximizing the information that could be transmitted.
>
>Who is doing this presuming in the system? I asked why the
>perceptual and reference signals should not both be considered to
>carry an information flow appropriate to a signal varying within
>a 2Hz bandwidth. You didn't answer that.

The presumption was one that you (Bill Powers) would have to make in order to assert that the perceptual and reference signals would convey to you (Bill Powers) information at a rate appropriate to a 2Hz bandwidth. To anyone else, the information rates might be different.

I'm working on a more considered presentation of the information-theory stuff, as requested. But it isn't going to be immediate. To tighten it up and make it didactically useful is going to take work, especially since I have to go back to basics, as these questions suggest. It could be that I will make up some document to be deposited with Bill Silvert or to be circulated on paper (lots of pictures required).
==========

(Tom Bourbon 921222 10:10)

Tom,
Thanks for your considered (and considerate) posting.

> I offer this suggestion: Demonstrate that you can work from
>first principles in information theory and (necessarily?) arrive at
>an architecture identical to that in the PCT model. If the model
>you derive is identical to the PCT model, you are right; there is
>no need to simulate it -- to run it. But if it differs in any
>details, run it, to confirm that it behaves as you think it will.
>That step should satisfy any questions, doubts or criticisms I have
>seen directed toward your posts about information theory and PCT.

Yes, I started working on it after yesterday's postings. See above. I would very much like to see it actually run as a model of the type you like, but as a practical matter I spend much more time on CSG matters than I should (as well as having contractors work on PCT-related issues). I am not a strong programmer, though I sometimes find it fun. Quite probably I will write the document that Bill asks for, and then see whether it opens fruitful lines of discussion that might induce me or someone else to do the actual generative modelling. Producing the document has obvious benefits for me, to make the ideas more precise, rather than intuitive, as in the posting to you. And it would seem necessary if the ideas are to be propagated (assuming that they have value).

>I believe a major question that is unresolved for some of the
>modelers is whether you would necessarily arrive at the PCT
>structure. Couldn't you just as easily arrive at other, sometimes
>implausible, structures? I have seen information theory used to
>justify or explain many varieties of theory in behavioral and

>cognitive science. Why should one person arrive at a PCT
>structure, when so many others did not? I am not saying that you
>will not, just that I do not see the necessity that you will.

You are quite right about the (mis)uses to which information theory has been put, and this paragraph will make me look much harder at my assumptions than I otherwise might have done. I do not want to provide a circular argument "PCT, therefore PCT." What I believe should come out is "chaotic world, therefore PCT."

>> It's hard for me
>>to get back to basics (or even to exact formulae, since I don't use
>>them much), but it will be a good exercise for me to try.
>
>Try it! You might like it! Even should the task prove daunting
>(which, for you, I doubt), the least you can expect is that your
>personal insights about PCT and information theory will be more
>easily digested by readers on the net -- the discussion will be
>much more likely to remain on a technical level.

I think I would like it eventually. But as with getting into physical shape, the muscles hurt during the build-up period, so one tends to avoid the exercise.

One problem I have is that I always feel that if I understand something, then any moderately competent person with whom I wish to communicate will also have understood it. Intellectually, I know this not to be true, but it always feels to me to be patronizing when I start to explain something that the listener presumably knows. This problem causes great confusion when I start speculating on the basis of things that are not known to the other parties. That's obviously what is happening here.

Anyway, thanks to you both.   Martin

Date: Tue Dec 22, 1992 5:04 pm PST
Subject: Internet Tear

[From Rick Marken (921222.1200)]

I just learned that our Internet connection is down. So that means 1) both of my last posts will show up on CSG-L at some "indeterminate" later time and 2) you will not see this note of apology for some time either. But, again, sorry for the double posting -- looking forward to getting into whatever might be the prevailing argument (er... discussion) at the time that Internet returns.

PS. This note should not be taken as evidence of feedforward control.

Regards   Rick

Date: Tue Dec 22, 1992 5:49 pm PST
Subject: Re: Rick on (off) Shannon

Sent this last night but got no ACK from CSG-L and didn't see it on the net this morning -- pardon me if this shows up twice; I'm possibly still suffering from results of bad trip (well, the trip was fine, actually -- the mail waiting on my arrival left something to be desired, however).

[From Rick Marken (921221 2000)]

Martin Taylor (921221 18:00)

Thanks for posting the correspondance; now I see what happened.

You said:

>I suppose I would agree that error could be controlled if it were to be
>provided as input to the perceptual input function of a control system that
>maintained the error at some reference level.

I replied:

>As you will see in a reply I plan to make to Greg, there is no
>supposing necessary in this case. The error signal (which is now
>a perceptual input to a control system) would DEFINITELY be controlled.

You said:

>But what on earth would that do other than enforce a situation in the
>ECS whose error was being controlled that would ordinarily arise only
>through unresolvable conflict. I can't see why any hierarchy would include
>such a mechanism--but I suppose there might be a reason somewhere.

In other words, you were rejecting the idea that perception of error in the control hierarchy should be the object of control by a reorganizing control system. But the notion that perceived level of error is an intrinsic variable controlled in a reorganization loop seems necessary to explain the many cases of learning where you develop new control skills even though you still can eat and breath. Dick Robertson did a nice experiment to show that reorganization can happen for no other reason than to solve a problem "better" than it's currently being solved; no obvious "intrinsic" variable is controlled except control skill (low level of error in the control system) itself.

So the discussion was about whether the reorganization model should be able to control the quality of control (measured as perceived level error in one or more control systems) -- and I think it should. But we seemed to have lost this thread and moved into a discussion of error and this is where I misinterpreted what you were talking about. You said:

>Actually, what Bill has done seems to be a little more complex. He has
>found that the derivative of the squared error is a better criterion.

And I said:

>You mean, as the variable controlled by the reorganization system, right?

This was my mistake. When I saw your formula for error, (K1(e^^2)+K2(e* de/dt), I assumed that this was the PERCEPTUAL FUNCTION, f, which transforms error in the hierarchical control systems (e) into the perception controlled by the reorganizing system, P, so that P = f(e). (I will adopt your convention of using capitol letters to denote variables in a reorganization loop -- small letters to denote the corresponding variables in a "regular" control system in the PCT hierarchy). In fact, your formula is for the OUTPUT function, g, that transforms the error in the reorganizing control system (E) into the reorganizing system output (O), so O = g(E). This fooled me because we rarely need to put

non-linearities into the output functions of our models of tracking type data; just linear amplification. If we did add such non-linearities, they would be "absorbed" by the control loop (assuming that they are at least monotonic). But the output of the reorganizing system is not an amount; rather, like the tumbles of e.coli, the time between reorganizing events is the output that matters. So a non-linear output function might be just what the doctor ordered to make reorganization efficient; now I recall that THIS is what Bill discovered in his studies of reorganization algorithms that we talked about this summer.

So, Martin, I was not arguing that error is controlled (as you mistakenly said in your summary of our exchange -- I'm quite sure, unintentionally; I don't think you tried to mislead); I just said that one intrinsic perceptual variable that might be controlled by a reorganizing system (which is itself a control system controlling perceptions relative to intrinsic references) is some measure of ambient error in the control hierarchy itself. So now we can get back to THAT conversation; do you think that reorganization involves control of perceived error in the hierarchy (in human control systems)? Why or why not? Can you think of a behavioral test for such a model? (These are serious questions-- I don't know the answers but I would appreciate hearing suggestions -- then maybe we could design some real experiments instead of just blabbering away).

As for Shannon and information theory; could you give me another sample of this "understanding" of PCT that you get with info theory that you don't get otherwise? Could it be that info theory is just one of those comfortable old pieces of wisdom (like reinforcement, statistics, reflexes, information processing, experimental methodology, decision theory. etc) that just MUST fit into this (PCT) SOMEWHERE? What if it doesn't?

Best regards Rick

Date: Tue Dec 22, 1992 8:41 pm PST
Subject: PCT For the Rest of Us

[from Gary Cziko 921223.0315 GMT] Bill Powers (921222.0800) observes:

>As long as the list of people actually
>devising and carrying out experiments and modeling is limited to
>Rick Marken, Tom Bourbon, and me, the queue of possible
>experiments with HPCT is going to grow while the actual work done
>trudges along at a slow pace.

>When I last looked, there were 132 subscribers to this list.
>Permit me a moment of impatience: when are some of you people
>going to get out of your armchairs?

I can understand Bill's frustration that there are so few people on this planet who are doing basic research on PCT. But surely, while the modeling is very important, I would like to think that there are lots of ways of advancing PCT without doing the nitty-gritty modeling. Look at what Phil Runkel, Kent McClelland and Ed Ford have done with PCT--none of them modellers. As for me, while I am eager to learn as much as I can from others' modelling efforts, I don't see myself doing this kind of research other than for learning PCT and helping my students learn it. I am interested in trying to apply what PCT has already shown us about human behavior to real problems that cannot wait until all the 11 or so levels have been mapped out and fully understood (if ever).

For example, lately I have been considering how PCT could be used to understand and help minority children in our schools. It has been noted (primarily by John Ogbu at Stanford and his colleagues) that cultural and language differences are in themselves not obstacles to school success. Many children (e.g., Asians in the U.S.) excel in U.S. a few years after arrival here. Others (e.g., Blacks and Native Americans) do not do nearly so well. Ogbu tries to make sense out of this by observing that the voluntary minorities see cultural and language differences as obstacles to overcome in order to succeed while involuntary minorities tend to see cultural and language differences as a type of protective barrier to maintain. So it is not cultural and language differences which "cause" academic failure, but rather the goals of the students. I think that PCT could be very useful in furthering our understanding of this.

I also think PCT could form the core of programs designed to help minority students. Take Black students, for example. They should already be learning in school about how many Black Americans were able to accomplish remarkable things in spite of the persecution and discrimination they suffered. But do they learn HOW this was possible? Do they learn how such people were able to rise above the "disturbances" imposed by the majority society and achieve their goals? Of all the sociological and psychological theories I know about, only PCT can make sense of this. And PCT can also give minority students the basis for understanding how they can also achieve very "improbable" goals in spite of the disturbances they will encounter and how the group statistics on drop-out rates and drug usage don't mean a thing to a living control system which has other goals.

When I put my imagination loop in overdrive I can begin to see PCT at the core of a new curriculum for minority children. Starting with learning about remarkable individuals of their and other minority groups through literature and history and films. Then learning about PCT and how this remarkable new perspective on life explains how people control and are not controlled by their environment. And how PCT gives a new understanding of stress and conflict and how to find alternatives to violence. And then getting into the kinds of things that Ed Ford does in having peolple examine their needs and priorities and helping them get what they really want out of life by looking at the world from the very top of their control hierarchy.

If it is not clear that such ideas are needed in American schools NOW, take a look at part of recent note from my sister who is a high school teacher in New York City.

>I can't begin
>to tell you how my high school has deteriorated. 4200 students,
>metal detectors, 17 full time security guards and still one of my
>ninth graders was shot right on the front steps in October. Kids
>sleep in class (when they come) or talk and curse through much of
>the lesson and the small group in each class that is still trying
>is so far behind in skills that I often feel its hopeless.

So while the modelling must be done so that PCT will have a firm base, I don't think many of us with applied interests should or need to wait much longer. I'm still trying to get the basics down and finish up some previously started projects. But once I have the expertise and time, I am going to apply what is already known about PCT to real problems. Who knows. Maybe even the computer modellers will find results of such applications interesting.--Gary

Date: Wed Dec 23, 1992 6:50 am PST
Subject: RE: On the lighter side

From Tom Bourbon (921223 08:43 CST
-- by the dim light of a new and foggy day amid the palms)

Thanks, Bill Cunningham, for a luminous little post with which to start the day. To you, and everyone else on csg-l, may Boss Reality tred lightly on the controlled variables of you and of those you love.

Happy holidays.   Tom Bourbon

Date: Wed Dec 23, 1992 8:08 am PST
Subject: Re: Rick on (off) Shannon

[Martin Taylor 921223 11:00]   (Rick Marken 921221 2000) (delayed)

Rick,

I think the misunderstanding has been cleared up by your posting.

> So now we can get back to THAT conversation; do
>you think that reorganization involves control of perceived error in
>the hierarchy (in human control systems)?

Yes.

>Why or why not?

It seems a very plausible way that the linkages in the hierarchy can be connected with appropriate signs (and as I mentioned in a posting a few weeks ago, there seem to be 12 different ways that reorganization of this kind could be effected, so I'm not plumping for a mechanism).

>Can you think of a behavioral test for such a model?

Not offhand. And I think that a good part of this reorganising system is outside the individual, occurring on an evolutionary time-scale, so that aspect couldn't be demonstrated behaviourally.

>As for Shannon and information theory; could you give me another sample
>of this "understanding" of PCT that you get with info theory that you
>don't get otherwise? Could it be that info theory is just one of those
>comfortable old pieces of wisdom (like reinforcement, statistics,
>reflexes, information processing, experimental methodology, decision
>theory. etc) that just MUST fit into this (PCT) SOMEWHERE? What if it doesn't?

I admit the possibility, since we can never know when we are fooling ourselves. But see the postings to Tom and Bill yesterday. I hope that I will be able to satisfy you with what I plan to write in a more serious vein than impromptu net postings.

Happy Xmas.   Martin

Date: Wed Dec 23, 1992 8:46 am PST
Subject: Bill, Gary, Tom

[From Wayne Hershberger]

Bill, I am delighted with your argument (Powers, 921220)--relating perceptual and conceptual EVs. Some of your remarks I found particularly refreshing; for instance:

> All we see is the combination of the natural law and the law represented by the form of the perceptual function.

And,

> The...problem is to find organizations of the input functions that will yield controllable variables

As for your comment:

> Some added thoughts about this were in my head when I woke up (921221).

I can not help observing that it must have been a long night.

More later, Bill.

Gary Cziko:

The VOR is not perfect; nor is the optokinetic reflex. I think the gain (eye-velocity/head-velocity) of these two reflexes combined is only about .9, meaning that during active head rotation of 50 deg/s, images slip across the retina at 5 deg/s.

David Robinson claims that the VOR involves the control of eye position as well as eye velocity. He also claims that the saccadic system CONTROLS EYE POSITION VIA CLOSED LOOP CONTROL, and argues that this phylogenetically late system commandeered aspects of the earlier VOR system in its development.

I believe Tom Bourbon's recent post regarding the control of error is really an essay concerning feedforward. Feedforward can be thought of as endogenous disturbances to reference signals, error signals, or output (as I argued in my ABS article in the special issue edited by Rick). You might want to reread Tom's posts with that in mind.

Tom Bourbon:
Your post regarding the disturbance of error signals is germane to the discussion Andy and I were having in Durango about Pavlovian conditioning. Has he read your post?

Warm regards, Wayne

Date: Wed Dec 23, 1992 9:31 am PST
Subject: Descriptive/generative; info theory; applying PCT

[From Bill Powers (921223.0915)]

It's interesting how a defense turns into an attack, and how letting down the defenses also reduces the attacks. Sometimes the best defense is no defense at all.

Martin Taylor (921222.1815) --

>But I'm equally impressed with the argument from evolution and
>other more abstract arguments of necessity and possibility.
>They all support one another.

The problem with arguments from evolution and other abstract arguments is that while they may apply to a particular case, they may also turn out to apply to counterfactual cases. They may contain flaws that don't show up in the factual case (for instance, flaws in internal logic), but those flaws can make the generalized explanations specious, even though they "support" a more concrete analysis.

A generative model can be wrong; it can produce right results for wrong reasons, which show up only when circumstances change enough to reveal a wrong prediction. This is how generative models progress; from error to error. Generative models commit themselves to specific proposals about underlying details of operation. That makes them very sensitive to experimental test.

Descriptive models, however, are not very sensitive to experimental test, some not at all. If they're cleverly put, their predictions can remain true no matter what the outcome of an experiment. So they are capable of "supporting" completely contradictory generative models. This is not really support at all. It is merely agreement. All experimental results pertaining to behavior, whether correct or erroneous, are agreed to by the generalization that natural selection produced the behavior we find with our experiments. So evolutionary theory "supports" whatever we find to be the case -- even if, later on, we discover a mistake and find something else to be the case.

This is why I prefer generative models. They are more sensitive to experimental test.

RE: information theory

>>Who is doing this presuming in the system? I asked why the
>>perceptual and reference signals should not both be considered
>>to carry an information flow appropriate to a signal varying
>>within a 2Hz bandwidth. You didn't answer that.

>The presumption was one that you (Bill Powers) would have to
>make in order to assert that the perceptual and reference
>signals would convey to you (Bill Powers) information at a rate
>appropriate to a 2Hz bandwidth. To anyone else, the
>information rates might be different.

Ok, so to me, the perceptual signal carries low information if I already know the reference signal, and vice versa.

However, if we define the comparator as the receiver of the information in both channels, doesn't this imply that to the comparator, the same must be true? This doesn't seem satisfactory to me. Perhaps we have to define the receiver more carefully. A comparator receives the reference signal and the perceptual signal at the same time, so it knows neither one before the other. Further, what it "knows" is only amplitude, over a relatively short span of time like 0.1 sec or less. It immediately forgets the history of both signals, and it contains no machinery for making extrapolations of either signal into the future. It seems to me that this comes down to the original Shannon application of information theory -- to defining the information CAPACITY of a channel rather than the actual information flow in that channel, which can be less than the channel capacity. At times when no signal is flowing, the information flow is zero, yet the physical channel capacity remains the same. Channel capacity is a function of physical design; information flow is a function of the kind of message being carried -- not so?

Gary Cziko (921223.0315 GMT) --

>But surely, while the modeling is very important, I would like
>to think that there are lots of ways of advancing PCT without
>doing the nitty-gritty modeling.

There are ways of APPLYING and TESTING the findings of PCT without modeling and formal experimentation, but there aren't any ways of ADVANCING it. If you're willing to take the word of modelers and experimenters about how control systems work (199223), then you can apply PCT all over the place. There are, however, dangers in putting too much trust in a theory that is underdeveloped -- in taking the pronouncements of theoreticians and researchers on faith.

One way to guard against these dangers to some extent is to make sure that every application of PCT that you think of is also designed as a TEST of PCT. In psychology, theories of behavior, once "determined" to be true by someone's study, are never tested again. They are used to explain behavior and diagnose behavioral problems, but nobody ever says "If this theory is still true, in this situation, then when I do X (or X happens) I ought to observe consequence Y." Instead, what is observed is some consequence Y, and it is assumed, because of the theory, that X must have occurred. If depressed white males have been found to have difficulty with following complex instructions, then if a person has difficulty following complex instructions, that person must be depressed (or a depressed white male).

PCT can be used the same way, of course. But PCT is designed from the ground up as a predictive model. It can be applied that way. PCT says that organisms, when pushed, push back (whatever the controlled variable being pushed upon). If you want to see if a person's behavior fits this model, you push on what you believe to be a controlled variable and see if the person pushes back to counteract your push. If so, you can explain that particular behavior using the same model.

If you want to find out whether black, brown, tan, or beige Americans "tend to see cultural and language differences as a type of protective barrier to maintain," you try to disturb these barriers (or look for natural disturbances) and look for actions that specifically counteract the disturbances. You may find that for some differences this holds true and for others it doesn't. "Cultural and language differences" cover a lot of territory, and much of it may be irrelevant. It would be better to find out what disturbances they do resist, and make the generalization afterward. And it's important to make a prediction from control theory (and your hypothesis about the controlled variable) FIRST, so you're committed before the fact. If this happens, that should happen. If _that_ doesn't happen, you have to revise your hypotheses about the controlled variable and try again, and keep trying until you predict correctly. Then and only then will you know what's going on. If you NEVER can make a right prediction, control theory is probably wrong.

I think you should always apply control theory as if you're putting the theory itself on the line, and challenging nature to behave the wrong way. Mind the turtle; it makes progress by sticking its neck out.

Tom Bourbon --

You said it for me; here it is again:

" .. may Boss Reality tread lightly on the controlled variables
of you and of those you love. Happy holidays."

Best to all, Bill P.

Date: Wed Dec 23, 1992 9:38 am PST
Subject: On the lighter side

[From Rick Marken (921223.9000)]

Bill Cunningham --

Holiday greetings to you too. Great exam. I'm still working on it. I think I got one:

>4. Propose a means whereby two PCTers might determine whether a light bulb
>Found on table might control for no change. HINT: flirting won't do it.

Suppose one PCTer tries to screw the bulb in -- the other tries to screw it out. Does this count as flirting? Or are we approaching "safe sockets".

>The very best holiday wishes to all. May all your perceptions be closely
>alligned with your higher level control system, this day and forever more.

And may all your goals be adjustable.

Regards Rick

Date: Wed Dec 23, 1992 9:53 am PST
Subject: tapping my gas

[From: Bruce Nevin (Wed 921223 12:33:51)]

/***
* (Martin Taylor 921222 18:15)   *
*   *
* . . . it always feels to me to be patronizing when I start to explain   *
* something that the listener presumably knows. This problem causes great *
* confusion when I start speculating on the basis of things that are not   *
* known to the other parties.   *
***/

A universal problem.

Once years ago my wife and I stopped at a park in a small town in the wastes of Nevada and there over picnic lunch struck up a conversation with a Basque sheep man. He had been a miner, and when I asked him what that was like he started his response thus: "Now you're tappin' my gas." I hope you'll indulge me as I did him, though I'm not so entertaining or colorful as he.

A writer's maxim: never underestimate the reader's intelligence; never overestimate the reader's prior knowledge. The general tone is "you probably already know this, bear with those reading this who are not so familiar with it." It's the reason I think it's a good idea to spell out familiar acronyms like ECS (elementary control system) on their first occurrence in a discussion of any length. It seems obvious and redundant, but can make a big difference to a newcomer, and precisely because of the redundancy the burden on the practiced reader is light.

Bruce bn@bbn.com

Date: Wed Dec 23, 1992 12:22 pm PST
Subject: Re: Descriptive/generative; info theory; applying PCT

[Martin Taylor 921223 14:40] (Bill Powers 921223.0915)

>>But I'm equally impressed with the argument from evolution and
>>other more abstract arguments of necessity and possibility.
>>They all support one another.
>
>The problem with arguments from evolution and other abstract
>arguments is that while they may apply to a particular case, they
>may also turn out to apply to counterfactual cases. They may
>contain flaws that don't show up in the factual case (for
>instance, flaws in internal logic), but those flaws can make the
>generalized explanations specious, even though they "support" a
>more concrete analysis.

What I'm talking about is sometimes called "converging operations." Or it was when I was in graduate school. If looking at a problem from several different viewpoints gets the same answer each time, or if an answer turns out to be in common among many from each of the various viewpoints, then that answer becomes much more credible than if it is precisely determined from only one viewpoint.

I doubt that I would buy PCT simply because an evolutionary analysis suggested that hierarchic control systems would have enhanced stability and hence would be observed preferentially among living organisms. But I would be much less likely to buy PCT if the evolutionary argument went the other way. It helps the credibility of PCT a great deal that organisms that are organized as control hierarchies are more survivable than organisms with other proposed structures (e.g. stimulus-reactors or simple planners).

>Descriptive models, however, are not very sensitive to
>experimental test, some not at all. If they're cleverly put,
>their predictions can remain true no matter what the outcome of
>an experiment. So they are capable of "supporting" completely
>contradictory generative models. This is not really support at
>all. It is merely agreement.

Yes, the degree of support depends on where the competition is. If the descriptive model supports two contradictory generative models, it supports neither, but it might support that class of models (the gaphroomus could be black or white) against a different class (the gaphroomus mustn't be spectrally coloured). The importance of agreement depends on the prior likelihood of disagreement.

This aspect of converging operations applies to your critique of Gary. If PCT usefully suggests ways of assisting people, and those ways work reasonably often (not necessarily always), then that success also argues that PCT is a valuable way of looking at the psychological world. It doesn't prove anything, but it does reinforce the idea that PCT is worth following up the levels hierarchy, and is not restricted to tracking tasks with measurable errors. You have also provided examples at the level of personality (self-image) that make the same point.

Occam's razor deals with the amount of specification needed in order to desribe phenomena. If one descriptive method can be used over a wider range of phenomena than another, or if it describes phenomena more precisely within a given range, it is to be preferred. PCT does both. It substitutes one descriptive method for a variety of topic-specific theories, and in the application area of each theory it makes more precise descriptions.

To me, a model is only a concise way of making descriptions. A generative model, as you put it, is distinct from a descriptive theory. To me, it is not distinct. It only uses a few more parameters to make a much better description, and if that description is accurate, Occam's razor says that it is preferred. But if its precision allows the world to say "that's not right," it could be that the parameters are wrong. If the parameters can be changed and still lie within the bounds of "the theory", then the information contained in the parameter specification has to be included in the "size" of the description, so the case becomes less clear.
==========

On information theory: Channel capacity is not specifiable from physical characteristics alone. A physical channel may have many different capacities simultaneously. And (as follows from one of your own postings a while back) you can have continuous information flow without signal flow.

I prefer, I think, to set aside postings on information theory until I can produce a coherent PCT-oriented tutorial. That will, I hope, provide a reasonably good argument as to why PCT and specifically HPCT is, with high probability, the only viable organization for thermally unstable organisms (i.e. life) to persist. As both Tom and Rick have said directly, and you imply, the argument may not turn out to be as strong as that, and may provide only agreement. But at least it will be an argument from a different viewpoint against simple planners and S-R organisms, and provide a converging operation in support of PCT.

I have a growing suspicion that we have a different philosophy of science, and that at some point this difference will be the theme of a posting thread.

Martin

Date: Wed Dec 23, 1992 12:33 pm PST
Subject: Feedforward; Information

From: Tom Bourbon (921223 13:20 CST)

[From Wayne Hershberger] (23 Dec 92 10:38:54 CST)
In a reply to Gary Cziko's post on VOR:

>I believe Tom Bourbon's recent post regarding the control of error
>is really an essay concerning feedforward. Feedforward can be
>thought of as endogenous disturbances to reference signals, error
>signals, or output (as I argued in my ABS article in the special
>issue edited by Rick). You might want to reread Tom's posts with
>that in mind.

Wayne, I tried to follow the suggestion you made to Gary, but it doesn't work. Every time I reread my post on control of error, I see a post on control of error, with

illustrations of what happens when various signals in a PCT model are disturbed, one at a time. I do not see an essay on feedforward.

Feedforward can be thought of as many things other than the possibility you describe here and in your article in ABS. I don't recall ever seeing anyone else interpret the word "feedforward" in the same manner as you. I still have problems with your interpretation in ABS (American Behavioral Scientist) of Pavlovian conditioning as an instance of feedforward.

Perhaps Gary does not need help with this, but I need some additional information from you if I am to understand why my post was really an essay on feedforward. Could you begin with your definition of feedforward?

>Tom Bourbon
>Your post regarding the disturbance of error signals is germane to
>the discussion Andy and I were having in Durango about Pavlovian
>conditioning. Has he read your post?

Andy (Papanicolaou) has been out of town for the past two weeks. A printout of my post is on his desk, where it will soon be joined by copy of your post. I will let you know what he thinks.

****
Rick Marken and Martin Taylor (several posts)

Thanks to both of you for sharing your previously off-line discussion about control of error.

****
[Martin Taylor 921222 18:15] (Bill Powers 921222.0800)

Bill:
>>As long as the list of people actually
>>devising and carrying out experiments and modeling is limited to
>>Rick Marken, Tom Bourbon, and me, the queue of possible
>>experiments with HPCT is going to grow while the actual work done
>>trudges along at a slow pace.
>
>>When I last looked, there were 132 subscribers to this list.
>>Permit me a moment of impatience: when are some of you people
>>going to get out of your armchairs?

Martin:
>Well, we are trying the Little Baby experiments, and are doing
>preliminary work for Genetic Algorithm experiments, both to study
>reorganization. Does that count?

I haven't seen a reply to this from Bill, but it certainly should count. Just don't tell me how many people other than Chris Love are working on the programs -- Chris alone pushes me to a distant fourth on a list of four!

****
Martin to Bill:
>I'm working on a more considered presentation of the information-
>theory stuff, as requested. But it isn't going to be immediate.

>To tighten it up and make it didactically useful is going to take
>work, especially since I have to go back to basics, as these
>questions suggest. It could be that I will make up some document
>to be deposited with Bill Silvert or to be circulated on paper
>(lots of pictures required).

and to   (Tom Bourbon 921222 10:10)

>Yes, I started working on it after yesterday's postings. See
>above. I would very much like to see it actually run as a model of
>the type you like, but as a practical matter I spend much more
>time on CSG matters than I should (as well as having contractors
>work on PCT-related issues). I am not a strong programmer, though
>I sometimes find it fun. Quite probably I will write the document
>that Bill asks for, and then see whether it opens fruitful lines
>of discussion that might induce me or someone else to do the
>actual generative modelling. Producing the document has obvious
>benefits for me, to make the ideas more precise, rather than
>intuitive, as in the posting to you. And it would seem necessary
>if the ideas are to be propagated (assuming that they have value).

It seems obvious that your intuitions concerning links between information theory and PCT are strong. If the sometimes heated exchanges on that topic here on csg-l result in your clarifying those ideas for yourself, then we all will share in the benefits you envision, whatever the outcome of your efforts.

Tom:
>>I believe a major question that is unresolved for some of the
>>modelers is whether you would necessarily arrive at the PCT
>>structure. Couldn't you just as easily arrive at other,
>>sometimes implausible, structures? I have seen information
>>theory used to justify or explain many varieties of theory in
>>behavioral and cognitive science. Why should one person arrive
>>at a PCT structure, when so many others did not? I am not saying
>>that you will not, just that I do not see the necessity that you
>>will.

Martin:
>You are quite right about the (mis)uses to which information
>theory has been put, and this paragraph will make me look much
>harder at my assumptions than I otherwise might have done. I do
>not want to provide a circular argument "PCT, therefore PCT."
>What I believe should come out is "chaotic world, therefore PCT."

If that is what comes out, you need no longer wonder whether your ideas should be propagated, or whether they have value.

Until later,   Tom Bourbon

Date: Wed Dec 23, 1992 2:29 pm PST
Subject: PCT Fanatics

[From Rick Marken (921223.1300)]

OK, nobody's in the office; I should be home cuddled up by the Christmas tree (yes, I prefer celebrating the apocryphal birth of the prince of peace to the non - apocryphal victory of the Macabees over the Assyrians [or whomever]). So, since I have time, I want to try to start a discussion of something that has been on my mind for a week or so but has taken a back seat to my discussion with Martin, in regard to which (I think) Bill Powers (921223.0915) said:

>It's interesting how a defense turns into an attack, and how
>letting down the defenses also reduces the attacks. Sometimes the
>best defense is no defense at all.

Will I ever learn????

Thanks for the above, Bill; an appropriate observation for the season.

What I wanted to talk about was the problem of being perceived as a, well, fanatic, when one gets into PCT. Of those posting regularly on the net, I think that I might be perceived as the one most deserving of the 'PCT fanatic' title (maybe it's my strident, obnoxious manner -- but there are others whom, I feel, are equally deserving of this title; Bill Powers himself, for instance; Tom Bourbon certainly). But I seem to be perceived as particularly fanatic -- or am I particularly paranoid? Some weeks ago Oded referred to me as the 'True believer', I think others have alluded to my apparent unwillingness to see the merit in non-PCT approaches to understanding human nature (info theory, dynamic attractors, fuzzy logic, artificial life, etc etc). So what I want to do is try to explain my fanaticism by arguing that there is really no way to be anything other than a fanatic once you accept the basic principle of PCT -- that behavior is controlled perception.

I think the idea that one is a fanatic about PCT comes from what I alluded to above -- the apparent failure of the fanatic to see any merit in non-PCT approaches to understanding human nature. These non-PCT type approaches are themselves often treated with something close to reverence by non-PCTers. So even people who are attracted to PCT (for whatever reason) assume that there must be SOMETHING of value in some old approach. How could geniuses like Freud (psychodynamic theory), G.A. Miller (information theory),Skinner (reinforcement theory), Green and Swets (signal detection theory), Estes (stimulus sampling theory), Rummelhart (parallel distributed processing theory), Chomsky (transformational grammar theory), Guilford (trait theory), Tolman (sign-stimulus theory) etc etc ALL BE WRONG? How can you (the PCT fanatic) ignore these theories? Why can't you incorporate what is useful and ignore what is not?

The answer is that ALL of these theories were based on a completely incorrect view of behavior. They are ALL based on the idea that outputs (neuroses, responses, operants, decisions, behavior, speech, intelligence, movements) are caused by events in the environment or the brain. PCT shows that this idea is completely wrong; it's not just a wrong point of view or the wrong description; ITS JUST NOT HOW BEHAVIOR WORKS; it can't be, because organisms EXIST in a NEGATIVE FEEDBACK SITUATION with respect to their environment. As Lee Iacocca says (but it's really true in this case) THIS CHANGES EVERYTHING.

For me, one of the most dramatic demonstration of this fact is given in my 'Cause of control movements...' experiment (included in Mind Readings). This experiment shows beyond doubt that THE STIMULUS in a tracking task IS NOT THE CAUSE OF OUTPUTS that control the stimulus; there is no cause-effect (where it seems that there must be) because the cause and effect are IN A LOOP. It's the loop that changes everything that

has always been taken for granted in all previous approaches to understanding human nature.

Once you do an experiment like this and experience the fact that what the math says is really true then all the old approaches become irrelevant. There is no longer any way to take seriously theories that propose 'stimulus guidance' or 'feedback guidance' because stimuli don't cause or guide anything in a loop -- they just don't. Nor is it possible to take seriously theories that propose internal mechanisms for generating behavior -- because behavior is not generated -- it is part of a loop in which the behavior that is generated is also the cause of what generated the behavior. The old explanations of behavior were based on a concept of behavior that was flat out -- completely -- wrong. The chances that such explanations might have something useful to say about behavior as it actually exists -- as controlled perception -- are, from the point of view of this fanatic, quite slim.

Even fanatic PCTers are willing to look at observations that might suggest places to look to better understand the nature of control. But once you know what it means to live in a negative feedback loop, you know that all explanations of behavior that have not correctly taken this loop into account (ie. ALL non- PCT explanations of behavior) can be safely ignored.

I guess my bottom line argument is that it's tough to understand PCT and NOT be seen as a fanatic. Nevertheless, I am more than willing -- anxious even -- to be convinced that there is some value in non-PCT approaches to understanding behavior. I guess Martin is preparing a thesis on the value of information theory for understanding control . I'm waiting with great interest to see what have missed by ignoring iformation theory. I have read several rather unconvincing attempts to show that some versions of behaviorism are equivalent to control models of behavior. If anyone else out there has a non-PCT theory that they think provides a real great explanation of some aspect of behavior then I'd sure like to hear about it. I may be a fanatic but I am willing to listen. But, being a fanatic, I can't promise that I will be convinced. But listening is good too, no?

Best regards Rick

Date: Wed Dec 23, 1992 3:09 pm PST
Subject: Re: PCT Fanatics

[Martin Taylor 921223 17:40] (Rick Marken 921223.1300)

Nobody is in the office here either. Aren't we all a bit crazy?

Rick, would you accept a small revision--

> How could
>geniuses like Freud (psychodynamic theory), G.A. Miller (information
>theory),Skinner (reinforcement theory), Green and Swets (signal detection
>theory), Estes (stimulus sampling theory), Rummelhart (parallel distributed
>processing theory), Chomsky (transformational grammar theory), Guilford
>(trait theory), Tolman (sign-stimulus theory) etc etc ALL BE WRONG?

>The answer is that ALL of these theories were based on a completely
>incorrect view of behavior. They are ALL based on the idea that outputs
>(neuroses, responses, operants, decisions, behavior, speech, intelligence,
>movements) are caused by events in the environment or the brain.

Not all of these examples are theories of behaviour, and so I might suggest adding to your first paragraph " ... ALL BE WRONG as applied to behaviour."

I think specifically of information theory, signal detection theory, and PDP, all of which exist quite independently of whether there is a living organism in the neighbourhood. And the first two are mathematical statements which have universal applicability, even within PCT, if they are used appropriately. The problem with them is not that they are wrong in themselves, but that they are often wrongly applied and misunderstood.

As for PDP, it is interesting that the simplest form of the connections within the perceptual side of a control hierarchy is exactly a multilayer perceptron, so that from PDP you can find an existence proof--anything a multilayer perceptron can discriminate is potentially controllable. And that means any configuration is controllable in a three-level hierarchy (maybe it is four, but I think three). So even if PDP is used poorly under normal circumstances (and I have long thought it is, before I heard of PCT), nevertheless you can point to it to show that simple structures simply connected CAN control perceptions of arbitrary complexity.

The flip side of that is that if the PDP people ever, with any kind of node, demonstrate the possibility of making any kind of classification or recognition, you can immediately say that this is demonstrably a controllable kind of percept. So PDP provides an ever-rising lower bound on the known possibilities of PCT. PDP isn't a theory of behaviour, although people have used its possibilities that way (as you say, wrongly). But it does what it does, and you can use it without fear and trembling.

>I guess Martin is preparing a thesis on the value of information theory
>for understanding control. I'm waiting with great interest to see
>what have missed by ignoring iformation theory. I have read
>several rather unconvincing attempts to show that some versions
>of behaviorism are equivalent to control models of behavior. If
>anyone else out there has a non-PCT theory that they think provides
>a real great explanation of some aspect of behavior then I'd sure
>like to hear about it.

I hope you aren't putting me in with people offering a non-PCT theory. I thought that what I had shown was that information theory provided a demonstration of the necessity of PCT, but apparently I wasn't convincing. When I subsequently showed my posting around here, the reaction was that it made intuitive sense. So I have hopes that a more careful discussion will make intuitive sense to more people.

People don't necessarily bring in ideas that have proved useful elsewhere just because they have a great fondness for the ideas and don't understand that PCT stands isolated. There IS the possibility that PCT hasn't reached the limits of its potential.

Martin

PS. You may be amused to know that I have observed in myself much the reaction you often express when reading or listening to psychologists talking in areas I used to think important. "What stupidities are they thinking of...Don't they know it's all control?"

Date: Wed Dec 23, 1992 4:01 pm PST

[From Francisco Arocha (921223; 18:49)] Bill Powers (921222.0800)

>When I last looked, there were 132 subscribers to this list. Permit me a
>moment of impatience: when are some of you people going to get out of
>your armchairs?

Well, I guess I'm one of those 132 people in the list. I find it very difficult to do PCT research when one is interested in levels beyond the category. Anyhow, I've not given up and I plan to spend a large portion of the days between Dec. 28 and Dec. 31 in an intensive care unit, trying to make sense of physicians' behaviour in terms of PCT, at least to the extent that my knowlege permits me. Maybe not everything is so grim...

Have everybody a happy holiday and a productive next year!

PCTvely yours, Francisco

Date: Wed Dec 23, 1992 4:27 pm PST
Subject: Reflexes; descriptive vs generative

[From Bill Powers (921223.1530)] Wayne Hershberger (921223.0930)--

I am delighted that you're delighted with my remarks on conceptual and perceptual EVs (environmental variables). If you could ever bear to look back over our several years of discussions on these matters, you might now see what I was trying to say. Thank goodness I found a way that expresses my idea (assuming that my present idea is really the same one I started with!).

The VOR is not perfect; nor is the optokinetic reflex. I think the gain (eye-velocity/head-velocity) of these two reflexes combined is only about .9, meaning that during active head rotation of 50 deg/s, images slip across the retina at 5 deg/s.

OOPS. My abysmal ignorance catches me up once again. Please explain the difference between these two reflexes!

A slip of 5 degrees per second per 50 degrees per second of movement implies about a 10% error at the end of a movement, doesn't it? This is pure coincidence, but the rule of thumb I've been using to specify a "good" control system is a loop gain of "5 to 10 and preferably greater." A control system with a loop gain of 10 would allow a disturbance to have 10% of the effect it would have without control. Splendid.

Note that when an object in the visual field moves 90 degrees and you recenter it within 1 degree of the center of vision, the implied loop gain is 90. If you recenter it within the limits of optical acuity (say, 2 min of arc) the implied loop gain is 2700! So the combined reflex is only 1/9 to 1/270th as accurate as the visual centering control system.

Martin Taylor (921223.1440) --

I agree that it would be a good idea to give information theory a rest until you can work up your paper on it.

To me, a model is only a concise way of making descriptions. A generative model, as you put it, is distinct from a descriptive theory. To me, it is not distinct. It only uses a few more parameters to make a much better description, and if that description is

accurate, Occam's razor says that it is preferred. But if its precision allows the world to say "that's not right," it could be that the parameters are wrong. If the parameters can be changed and still lie within the bounds of "the theory", then the information contained in the parameter specification has to be included in the "size" of the description, so the case becomes less clear.

I think there are really two kinds of models. Consider the model contained in a schematic diagram of a radio, plus the theory of electronics that applies to the symbols in the schematic. Basically this schematic is a specification for interconnecting physical components having certain properties. In one part of the schematic there will be several tuned circuits consisting of an inductance, a capacitance, and some series or parallel resistance. Depending on the exact values in henries, farads, and ohms, the combined circuit will have a certain frequency response in terms of amplitude versus frequency. The shape of the passband measured this way is determined by the physical properties of the components, and nothing else (assuming no important loading by other circuits).

It follows that all abstract properties of the tuned circuits, such as their combined "Q", bandwidth, rise and fall time for a step input, and gain, are also determined by the properties of its physical components.

It is possible to describe the behavior of this part of the radio without reference to the physical components that comprise it. One could, for example, determine the bandwidth, rise-time, Q, or gain by observing how the circuit's output relates to its inputs. This would lead to a descriptive model of the circuit, cast not in terms of interactions among components, but in terms of behavioral measurements.

Mathematical relationships among the measurements thus found could be the basis for still greater generalization, for example input-output power spectra or various kinds of transforms: Fourier, Laplace, or z. And one could go to still greater degrees of abstraction and express the input-output relationship in terms of equivalent sampling frequencies, bit transfer rates, information capacity, and so forth.

This whole genre of representation is what I mean by descriptive models. They are models drawn from descriptions of whole-system behavior, either with or without experimental input to the system as a whole. They are all attempts to find simple invariants of behavior -- simple, that is, in comparison with the potential complexity of behavior of which the system is capable.

A generative model goes in the other direction from observations of behavior. In effect, it is an attempt to draw the schematic diagram of the system. It treats behavior as the outcome of more detailed processes, as a consequence of the interactions among components, no one component showing the behavior of the whole system, but the whole-system behavior being the necessary outcome of the properties of the components and their interactions. I guess the latest buzz-word for this is "emergence." From the standpoint of generative modeling, the behavior of the model, and presumably of the real system being modeled in this way, is an emergent phenomenon. In a generative model of a control system, there is no component that controls. Control is an emergent phenomenon.

A generative model is created by a feedback process. A model is constructed and made to behave. Its behavior is perceived in relation to the behavior of the real system, and the difference is noted. On the basis of the difference, the construction of the model is modified in a way that reduces the difference. The aim is to construct a model that produces outputs like those of the real system when presented with any possible inputs.

A descriptive model is generated by a process of induction, which is also a feedback process but operating at a different level. A generalization is proposed. The behavior of the real system is observed, and its fit with the generalization is noted. If exceptions are found, the generalization is changed until all cases of real behavior are covered by it. In psychology (and other fields), the generalizations are stated in statistical terms, and the measures of behavior are also subject to statistical representation. As a result, detailed deviations of the observed behavior from the generalization are averaged out, and only means, trends, and the like are compared. Therefore the process of generalization can arrive at an end-point even through individual instances of observed behavior depart markedly from the general representation of it.

A basic difference between these kinds of models is the degree to which imagination plays a part. A generative model begins as pure imagination. One imagines components which, if they really existed and really interacted as imagined, would produce behavior like the real system's behavior. A person making a generative model of the pass-band filter in a radio might imagine that there are four successive tuned circuits with a certain 'Q', or that there is a digital computer that creates an equivalent frequency response. The model would consist of imagined coils, capacitors, and resistors, or of a minimal microcomputer running a specific program. The model would be given a simulated input waveform similar to the waveform entering the real system, and its operation would then produce an output waveform for comparison with the output waveform of the real system. The differences in output waveform would be minimized by adjusting the variables in the model. If the resulting fit were within observational error for all possible input waveforms, the model would be accepted. Its components would be treated as real, and their values that produce the best fit with real behavior would given as the values of the critical variables. It is perfectly possible that the analog model works just like the digital one; in that case both have to be retained as viable alternatives.

When, as often happens with generative models, the real system becomes amenable to dissection, a further refinement of the model becomes possible. If the system, opened up, proves to contain nothing resembling a digital computer, and many components that show continuous input-output properties, then the version of the model with coils, capacitors, and resistors is chosen. Such detailed examination can show where the model is wrong: there might be, for example, five stages of filtering instead of four, and the assumed capacitances and inductances might prove to be generated in part or totally by active local feedback through amplifiers.

In short, even if the imagined model proves to be correct in the large, it is unlikely to be correct in detail. It can, however, easily be modified to become correct in detail, as far as the dissection allows. Detailed enough dissection might show that the coils, capacitors, and resistors of the model must be replaced with other physical components that have equivalent properties. But the development is always in the direction of more detail and more precision.

A descriptive model does not enter this world of imagined components and interconnections. It cannot, because its lowest level of abstraction is the observed behavior of the whole system, and it uses no imaginary components. It is, if you will, strictly empirical. It can lead to more and more compact descriptions in terms of broader and broader concepts, but at its base is always behavior itself. There is no possibility of arriving at greater and greater detail of explanation; in fact the trend is always toward less and less detail.

For me, the choice between generative and descriptive models is the choice between ever-more-precise prediction of behavior, and ever-more general characterization of it. It is the choice between understanding how the system works and making true but

non-predictive statements about the system's behavior. My choice is the generative model; I simply find it more satisfying than the other kind.

As to your work with Little Baby and Genetic Algorithms -- I definitely count that as a step toward making a generative model. How's your program for comparison of the results with real human behavior coming along?

Best to all, Bill P.

Date: Wed Dec 23, 1992 6:18 pm PST
Subject: Re: PCT Fanatics

[From Rick Marken (921223.1800)] Martin Taylor (921223 17:40) --

>Rick, would you accept a small revision--

>Not all of these examples are theories of behaviour, and so I might
>suggest adding to your first paragraph " ... ALL BE WRONG as applied
>to behaviour."

The Xmas tree is up, the logs are on the fire (to protect us from the fierce 50 degree temps outside) and I COMPLETELY AGREE WITH MARTIN TAYLOR. I thought of this after I posted and your amendment would strengthen my point considerably. There is nothing wrong with any of the theories I mentioned "per se" and in some applications they are very useful. It's the way they have been applied to behavior that's the problem -- all are applied to behavior under the assuption that behavior is caused output rather than controlled input.

Good point, Martin.

Regards Rick

Date: Thu Dec 24, 1992 3:12 am PST
Subject: Re: PCT Fanatics

[From Oded Maler (921224)]

* [From Rick Marken (921223.1300)]
*
* OK, nobody's in the office; I should be home cuddled up by the
* Christmas tree (yes, I prefer celebrating the apocryphal birth of
* the prince of peace to the non - apocryphal victory of the Macabees
* over the Assyrians [or whomever]).

I'm writing this before reading the rest of the message, although I can predict (with 95% accuracy) the rest from the title.. I hope I'll be able to respond after the Fois Gras festival is over.

I suppose the historical error was meant to be a trap, so in short my interpretation of Hanukka is the victory of mid-eastern spiritualism over helenic/western materialism (it were not the Assyrians but the Syrian/Greek part the post Alexander empire). The second incarnation of such a victory was the Khomeini revolution. But all that belongs to

domains were the PCT still does not have (and will not have, in this rate of development) generative models. I will also not enter in this son-of-a-god business.

So I want to wish you all PCTers, real or phoney, blind or deaf, armchair philosphers, experimentalists and rejection-record breakers, a happy new year. I don't know how many AHA's I had since I started reading CSG-L but I surely had a lot of HMMM's and I thank you for that.

--Oded

Date: Thu Dec 24, 1992 5:14 am PST
Subject: getting out of the armchair

I am one who says very little on this list, though I read as much as time will permit. My reasons are that I am not a PCT theoretician and I do not want to make comments that would not be useful in furthering the dialogue. However, if and when I learn enough, I will say more.

Just wanted to say that lurkers are not necessarily lazy or uninvolved.

AND... HAPPY HOLIDAYS TO ALL Eileen Prince

Date: Thu Dec 24, 1992 6:26 am PST
Subject: HAPPY HOLIDAYS TO ALL

##*#*#*#*# CHUCK TUCKER DECEMBER 24, 1992 #*#*#*#*#*#*

Dear CSGers,

I have continued to enjoy and learn from the posts on the Net and hope that smoneday to get our of my armchair and do something that amy prove to be useful to us all.

A question for the holiday: How does a person alter his/her theoretical view of human behavior? An answer to this may lead to some actions that could change the theories of human behavior for self and others.

Have a great and nondisturbing Holiday,

Regards, Chuck

Date: Thu Dec 24, 1992 8:22 am PST
Subject: Misc. blah

[From Bill Powers (921224.0845)]

Martin Taylor, Rick Marken

That's nice, don't fight.

It occurs to me that anyone who refuses to buy into your system is a considered a fanatic relative to whatever that person does buy into.

A reliable mark of a true fanatic is a complete lack of a sense of humor.

Remember: YOU are a fanatic. I am steadfast.

RE a private post:

Netters with Macs will be delighted to know that Martin's associate Chris Love is rewriting the Primer Series for Macs. The result, Martin say, will be put on Bill Silvert's server some time soon. I'll be adding to that series in January. Others are welcome to contribute "chapters" that make specific points.

To all who felt that my "armchair" complaint was somewhat unreasonable:

Well, it is unreasonable to ask people who are out in the world of applications to turn into basic researchers. In my post to Gary yesterday I tried to indicate what such people can do to TEST the theory every time it's applied. Figure out what the theory says SHOULD happen, and see what DOES happen.

I guess I'm asking people not to use PCT simply as another way of talking about behavior after the fact, another form of interpretation. If there is anything to PCT, it will stand up under tough scrutiny. If someone comes up with a criticism of PCT, the last thing you should do is defend it. That person might be right. What you need to do is find out if the criticism holds up. Don't be afraid to make predictions using PCT. Whether they work out or not, you'll learn something. When they work, you'll get a new jolt of confidence. When they don't, you'll discover, perhaps, something you hadn't understood about PCT, or you may discover something really wrong with the theory which we all would be grateful to know about. If you don't defend the theory you'll have nothing to lose however the tests come out.

One of our great difficulties with conventional science is the prevalence of devoutly held beliefs that scientists defend as if they've taken vows of loyalty to whatever subject they know the most about. This means that any suggestion that their own theory is wrong arouses an instant defense regardless of the merits of the suggestion. If humanly possible, we should avoid putting that kind of face on PCT. One way to do this is to treat the interpretations of PCT as if they have to prove themselves anew every time they're applied. Don't use easy tests; use HARD tests. You can do this even if you're not doing basic research.

Waiting for the rest of the offspring to arrive for a Christmas visit. It's possible that I may skip replying to posts tomorrow!

Best to all. Better times ahead. Bill P.

Date: Thu Dec 24, 1992 9:19 am PST
Subject: Re: PCT Fanatics

[From Rick Marken (921224.0800)]

Oded Maler (921224)--

>I'm writing this before reading the rest of the message, although I
>can predict (with 95% accuracy) the rest from the title..

How'd you do?

>I suppose the historical error was meant to be a trap

Nope. I actually consulted with my Catholicly trained wife before posting; her guess was Assyrian but she knew she might be wrong (hence I added the "or whomever"). Before meeting Linda, the Bible was just another book that I figured could probably be skipped over (like the 1932 World Almanac). But Linda told me all the great stories and I really liked them. Unfortunately, it quickly became clear that I didn't really "get" many of these stories. For example, to this day, I still don't get the story of Jacob and Esau (at least, not the way my Orthodox Jedwish step father gets it). To me, the obvious hero of the story -- the admirable person -- is Esau; Jacob is a cowardly, deceitful slimeball (like his mom). Obviously, it is possible to see Jacob as the admirable person in the story -- most people seem to. But I just can't. Those of you who haven't read it -- I recommend it. To me, the last scence rivals that in "It's a wonderful life". Jacob returns home (after screwing Esau out of his entire inheritance). He see's Esau's army lined up on the ridge and he's scared s**tless. So he hides behind the skirts of his wives as the majestic figure of Esau descends the hill towards him. Instead of chopping Jacob into little pieces (which even Jacob figures he has every right to do) he embraces his brother and welcomes. It brings a tear to my eye -- what a mensch, that Esau.

>my interpretation of Hanukka is the victory of mid-eastern
>spiritualism over helenic/western materialism (it were not the
>Assyrians but the Syrian/Greek part the post Alexander empire).

That's nice. I don't like celebrating Hanuka because the story is largely about a war victory. The way you put it makes Hanuka seem less militaristic, but it is still about a "victory" in a conflict -- in this case, a conflict of ideas. But I don't like celebrating any victory -- it suggests that a reasonable way to solve conflicts is for one side to win -- whether this winning is done by "intelligent debate", brow-beating or war.

I like Bill Powers' comments about war. War happens, and when it does you might as well do it as best you can. But realize while you are doing it that war represents a failure; a desperate, horrible failure of conflict resolution. And if you happen to be lucky enough to be on the side that "wins", don't celebrate -- mourn for the failure that the stupid war represents. Regret war; regret victories as well as defeats; they are both part of the same process. Devote yourself more fervently to thinking of ways to resolve conflict; be prepared with many, clever options; use your head to get above the conflict (psychologically) instead of figuring out new ways to defeat the enemy. It can be done -- just devote about half of the defense budget to paying people to think of ways to "go up a level" instead of perpetuating the conflict. It can be done if we stop celebrating victories (or mourning losses). That's why I like what I perceive as the spirit of Christmas; to me, the one great notion that comes out of Christian thought is the idea that "fighting back" will not solve problems. "Turn the other cheek" means that the best way to solve a conflict is to move above it (don't participate at the level at which the conflict is occurring) -- fighting back is participating. This is a truly magnificent realization -- of course, there are damn few Christians who actually practice this realization (far more of the "kill a commie -- I guess now it's kill a homo -- for Christ variety). But I think this non-defensive approach to dealing with conflict was a major epiphany that occurred around 1 AD and that's what I celebrate at this time of year; I'm certainly no Christian, but I do believe in what I think of as the spirit of Christmas -- non - defensiveness -- another name for which is "forgiveness". PCT shows why "forgiveness" (like 186,282 miles per second) is not just a good idea -- it's the law (at least, if you want to avoid the unpleasant side effects of conflict).

Best Rick

Date: Thu Dec 24, 1992 10:54 am PST
Subject: Good News

from Ed Ford (921224:1108) To All.....

I guess Bill Powers' recent post suggests we need each other regardless of what we're doing. I guess it is this recognition and respect for what each person does that is the strength of our CSG. Recently, I've received news which I wish to share with you.

The NASPA (National Systems Programmers Association) News ran a review of my book, Freedom From Stress. Have had some sales as a result. The reason was a local (Phoenix) programmer, Jim "JD" Drechsler, with American Express who has been promoting PCT and has gotten a lot of his peers excited about the ideas. He has spoken at several of their conferences and was recently elected to their national board.

The second is with regards to my TV program produced earlier this year by the local PBS station, KAET-TV. They've aired it three times and plan another airing in March. The first half of the program is dedicated (a lot) to explaining PCT. I just learned from the station's program manager that he presented my program for possible use by other stations at the annual conference of PBS stations in November. Apparently, 32 PBS-TV stations throughout the country expressed a desire for copies of the program to show in their own areas, which included Boston, Washington(CD), and Dallas, three of the nine largest viewing areas for PBS. Others areas include Denver, Atlanta, Indianapolis, plus San Jose, San Mateo and Huntington Beech in California, and Tallahassee and Daytona Beech in Florida, and so forth. March would probably be the next time stations have their promotions and would use this show, which is designed for such use.

May all of you have a holy and joyful holiday season. Ed

Ed Ford

Date: Thu Dec 24, 1992 11:07 am PST
Subject: Science letter

[From Mary Powers (921224.1200)]

The following letter appeared in Science for 11 Dec 1992. It
should be of interest to CSGnet.

Conflicts of interest

In his article "When does intellectual passion become conflict of interest?" (Special News Report, 31 July, p. 620), Eliot Marshall wrongly focuses on individual passion. Shared intellectual passions generate much more powerful conflicts of interest and are a greater threat to scientific progress. Enthusiasts for widely held ideas are in a strong position to promote their interests by advancing cherished, but flawed, theories. Consensus among many scientists is no guarantee against major errors in thinking. Lone thinkers have only the strength of their aruments behind them, yet sometimes their arguments prevail and lead to major advances. Proposals to censor unfavored ideas by invoking legalisms such as "conflict of interest" are alarming. Suppression of the opinions of scientists with strongly held, idiosyncratic points of view is profoundly

antiscientific. Individual intellectual passion remains essential for scientific progress.

Jerome L. Sullivan
Veterans Affairs Medical Center,
Charleston, SC 29401-5799

Date: Thu Dec 24, 1992 9:46 pm PST
Subject: Seeing the Loop

[from Gary Cziko 921223.1815 GMT] Rick Marken (921223.1300) says:

>For me, one of the most dramatic demonstration of this fact is given
> in my 'Cause of control movements...' experiment (included in Mind
>Readings). This experiment shows beyond doubt that THE STIMULUS
>in a tracking task IS NOT THE CAUSE OF OUTPUTS that control the
>stimulus; there is no cause-effect (where it seems that there must
>be) because the cause and effect are IN A LOOP. It's the loop
>that changes everything that has always been taken for granted in
>all previous approaches to understanding human nature.

I must admit that this short paper by Rick is my ALL TIME FAVORITE of demonstrations of perceptual control. I thought I understood the basics of PCT before I first read it and then when I realized that Rick's results seemed like magic I realized that I did not. Now when I read it, the results seem only 50% magic. Progress, but I still have a way to go (but I was happy that Rick himself admitted to me last July in Durango that he also finds the results somewhat mysterious).

I strongly recommend that anyone wanting to understand PCT read Rick's paper. So let me (as my Christmas gift to the net) remind people how they can do this.

1. Get the paper from your local academic library in the journal in which it as originally published:

Marken, R. (1980). The cause of control movements in a tracking task. _Perceptual and Motor Skills, _51_, 755-758.

2. Buy the collection of Rick's papers published as _Mind Readings_ available from The Control Systems Groups, 460 Black Lick Road, Gravel Switch, KY 40328. Price is $18 postpaid. (I believe that Greg Williams told me he will send the book anywhere in the world for that price, but you should check with Greg <4972767@mcimail.com> first about this for orders coming outside the USA). This also reminds me that I was going to try to devise a convincing portable "manual" demonstration of what Rick does in this paper. I'll keep the net informed if I come up with something useful.--Gary

P.S. Rick, why do I keep seeing ? in your posts where single and double quote marks should be"""

Gary A. Cziko

Date: Fri Dec 25, 1992 4:15 am PST
Subject: Solsticial celebrations

[From Bill Powers (921225.0500)]

Merry Christmas to those who celebrate it for various reasons; to the rest, the days are getting longer up here in the Northern Hemisphere, so cheer up. You upside-down antipodeans, I am afraid, must wait six months for your solsticial celebrations. Anyway, Venus is in the evening sky for everyone orbiting our common luminary here and farther out and Orion looks the same, upright or standing on his head, to all Solarians. We have more in common than we think, whatever we call it.

Ed Ford (921224) --

Congratulations on all your good news. I see that Denver is on the list for airing your PBS program, so we'll get to see it out here in the boondocks. You should have little difficulty enjoying your holiday.

Best to all, Bill P.

Date: Fri Dec 25, 1992 3:00 pm PST
Subject: ALTERING ONE'S THEORETICAL VIEW OF HUMAN BEHAVIOR

I think one alters one's theoretical view by finding a(nother) theory that fits the data better than the one previously held or better than the atheoretical view that one has been taking. Naturally, a knowledge of a theory that works will probably speed up this process. Naturally I speak of the open-minded.

Just call me a die-hard empiricist, but not one that is atheoretical.

HAPPY CHRISTMAS TO ALL AND TO ALL A GOOD NIGHT.

Eileen Prince/Northeastern University

Date: Fri Dec 25, 1992 3:03 pm PST
Subject: Re: Misc. blah

To the limits of my knowledge, and these are great, I am in fact trying to apply PCT; however, it doesn't always help with those who are not of a PCT bent themselves. If you want me to elaborate, ...

MERRY CHRISTMAS, EILEEN

Date: Sat Dec 26, 1992 3:41 am PST
Subject: Re: PCT Fanatics

[From Oded Maler (921226)]

Instead of entering into a biblical discussion I'll answer the rest of Rick's original post about fanatism. Your main problem is not being able to go up one (or several) levels above your background as an ex-"scientific psychologist" and realize that some people are simply asking different questions or have different scientific goals than "building a predictive theory of human behavior". So it's no use attacking this or that person/theory immediately each time you detect the he/she/it has not realized what behavior is, etc. People who are doing, say, mathematics of non-linear dynamics are interested in general properties of some systems obeying certain rules. Period. Although some others may try to

apply this math to psychology by using the wrong (i.e., non-PCT) model of behavior, it does not mean that some fundamental truths about such systems are not relevant and will not be needed when more complex PCT models will be built. The same is true with info theory, which, if I understand Martin's intentions, has implications to every situation where information passes along a complex network. I think he is trying to answer the question "what in the structure of the world enables a system organized according to HPCT architecture, survive". If we take your favorite mathematical formalism, linear algebra, does the fact that arithmetical operations are also used in analyzing behaviorist models, make them uselss in modeling?

The same goes with your outrage toward roboticist until you realized that they have other (orthogonal..) goals, namely to build toys and not to analyze existing living systems. The same goes with Braitenberg, whose main occupation is experimental neurobiology and his little "vehicles" book was just written for fun and speculation (and yet it has some interesting ideas, including the imagination-loop idea used in planning) and not as a serious suggestion of an all-embracing theory of behavior.

Your observation on the trendiness of science are correct, and I also buy your observations on the non-foundedness of most of Psychology as a science (thank god I resisted the temptation to quit my CS B.A. studies and move to psychology, now I know that I did not miss anything), but please remember that there are other questions and goals. The world (at least not all of it) does not turn around the PCT-non-PCT controversery in the explanation of human behavior. This hard fact do not under-determine the *objective* beauty and power of PCT, nor its importance as a stage in the development of human understanding. But not realizing this, and classifying all the rest of the world as "us" and "them" might lead an untrained observer to perceive a peace-loving other-cheek-turner as a fanatic.

On another topic, I'm reading Sacks' "The man who mistook his wife for a hat" and although apparently the author does not know that ..., I think it is really worth reading. It might be intersting to try to give PCT crude explanations of the phenomenon he describes.

--Oded

Date: Sat Dec 26, 1992 7:08 am PST
Subject: PCT and other disciplines

[From Bill Powers (921226.0700 MST)] Eileen Prince (921225) --

>To the limits of my knowledge, and these are great, I am in
>fact trying to apply PCT; however, it doesn't always help with
>those who are not of a PCT bent themselves. If you want me to elaborate, ..

We are birds of a feather. By all means, elaborate.

Oded Maler (921226)--

I do like your sensible and realistic comments. Of course not everyone is interested in PCT. And of course play is necessary -- doing things that have no immediate application, just to enjoy the truth and beauty of whatever one can discover.

>People who are doing, say, mathematics of non-linear dynamics
>are interested in general properties of some systems obeying
>certain rules. Period. Although some others may try to apply
>this math to psychology by using the wrong (i.e., non-PCT)
>model of behavior, it does not mean that some fundamental
>truths about such systems are not relevant and will not be
>needed when more complex PCT models will be built.

This is all true. I trust you aren't saying that ALL of the studies of arbitrary systems will prove to be relevant and applicable to understanding human behavior.

>The world (at least not all of it) does not turn around the
>PCT-non-PCT controversery in the explanation of human behavior.
>This hard fact do not under-determine the *objective* beauty
>and power of PCT, nor its importance as a stage in the
>development of human understanding. But not realizing this, and
>classifying all the rest of the world as "us" and "them" might
>lead an untrained observer to perceive a peace-loving other-
>cheek-turner as a fanatic.

A little fanaticism is appropriate if it's limited to the boundaries of the PCT-non-PCT controversy. You need some kind of support structure when there are so many people who look on your views with disdain, condescension, and irritation, apparently believing that this is how science is supposed to work.

>On another topic, I'm reading Sacks' "The man who mistook his
>wife for a hat" and although apparently the author does not
>know that ..., I think it is really worth reading. It might be
>intersting to try to give PCT crude explanations of the
>phenomenon he describes.

The literature of mental malfunction must be rich with possibilities for the furtherance of HPCT. To make use of it, however, there must be people willing to sort through the mountains of information available to look for dependencies among perceptual processes and begin the enormous task of drawing the map we need. A large obstacle is the fact that the behavioral deficits that have been found have been characterized without any coherent model in the background. We need a systematic approach to this subject with model-relevant experiments used for diagnosis instead of rather casual subjective impressions of what is wrong. It may be that even with all that data available, the facts that we need to know simply have not yet been observed.

>The following is a cross-posting from the Control (in the
>mathematical engineering sense) mailing list. It contains
>titles of all papers in the subject published recently. Just
>for information I don't claim anything will be relevant.

If you wanted to make me feel ignorant, you certainly succeeded. How I wish that I could understand all that stuff! The next great leap forward in PCT is going to be generated by a person who is comfortable with those advanced mathematical treatments, AND who has a clear idea of the phenomena of behavior that need to be explained. That person hasn't appeared yet, and probably won't until the basic concepts of PCT have been accepted widely enough that a person could devote a career to it.

I would love to write a paper for journals like these explaining what we are trying to do with PCT and how people with such great talents could contribute to the work. But such a paper would have to be written by someone who speaks the language; anything I wrote would be considered too simple-minded even to be interesting to the readership. PCT needs translators; people like Gary Cziko and Hugh Petrie in education, and McPhail, Tucker, and McClelland in sociology, and Robertson and Goldstein in psychotherapy, and Ford in counselling and social work, and Nevin and Andrews (and more) in linguistics, and Forssell and Soldani in management consulting, and Martin Taylor and the various Gangs (of 1, 3, or 5) in the design of complex systems, and Cliff Joslyn and his cohort in cybernetics, and Rick Marken in (now) human factors, and Tom Bourbon in neuropsychology, and all the rest who have a foothold in two worlds, one of which is PCT.

The world of psychology seems almost closed to PCT, but psychologists are not the only ones who are trying to understand human nature. PCT can spread to other disciplines, and is doing so. In every case, however, this spread has been none of my doing, but the doing of others who can take the basic ideas to their own colleagues and explain them in relation to the interests of those other disciplines. There always remains the problem of displacing the old concepts of human behavior, traceable mostly to conventional psychology and biology, but this is done most easily by people who grew up with those ideas and understand how they look to those who have adopted them.

We lack biologists and biochemists and control-system engineers, among others. Maybe these, along with psychologists, are the toughest nuts to crack because of the direct contradictions involved in biology, and the implied competition in control engineering. If anyone knows people in these fields who might be willing to join in, by all means try to recruit them.

And you, Oded. Are you all alone in your appreciation of the concepts of PCT? Do you have any colleagues who show any interest?

Best, Bill P.

Date: Sat Dec 26, 1992 7:27 am PST
Subject: Re: PCT and other disciplines

Dear Bill,

Thanks for the quick comment and invitation. It feels good to have someone pay attention to what I say and/or ask.

I will write briefly for now, as my (autistic) daughter Katy is home for the holidays and I both need to supervise and want to be with her.
Also,
I hope you and yours are having a wonderful holiday season.

I had two thoughts in mind when I wrote my comment:

1. In working with those who have minds that are closed to PCT and any theory other than the one they dogmatically follow, I have found it difficult to introduce new ideas. This goes for the ESL teachers I work with and supposedly supervise and others who "manage" within our program. It also goes for the educators who work with Katy and are of the B-Mod persuasion. However, with these latter I have found it useful to use their terminology as much as possible and praise them for the wonderful successes that have in fact taken place. As for getting more control FOR Katy, it's hard, but I think my deliberate attempts to make them think that I partially agree with their approach and at

least understand it have given me sympathetic ears. There has been far less success with many of the ESL type.

2. In trying to operate from a PCT approach myself, I find again that the closed mind is the greatest "enemy." (Naturally, I can't comment on the degree to which my own mind is closed :-).) Let's say I choose to think of my current spouse or one of my daughters in a certain way, trying to avoid the stereotypes that I naturally grew up with. (And, believe me, those have changed anyhow!) The other person may have already neatly categorized me as wife, mother, etc. and found me to fall short of the definition. What to do? Say fine, this is what I am, take it or leave it. (Often my response, though not overtly.) Or to try to change their perceptieon in other ways, and then how?

(It's probably a mistake to try to change someone's perceptions. I can probably describe what is going on better than affect it, except in myself. Then, just because I am perversely human, I don't choose to easily end relationships just because the other does not perceive me as I am. Though perhaps I eventually do/have. But with my children? Completely?)

Happy Holidays again, Eileen

Date: Sat Dec 26, 1992 8:19 am PST
Subject: Re: PCT and other disciplines

Apologies to all. I had meant my last posting to go to Bill but sent it to the entire list instead.

Eileen

Date: Sat Dec 26, 1992 12:12 pm PST
Subject: Eileen, Theory

[From Dag Forssell (921226 12.00)] Eileen Prince (921226)

>Apologies to all. I had meant my last posting to go to Bill but sent it
>to the entire list instead.

This is what the list is for. Bill is not the only one who will appreciate your thoughtful post. If you want to apologize, do so for EVEN THINKING of depriving the rest of us of your contributions.

By the way, have you taken the time to read Ed Ford's book "Freedom from Stress?" What did you think of it? Did you find it helpful? How can such an introduction be improved? What questions (if any) does it open but not answer? Ed and I would like to know.

With reference to the discussion between Rick and Martin, as well as Bill's and Oded's postings, I would like to resubmit my chart of Dec 14.

What does "THEORY" mean to you?

APPROACH TO THEORY	LEVEL OF SCIENCE	TYPE OF KNOWLEDGE

Type 1	Hunch, expectation based on experience. Intuitive / Formal	Common sense / Statistical research	What works (sometimes?)
Type 2	Explanation, prediction, test.	Engineering science	Why it works (always?)
Type 3	Logical reasoning.	Abstract science	Abstraction

Continued to the right.

	METHOD OF LEARNING	TIME TO LEARN	PREDICTION CAPABILITY	RESULTS
..1	Trial & error / data collection	Long	Poor	Spotty
..2	Create theory, test theory	Short	Excellent	Confident
..3	Deduction	Short	Depends on fit with type 2	Depends fit with type 2

Cause-Effect is in the category of an Engineering science. Type 2. Information theory etc. is in the category of Abstract science. Type 3. Control Theory is in the category of an Engineering science. Type 2.

The abstract sciences (math, geometry, information theory, etc., etc.), have NO connection to Boss REALITY. They cannot be tested, since they are logical constructs ONLY. Their "validity" and usefulness depends ENTIRELY on their application in support of a type 2 theory, which DOES have connection with Boss REALITY.

The Cause-Effect engineering theory has failed more than once, but even though the experimenters have read Karl Popper, they WANT to interpret their "data" as supporting their theory. As Jean Luke Picard of "Star Trek" fame so appropriately commands when someone has made a suggestion: "Make it so!" "(Control for that perception)!" - "Proof" is found. (Ref: "Skinners mistake" by W.T. Powers, CSG-l March 3, 1991)

As Rick and Martin now agree, it is the application of an abstract theory (about which one otherwise can make NO value judgements) to a FAILED engineering type theory, that created at least some of the previous disagreements. (Certainly, the abstract theory being applied to the engineering theory has to be appropriate to be useful).

What is needed above all is sound engineering theory. When it comes to behavior, it is called PCT. If and when PCT fails once, we will not ignore Popper, but modify PCT.

In the spirit of the season, I too wish everyone a time of appropriately chosen perceptual references, allowing yourself and others enough perceptual degrees of freedom, effective action on whatever Boss REALITY is out there and satisfying perceptions of it.

Your choice: It's all perception / It's all control / All of the above

Best, Dag

Date: Sat Dec 26, 1992 6:25 pm PST
Subject: Re: Eileen, Theory

To Dag and all:

Thanks for the encouragement.

I have read about a third of the book and hope to finish it this vacation. So far it makes sense and I am both enjoying it and trying to learn from it. I have had no trouble with the idea that we have control over our perceptions of others nor with the idea of dare I call it archetypes (though not necessarily Jungian in the sense of being the same for all people -- I think that was part of his theory) that are a kind of baggage that may govern our perceptions if we don't see beyond them. I have to a great extent "believed" these ideas since long before my exposure to PCT. That's part of what appeals to me so much about the theory.

Here's a thought if someone really wants to proselytize. How about an article for a magazine like COSMOPOLITAN, something with a header like: "If You Think He's Prince Charming, It May All Be in Your Head: How PCT Can Help in Evaluating Relationships." I say this not at all tongue-in-cheek. I strongly believe in popularizing theories within pop culture. Then they may be taken more seriously be "scientists."

Are there any takers? If not, I just may write the article myself, though I won't have time until the second half of the new year because of my ESL text writing schedule, etc.

Hope you really want my contributions on the list, because now I fear I've just gotten started.

Here's to a fruitful new year, and thanks to all.

Eileen

Date: Sat Dec 26, 1992 8:22 pm PST
Subject: Off time

My system's vaxes are going to be down from Sunday to Wednesday. See you all then.

Best, Bill P.

Date: Sun Dec 27, 1992 3:26 am PST
Subject: Re: Good News

To Ed:

I hope you will jog our memories around the date of your program's airing. I will encourage as many as I can to watch it.

Also, the book review idea is an interesting one. I think one of the newsletters I get from TESOL, my professional organization, has a section for reviewing books which are not on ESL but of interest. I'll look into it as soon as I finish reading the book.

Best, Eileen

Date: Sun Dec 27, 1992 3:47 am PST
Subject: Re: Misc. blah

Bill's recent posting on defending theories reminded me of an incident that took place when I was a doctoral level grad student at Harvard (in linguistics, still ABD). My area of specialization was/is discourse analysis, and I had written a paper, I believe on article use, though I'm not positive at this time (how soon we forget when removed from the context!). In the paper, I had made the claim that my theory accounted for most of the data but that there were certain data which it did not adequately describe. I then gave examples of that data. My adviser, who I still respect in many ways, chuckled. He told me to take out that part and to never admit in a paper or publication that a theory of mine did not work completely. He said that if I felt compelled I could mention the deviant (recalcitrant?) data in a footnote, but that even that was not necessary. He said to wait till others pointed out what the theory did not account for and then to respond.

This was "only" linguistics, love it though I do. If this type of stuff goes on in the hard sciences, ... Speaking of which, I think that Richard Feynman tried to debunk the system, but he of course had already made his reputation. Remember the rings on the rocket or something like that?

Best again, Eileen

Date: Sun Dec 27, 1992 5:23 am PST
Subject: Re: generative approach

In theory pure generative approaches are subject to the checks recently outlined. However, the latest version of Chomsky's theory with which I am familiar allows for this to be gotten around. The generative system is allowed to overgenerate based on universal rather than specific language-specific rules. Then, whatever does not apply to language X is filtered out through the use of what really amounts to cannonical forms. If something that does not fit these forms/constraints which are language specific, it is discarded even though it has been generated. Perhaps later versions of the theory (I think I'm going back to Government and Binding, though there may be a bit of Logical Form here too) deal with such theory-based problems differently.

I am reminded in this context of Bill Labov's comment on the Chomskyan approach back in the mid 60's. He said that "they" would claim universal applicability for their theories and then offer to prove it by saying "Let's take any language. Hey, how about English?"

The point of all this is that when pre-conceived models take over good theory goes out the window as I feel it has with Chomskyanism. Hope this has been relevant.

Best, Eileen

Date: Sun Dec 27, 1992 6:28 am PST
Subject: Generative models vs generative grammar

[From Bill Powers (921227.0700)]

Trying to squeeze another post in before the system goes down for three days.

Eileen Prince (921227) --

Omigod, another usage for "generative" that I had overlooked.

>In theory pure generative approaches are subject to the checks
>recently outlined. However, the latest version of Chomsky's
>theory with which I am familiar allows for this to be gotten
>around. The generative system is allowed to overgenerate
>based on universal rather than specific language-specific rules.

Chomsky's generative grammar is not a generative model in the sense being used on this net. It does not produce a behavior, out of its own rules, that can be matched moment by moment against the behavior of a real human speaker. As far as I know, Chomsky's model doesn't produce any behavior at all: it produces an analysis, a structure, to which speech is supposed to conform, or of which specific instances of speech are supposed to be valid examples. People like Avery Andrews who are madly writing programs that use the principles of generative grammar are trying to supply generative (my meaning) models in the form of computer programs that will in fact generate realistic human utterances.

In fact, Chomsky's system is really a generalization, an abstraction. This is one of the difficulties I see in it (dimly, not being a linguist). The forms that this grammar produces are not specific instances of speech, but classes to which instances of speech are supposed to belong (like noun phrases). How does a specification for a class produce the specific muscle tensions that will produce a pattern of utterances that happen to belong to that class? The fact that there is an infinity of utterances that would qualify means that this can't be a pure top-down system for generating speech -- there simply isn't enough information in the name of a class to pin down the details to a unique utterance.

This WILL work, however, if the forms of which Chomsky speaks are considered to be perceptual patterns, not output patterns. Now, in order to produce a sense that a noun phrase is being uttered, all that is necessary is to produce any specific utterance that is perceived as belonging to this class. In other words, generative grammar can actually work only if for "generative" we substitute "perceptual." A person does not generate general output forms that are elaborated into more specific instances. It's the other way around: the person generates specific instances such as to create a perception of a more general form. In short, an HPCT model of this concept of grammar will work, but a top-down model will not.

To make a generative model of this process (our meaning), it would be necessary to design the actual control systems with the perceptual functions implied, and run them to produce real utterances.

Your comments on defending theories are right on the mark.

Best, Bill P.

Date: Sun Dec 27, 1992 12:18 pm PST
Subject: good news

from Ed Ford (921227:1300)

To CSGnetters who've expressed an interest in my TV show for their area, here are the other stations (not mentioned in my 921224 letter) who've expressed an interest in using my show. SOCA (S. Carolina); GEOR (Georgia); KENT (Kent, O); CONN (Conneticut); OREG (Oregon); ALAB (Alabama); IDAH (Idaho); ARKA (Arkansas); WNED (Buffalo, NY); KCPT (Kansas City, Mo.); WHRO (Norfolk, VA); NDAK (N. Dakota); WXXI (Rochester, NY); WCVE (Richmond, VA); KUAT (Tucson, AZ); WMEA (Portland, ME); KUSH (Bozeman, Montana); KTWU (Topeka, KS); WVPT (Harrisenberg, VA); (KEET Eureka, CA); and WNMU (Marquette, MI).

Usually, most spring promotions are done in March. Best to call your local station and ask for the program manager. You might mention the show's name (Love Guaranteed With Ed Ford) and the station that produced the show (KAET-TV, Phoenix). Again, these stations expressed an interest in using the show, it doesn't mean they will definitely use it. Those interested in viewing a commercial copy of the show, it is available from Brandt Publishing (my address below) to CSGnetters for $10 (half price) plus $2 shipping.

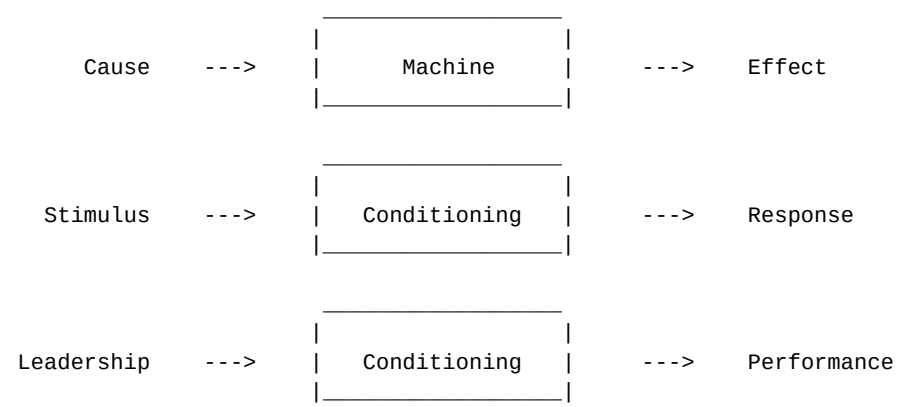
Ed Ford ATEDF@ASUVM.INRE.ASU.EDU
10209 N. 56th St., Scottsdale, Arizona 85253 Ph.602 991-4860

Date: Sun Dec 27, 1992 12:21 pm PST
Subject: Theory, Ray, Eileen

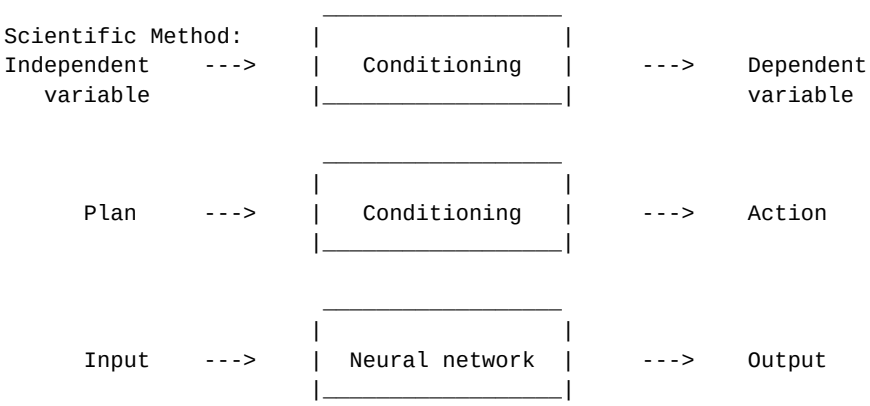
[From Dag Forssell (921227 12.00)]

More on theory. As part of an introduction, I would like to briefly show people the prevailing (inadequate) use of the Cartesian dualism. Here is my draft. Comments solicited.

EXISTING THEORY
The Cartesian dualism



Conventional wisdom:
When the response is not what you want, change the stimulus or leadership or work on the conditioning.



This scientific approach is entirely appropriate where the phenomenon under study is in fact a cause-effect mechanism.

Ray: I look forward to your edit suggestions on the Deming paper. Snail mail has not delivered as yet what you mailed last weekend.

Eileen: Appreciate all your comments. I think my linguist/ESL daughter might take an interest in your posts. I am thinking about a compilation from your beginning. When did you first post about autism? Last March?

Best to all, Dag

Date: Mon Dec 28, 1992 6:26 am PST
Subject: RE: PCT Fanatics

Rick,

As you probably saw, I was NOT on the net during the break. Just logged on and read your post from the 23rd first. I will send a more detailed reply after reading the rest of the mail, but for now:

1. It was amazing, but true to form, to see Martin retreat all the way back to, "I'll think about it and say more if I think it is worthwhile, and by the way it probably won't be what I said when I told all of you I knew more than you did." I relly don't know what he is up to, except for ego-puffing.

2. Your rock em, sock em post to Martin was hot, but not off the wall. He had muddied the discussion with notation and wording that confounded several key points, then he had failed to point out where and why he did that.

3. The levels business does seem to help, doesn't it. If that was what you used before your calm and tranquil post in follow up to Martin, the clinicians and counselors can use you as a case study!

4. I am curious as to whether Wayne replied to my comments. I will look for something from him in the rest of the mail, but he probably was down for the break, also.

What is this stuff on PCT fanatics going to be about, I wonder? Better go look.

!Bienvenidos a la semana nueva! Tom Bourbon

Date: Mon Dec 28, 1992 6:31 am PST
Subject: RE: Science letter

Mary,

Great letter from Science. I had missed that one. Sullivan went straight to the heart of it.
Love, Tom

Date: Mon Dec 28, 1992 7:09 am PST
Subject: Re: Seeing the Loop

[From Rick Marken (921226.1500)] Gary Cziko (921223.1815 GMT) --

Thanks for the nice comments on the "Cause of control movements.." paper.

>P.S. Rick, why do I keep seeing ? in your posts where single and double
>quote marks should be"""

This happens when I post using Microsoft Mail on my Mac. I don't understand why -- but I see it when I look at the mail delivered (from me) to my other address. I believe I'm using the appropriate terminal emulation (vt100) -- and I see """, not ? when I create my posts. It's quite annoying; I guess I could just avoid typing """ when I post using Microsoft Mail. Any other suggestions would be welcome (though probably most appropriate if posted directly to me).

Best regards Rick

Date: Mon Dec 28, 1992 7:48 am PST
Subject: Re: Eileen, Theory

From Tom Bourbon (921228 9:23) Eileen Prince (921226)

It looks to me as though you are EXACTLY the person to do that article for COSMO. That would move COSMOPOLITAN way up on my list, over some of the things I read and skimmed during the extended weekend for the holidays. If you want to get an idea of what behavioral ald life science are like without the realization that living things are in a high-gain negative feedback interaction with their environment, try these two recent collections:

American Psychologist, Vol. 47, No. 11, November 1992. The entire issue is devoted to the topic, "Reflections on B. F. Skinner and Psychology." The 22 articles, an introduction, and a historical note make this a rich source for anyone who needs up-to-the-minute evidence that radical behaviorists say the things we (would-be PCT authors) attribute to them -- reviewers like to tell us our attributions are incorrect. (By the way, one of the denizens of CSG-L is co-author of one of the articles, and a nice one it is! Dennis J. Delprato & Bryan D. Midgley, Some fundamentals of B. F. Skinner's behaviorism," pp. 1507-1520. This is a nice review exactly what they say it is -- many of the fundamentals.)

Behavioral and Brain Sciences, Vol. 15, No. 4, December 1992. An issue on "Controversies in neuroscience I: Movement control. Gary Cziko mentioned this one a few days back, but I just had to see for myself. Not one mention of PCT or anything close -- well, there are a few commentaries that come a little close, but not very. And the first of eight target articles is by Bizzi, Hogan, Mussa-Ivaldi, and Goszter: "Does the nervous system use equilibrium-point control to guide single and multiple jointt movements?" Bill and Greg -- when you re-submit your manuscript on ARM-LITTLE MAN, I am certain this reference, and its long list of commentaries in B&BS, will be thrown back at you. Better look at it and mention it.

All in all, these two thick collections gave me a renewed appreciation of the degree to which the idea of high-gain negative feedback control has penetrated the behavioral and life sciences -- not one bit.

A few days ago, I posted, "May Boss Reality tread lightly on the controlled variables of you, and of those you love." I didn't expect to find my own CVs stomped flat!
(Bizzi's coauthor is Giszter, not Goszter.)

Until later, A two-dimensional Tom Bourbon

Date: Mon Dec 28, 1992 10:40 am PST
Subject: e-mail address change

Please be informed that my email address has changed.

It was: goldstein@saturn.glassboro.edu

It is: goldstein@saturn.rowan.edu

The reason is that the college changed it name after Mr. Rowan donated 100 million dollars to it.

David Goldstein

Date: Mon Dec 28, 1992 11:20 am PST
Subject: Re: PCT Fanatics

[From Rick Marken (921228.0900)] Oded Maler (921226) --

>Your main problem
>is not being able to go up one (or several) levels above your
>background as an ex-"scientific psychologist" and realize that

>some people are simply asking different questions or have different
>scientific goals than "building a predictive theory of human behavior".

I don't believe that I have ever taken issue with people whose scientific goals are not obviously relevant to PCT. When have I done this?

>So it's no use attacking this or that person/theory immediately
>each time you detect the he/she/it has not realized what behavior
>is, etc. People who are doing, say, mathematics of non-linear dynamics
>are interested in general properties of some systems obeying certain
>rules. Period.

Perhaps I have not been clear; I have no beef with non-linear dynamics as an area of study per se (nor with any of the other examples of what I have called "trendy" theories). I just assumed it would be understood (from the nature of this list and from the context of the posts) that I was taking issue with the theory or formalism or whatever when it was being proposed as a MODEL of some aspect of purposeful behavior.

>Although some others may try to apply this math to
>psychology by using the wrong (i.e., non-PCT) model of behavior, it
>does not mean that some fundamental truths about such systems are
>not relevant and will not be needed when more complex PCT models will be built.

OF COURSE. But this list that is about PCT and purposeful behavior (control). There is no rule against posting interesting findings about the behavior of non-linear dynamical systems or whatever. But I can't imagine that there will be too many people who will be that interested in those findings per se unless their relevence to modelling and understanding purposful behavior is fairly clear NOW.

>If we take your favorite mathematical formalism, linear algebra,
>does the fact that arithmetical operations are also used in analyzing
>behaviorist models, make them uselss in modeling?

Of course not -- and the tools themselves are important, of course. I don't believe I have ever criticized the tools themselves -- why would I? I only know about most of these tools because of the efforts of various people to use them as models of purposful behavior; in that case, the people who use these tools have goals that are the same as mine and I think they are trying to achieve that goal in the wrong way; so I criticize the tool as a MODEL OF BEHAVIOR, not as a tool per se. I don't criticize statistics per se, for example, just because I think it is generally irrelevant to understanding the purposeful behavior of individual organisms.

>The same goes with your outrage toward roboticist until you
>realized that they have other (orthogonal..) goals, namely to
>build toys and not to analyze existing living systems.

I don't know about "outrage"; when roboticists try to build purposful systems using the wrong approach, then I think it's OK to say what's wrong. When the goals of the roboticist (or anyone else) are truly orthogonal to those of the PCTer then there is no disagreement. But sometimes "orthogonality" is in the eye of the beholder -- the cry of "orthogonality" can be used to stifle legitimate dialog. For example, you say:

>The same
>goes with Braitenberg, whose main occupation is experimental
>neurobiology and his little "vehicles" book was just written for

>fun and speculation (and yet it has some interesting ideas,
>including the imagination-loop idea used in planning) and not
>as a serious suggestion of an all-embracing theory of behavior.

Does this mean that it is not legitimate to show that Braitenberg has ignored the fact that his vehicles actually operate by controlling sensory variables? If Randall Beer said that he was just building bugs for fun should we have just assumed that he was using the right approach? If roboticists are just trying to build machines that produce particular (purposeful) results using inverse kinematics, should we say nothing just because this is what they want to do -- ie. they want to figure out faster, more efficient algorithms for doing inverse kinematics -- even though we know that output generation is, in itself, the wrong approach to producing purposeful results?

If a person just cares about the tool per se (info theory, statistics, detection theory, non-linear dynamics, transformational grammars, etc) then I have no beef -- never have, never will. When they start applying the tool as an explanation of some aspect of purposeful behavior, then I feel like it's OK to criticize the use of that tool from a PCT perspective.

The life sciences have used the plea of "orthogonality" for years as a means of avoiding a confrontation with PCT; they just say that purpose is orthogonal to what they are about. Pardon me if I disagree.

Best Rick

Date: Mon Dec 28, 1992 2:13 pm PST
Subject: Apologies

From: Tom Bourbon (921228 15:50 CST)

This is a public apology to Martin Taylor and to everyone else on CSG-L. A short time ago I logged on to post a direct note, following up on another I had sent early this morning. I learned that the earlier private post was not so private after all. I apologize to all of you, but most of all to Martin.

In the direct post I intended to send this afternoon, I was going to comment on the quality of the mail that had accumulated during the holiday. I was especially impressed by the richness and depth added to discussions of PCT by the presence on CSG-L of so many different perspectives that were not included at the start. In the holiday mail, those "new" perspectives were represented well by Oded, Eileen, and Martin. From time to time, when someone comes in with a different interest in PCT there is smoke and fire, at least for a while, but the result is a deeper discussion on the phenomenon of control and on PCT.

Those were ideas I intended to explore in a post. But my earlier flippant and hurtful post did more to damage the process I wanted to praise than I care to imagine.

With apologies to all,

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Date: Mon Dec 28, 1992 2:16 pm PST
Subject: Purpose and Behavior

[From Rick Marken (921228.0800)]

Here is a little gem I found this morning in the PSYCHOLOQUY Newsletter. The Newsletter comes out at what appears to be random times (Poisson distribution of intervals between posts?) with news of interest to psychologists (like myself, before I stopped understanding it). This is one news item from the current edition of the Newsletter (posted Fri, 25 Dec 1992). I think it speaks for itself:

From: CATANIA@UMBC2.UMBC.EDU (A. Charles Catania)
Subject: (7) Query Response: Quotable Quotes

I am submitting the material below in response to R. Allen Gardner's request for a quotation. Ordinarily, I expect that such things would go directly to the requester, but this case seemed to me to have sufficient intrinsic interest that I thought it might be more appropriate to distribute it more widely through PSYCOLOQUY. I have not yet sent a copy to Gardner, but plan to do so as soon as I hear from you as to whether or not this seems appropriate for PSYCOLOQUY. I have tried to set this reply up in a format similar to Gardner's.

Quotation on Describing Behavior in Terms
of Purposes, Intentions, or Goals

R. Allen Gardner has requested a quotable quote to the effect that it is necessary or correct to describe behavior in terms of purposes, intentions, or goals. The material offered below fits the suggested time frame (since 1960, and preferably later), it is by a prominant psychologist (though I will make no claim about respect by cognitive psychologists), and it seems clearly relevant. It is among my favorites, mainly because it so clearly makes the point that events that have not occurred yet cannot affect current behavior. Consistent with its author's concern with the origins of our self-descriptive language, its purpose (sic) was not to eliminate the language of purposes, intentions, and goals, but rather to suggest constraints on the functions of that language within a scientific account. Whatever Gardner's final choice of quotable quote, I hope it will be one that is consistent with what follows, in the sense that it would be inappropriate for cognitive psychologists to rally around a quote that espoused a teleological and therefore scientifically untenable characterization of these important human concepts.

"An attempt has been made to solve the problem by creating a prior surrogate of a given effect. A quality or property of purpose is assigned to behavior to bring 'what the organism is behaving for' into the effective present; or the organism is said to behave in a given way because it intends to achieve, or expects to have, a given effect; or its behavior is characterized as possessing utility to the extent that it maximizes or minimizes certain effects. The teleological problem is, of course, not solved until we have answered certain questions: what gives an action its purpose, what leads an organism to expect to have an effect, how is utility represented in behavior. The answers to such questions are eventually to be found in past instances in which similar behavior has been effective."

p. 105 in B. F. Skinner, CONTINGENCIES OF REINFORCEMENT, New York:
Appleton-Century-Crofts, 1969. Other material that may be appropriate for quotation
appears on pp. 125-126, 193-194, and 289-290.

A. Charles Catania
University of Maryland Baltimore County
CATANIA@UMBC4.UMBC.EDU

I think we might be able to help Catania (and R. Allen Gardner) out by
providing some other "quotable quotes to the effect that it
is necessary or correct to describe behavior in terms of purposes,
intentions, or goals". Here's one by another prominent psychologist:

Psychology, which bills itself as the study of behavior, has yet to provide a universally accepted definition of its subject matter. The term "behavior" typically refers to some observable result of an organism's actions, such as a "level press". But actions produce many results, any one of which could be considered the organism's behavior (Powers, 1973). The actions that produce a lever press also move a limb, close an electric circuit, move air molecules near the level and produce a food pellet. Which result should count as behaviors of the organism? Some have argued that only intentionally produced results should count as behavior, other results being accidental side effects of actions (Powers, 1973; Searle, 1981). This approach to defining behavior is rejected by many psychologists who consider intentions both unnecessary and unobservable (Schwartz, 1978). This report shows how intentions can be observed and why the concept of intention is necessary in order to know WHAT AN ORGANISM IS DOING.

R. Marken, Psychological Reports, 1982, 50, 647-650

Happy Holidays   Rick

Date: Mon Dec 28, 1992 9:07 pm PST
From: Jackson
TO: * Dag Forssell / MCI ID: 474-2580
Subject: edits on Deming Mgmt Philo paper

Monday, December 28, 1992

Hello Dag!

>Ray: I look forward to your edit suggestions on the Deming paper.
>Snail mail has not delivered as yet what you mailed last weekend.

I don't doubt that...I haven't mailed it yet; I missed my self-imposed deadline because I spent more time with it than I thought I would (and not as much as I wanted to). I really enjoy working with things that are well written.

As you look at the notations I made, please remember the edits are based on my own personal writing preferences and, in some cases, may actually be grammatically incorrect -- I didn't take the time to look up anything I doubted, so I went with my gut feeling. Also, I'm giving you this input with all due respect; you really did some fine work here, so to contribute, I just tried to find some minor things wrong with it for you.

By the way, I ESPECIALLY like the explanation of behavior in the section on "Human Understanding"; is there any way you could e-mail me a copy of that chapter so I could have the text on my hard drive? I'm teaching an Org Behavior class next fall at Grand Canyon University, and I'm looking for materials to help me introduce those students to PCT.

The hard copies with edits will go out tomorrow, so you should get them in a few days. Please excuse the delay.

Dag, it's a real privilege to be able to work with you. I wish you and your family the very best of wishes for the New Year.

Warmest Regards, Ray

Ray L. Jackson rljackson@attmail.com
3613 W. Saragosa St.
Chandler, AZ 85226
Home: 602-963-6474
Pager: 602-244-3252 #2545

Date: Tue Dec 29, 1992 6:39 am PST
Subject: Re: PCT and other disciplines

[From Oded Maler (921229)]

[Bill Powers (921226.0700 MST)]

> This is all true. I trust you aren't saying that ALL of the
> studies of arbitrary systems will prove to be relevant and
> applicable to understanding human behavior.

I agree and even more, only a small fraction of it will be relevant to anything... but which fraction?

* A little fanaticism is appropriate if it's limited to the
* boundaries of the PCT-non-PCT controversy. You need some kind of
* support structure when there are so many people who look on your
* views with disdain, condescension, and irritation, apparently
* believing that this is how science is supposed to work.

Each community has its own system of rewards and encouragements, whether it is the church of established Science or the cult of enlightened PCTers. Maybe if you could offer tenures, you would have by now more experimental research done, but you will lose some the flame and devotion to truth. Just look at the history of some well-known religions.

About Sacks's book. I still recommend it (it is very easily read, especially for native English speakers). It has some cases of people who lost proprioperception and compenstated it thry visual feed-back loops, etc.

* >The following is a cross-posting from the Control (in the
* >mathematical engineering sense) mailing list. It contains
* >titles of all papers in the subject published recently. Just
* >for information I don't claim anything will be relevant.
*

* If you wanted to make me feel ignorant, you certainly succeeded.
* How I wish that I could understand all that stuff!

Me too!

The next great
* leap forward in PCT is going to be generated by a person who is
* comfortable with those advanced mathematical treatments, AND who
* has a clear idea of the phenomena of behavior that need to be
* explained. That person hasn't appeared yet, and probably won't
* until the basic concepts of PCT have been accepted widely enough
* that a person could devote a career to it.
*
* I would love to write a paper for journals like these explaining
* what we are trying to do with PCT and how people with such great
* talents could contribute to the work. But such a paper would have
* to be written by someone who speaks the language; anything I
* wrote would be considered too simple-minded even to be
* interesting to the readership. PCT needs translators;

*
* The world of psychology seems almost closed to PCT, but
* psychologists are not the only ones who are trying to understand
* human nature. PCT can spread to other disciplines, and is doing
* so. In every case, however, this spread has been none of my
* doing, but the doing of others who can take the basic ideas to
* their own colleagues and explain them in relation to the
* interests of those other disciplines. There always remains the
* problem of displacing the old concepts of human behavior,
* traceable mostly to conventional psychology and biology, but this
* is done most easily by people who grew up with those ideas and
* understand how they look to those who have adopted them.
*
* We lack biologists and biochemists and control-system engineers,
* among others. Maybe these, along with psychologists, are the
* toughest nuts to crack because of the direct contradictions
* involved in biology, and the implied competition in control engineering. If
* anyone knows people in these fields who might be
* willing to join in, by all means try to recruit them.
*
* And you, Oded. Are you all alone in your appreciation of the
* concepts of PCT? Do you have any colleagues who show any interest?
* ---

To clarify matters let me state that I'm almost 36, with Ph.D. in computer science (automata theory, logic, verification of programs) and without a permanent position. If the latter fact will not change within 3-4 years, I will have to abandon my academic habits and addictions (reading e-mail, visiting libraries, day-dreaming on some interesting problems, proving some theorems and writing papers from time to time, etc.) and start getting paid for doing things that are of interest to other but not to me. In order to prevent this prostitutional nightmare from becoming true, I must have some recognized achievements which will somehow fall within the boundaries of some discipline. My only chances are in the neighborhood of computer science. My niche (hybrid discrete-continuous dynamical systems) is already enough relatively multi-disciplinary

(bridging between CS and Control people) to be dangerous, and mixing it with an (unrecognized!) species of the life/mind sciences will do me no good.

My background in the mathematics of control is probably weaker than yours, but I'm in the situation of knowing some (friend of cousin of..) control mathematicians and knowing what they are talking about. Your main problems, I think, will be to define the environment, (Boss reality, CEV etc.). The type of results these people usually have is of the form "given an environment obeying such and such restrictions (including stochastic ones), a controller defined like this and that will achieve performance such and such (e.g., mean error smaller than something). " Apparently you might want to prove some properties of your infamous hierarchical servo-loops aka HPCT, but what kind of properties exactly? Does the fact they they model living systems play some role? I think a first step in any direction is to state exactly what is your current dissatisfaction with the mathematical state-of-the-art of PCT. And please do it very slowly in small pieces, because I'm a very atypical pseudo-mathematicians.

Best regards --Oded

p.s.

To Rick; Your atittude towards Jacob might change a bit if you notice that his cheating Esau was one of the first instances of manipulating perceptual variables of others (Isaac's) . Maybe he predated Bill in discovering some levels of PCT, although at that time, since he didn't yet have principles, he abused is advanced state of knowledge. Also, criticising his "cowardness" when returning to Knaan, is not consistent with your attitude toward conflicts. He was seeking a compromise and was ready to offer Esau some of his herds, instead of getting into a fight.
--
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Oded Maler, LGI-IMAG (Campus), B.P. 53x, 38041 Grenoble, France
Phone: 76635846 Fax: 76446675 e-mail: maler@vercors.imag.fr
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Date: Tue Dec 29, 1992 8:27 am PST
Subject: Conflict and Control

[From Rick Marken (921229.0800)]

In an effort to direct the net discussion toward more tangible concerns, I would like to discuss some research I started this weekend on the phenomenon of conflict.

Conflict is a uniquely control system phenomen. It occurs when two control systems try to keep the same perception at different reference levels. For example, consider two control systems that are controlling the two dimensional position of a dot on a computer screen. One system controls the perception of the dot in the x dimension, the other controls the perception of the dot in the y dimension. The systems control these perceptions by varying lower level perceptions -- such as the horizontal, h, and vertical, v, position (as sensed) of the mouse.

We can represent the situation like this:

1) x = a1*h
y = b2*v

x and y can be considered the reference values for the position of the dot on the screen; the subject must vary the lower level perceptions, h and v, to produce the perceptions that equal the reference values. Obviously, this can be done in this situation since it is possible to find values for h and v that produce perceptions that equal x and y; there is no conflict.

A conflict would exist if we set things up like this:

2) x = a1*h
y = a2*h

Now there is no way to solve for x and y (assuming they x<>y) because there is only one lower level perception that can be varied to produce two different perceptions that satisfy both the x and y references; there is no way to vary h and have the result equal BOTH x and y if x<>y. There is a conflict -- both goals cannot be achieved simultaneously (this is what would be called an "approach-appoach" conflict; other "classic" conflicts -- like "approach - avoidance" can also be seen as conflicts between control systems).

It is possible to produce intermediate levels of conflict between the no-conflict situation of equations (1) and the conflict situation of equation (2). This is done by having the lower level perceptions contribute to the perceptions requested by both higher level references such that:

3) x = a1*h + b1*v
y = a2*h + b2*v

Now the no-conflict case, (1), is the situation where b1 = 0 and a2 = 0; and conflict exists when b1=0 and b2=0. There is a theorm in linear algebra (as Oded knows, my favorite math) that says that there is a solution to a pair of linear equations, as in (3), when the determinant of the system is not zero. The determinant of (3), D, is a1*b2 - a2*b1. So there is a solution to (3) when

4) D <> 0.

When b1 = 0 and a2 = 0 (the no conflict case) there IS a solution; when b1=0 and b2=0 there is no solution (and, indeed, this is what a conflict means -- there is no way to solve for the two goals value, x and y, simultaneously).

So if inequality (4) is satisfied, there IS a way to achieve both goals simultaneously. But some values of the coefficents -- a1,a2,b1,b2 -- result in a determinant that is closer to 0 than others. Indeed, we can pick coefficients so that D ranges from 0 to infinity (I'll ignore negative values of D for now because that changes the polarity of the control task). So D can be considered a measure of "degree of conflict" -- with small values of D indicatingf high levels of conflict. But, as long as D>0 there IS a solution to the conflict.

I did an experiment to see if D (closeness to conflict) made a difference in a control task. My intuition was that it would NOT; if there is a solution to equation (3) then a pair of independent control systems (one trying to produce a perception equal to x, the other a perception equal to y) should find the solution. In fact, it DOES make a difference -- at least when there are continuously varying disturbances present.

I set up a two dimensional tracking task based on equation (2) except that continuously varying disturbances were added to x and y so that:

5) x = a1*h + b1*v + dx
    y = a2*h + b2*v + dy

where dx and dy are time varying disturbances, h and v are the horizontal and vertical measures of mouse position and x and y are the target position of the cursor on the screen (x = 250, y = 150).

I could pick values for the 4 coefficients to determine a D value of my choice.The subject then performed the tracking task with that level of D in effect. The results were quite clear -- the subject's ability to control the cursor (keep it near the x,y target position), as measured by RMS error, declined as the value the value of D decreased -- the closer to conflict, the poorer the control.

I was surprised by this but discovered, to my relief, that the basic control model behaves in exactly the same way. Mouse movements (h,v values) produced by two independent control systems were almost exactly the same as those produced by the subject, as a a function of D. So the control exerted by two independent control systems worsens as they approach conflict -- D = 0. For both human and model this is only true when there is a continuous disturbance to the controlled variable; when there is no disturbance, both human and the control model find the h and v values that bring the cursor to the goal x,y position.

There are a number of interesting implications of this little set of experiments:

1) A person can be operating with "nearly" conflicted control systems with no problems as long as there is no disturbance to the variables controlled by the conflicted systems. Thus, "nearly" conflicted systems can act like Martin Taylor's hypothetical "bug" in a control hierarchy -- having no deleterious effect until disturbances start to vary.

2) Control with "nearly" conflicted systems mimics control when there is no conflict but the control systems themselves are poor (low gain, for example). So the same behavioral sympton (poor control) could be the result of poorly functioning control systems -- or from perfectly functioning control systems that are in conflict (there was nothing wrong with the control systems used to model conflict -- the same, high gain systems that produced nearly perfect control when D was large produced lousy control when D was small; the control systems were fine; what they were controlling -- mutually -- was the cause of poor control). I plan to do some further research to see if there are some simple ways to distinguish poor control due to conflict from poor control due to poorly developed control system. This could have interesting practical implications -- if poor control is the result of conflict then the solution would be some form of therapy -- like "going up a level"; if it's due to a poorly developed control system then the solution would be some form of training.

3) There is some evidence in that data that when the subject is in a noticable conflict (because it is impossible to control the cursor) some reorganization is occuring; the gain of the subject's control system seems to change a bit when the conflict (actually, the symptoms thereof) is perceived. This reorganization (or, possibly just the work of higher level control systems) shows up in slight but apparently systematic deviations of the subject's behavior from that of the model. Thus, there may be the seeds of an approach to studying reorganization here -- by varying D.

Any comments, questions or suggestions would be most welcome.

By the way -- all this work (including the modeling) was done in HyperCard with HyperTalk (inspired by some offline discussions with Rich Thurman). Take THAT, C freaks.

Best Rick

Date: Tue Dec 29, 1992 10:52 am PST
Subject: Re: Conflict and Control

[Martin Taylor 921229 13:45] (Rick Marken 921229.0800)

Rick,

Your experiments on conflict sound great. Would it be too much to ask for a copy of your HyperCard stack? I promise not to do anything with it to pre-empt you (I don't have access to experimental subjects, anyway), but I might be able to send you some proposed test conditions or script modifications that could link into (a) Bomb, (b) superstition, or (c) information theory (no promises on any of them).

For this kind of thing, HyperCard sounds like a good way to go. (Isn't there a translator to an HC look-alike on IBM compatibles, too? That would let Bill P look at it.)

Martin

Date: Tue Dec 29, 1992 11:14 am PST
Subject: Jacob and Esau

[From Rick Marken (921229.1030) Oded Maler (921229) --

> I will have to abandon my academic habits and
>addictions (reading e-mail, visiting libraries, day-dreaming on some
>interesting problems, proving some theorems and writing papers from
>time to time, etc.) and start getting paid for doing things that
>are of interest to other but not to me.

Oded -- we have the same addictions! It's really not so bad doing some things that are of more interest to others than to oneself; so that one can eat (and send the kids to college). But obviously, one can find time -- even while prostituting oneself -- to enjoy one's addictions (as I am now, during this holiday lull at work). Luckily, my company is a very kind master; believe it or not, they just gave me a promotion and a raise.

>To Rick; Your atittude towards Jacob might change a bit if you notice
>that his cheating Esau was one of the first instances of manipulating
>perceptual variables of others (Isaac's) .

As I said, there are probably many ways to appreciate the story of Jacob and Esau -- great art has that characteristic. I have heard at least two other "interpretations" of the story, both of which were aimed at improving my attitude toward Jacob. But what I was describing was my own personal experience of the story -- no matter how much it get's re-explained, when I hear the story my own experience is one of being greatly moved by Esau -- and greatly repelled by Jacob. It's just a visceral thing that interpretation can't seem to change. I can compare it to the experience I have when listening to a version of the Brandenberg Concertos that I have at home. I find it one of the finest, most moving performances I have ever heard -- beautiful coloration, all the right tempi, just wonderful. I can listen to it over and over. About a month after I got it (it was a gift) I noticed that the performance was conducted by van Karijan -- a charter member

Nazi. I hate Nazis. But, damnit, even after I found out that it was van Karijan, the Brandenbergs still sounded great. Of course, it might have helped that van Karijan is dead. But, truth be told, the music sounds great (to me). The same is true of the Jacob and Esau story -- you can tell me all this great, deep interpretation that shows all these wonderful things about Jacob and these awful things about Esau. But, damnit, when I listen to the story, I feel good about Esau (like I feel good about Bach) and Jacob makes me feel slimy. I'm moved when Esau embraces Jacob; I cringe when Jacob is obsequious to Esau. I'm perfectly happen to be told that I shouldn't feel this way (just like I'm perfectly happy to be told by the local music maven on KUSC -- Jim Sveda -- that the van Karijan Brandenberg's are really for s**t). I'm willing to believe it; but what I experience when I hear the van Karijan Brandenberg's and when I hear the story of Jacob and Esau is what I experience.

This week, I'll take the "It's all perception" choice, Dag.

Regards Rick

Date: Tue Dec 29, 1992 11:38 am PST

[From Rick Marken (921229.1130)] Martin Taylor (921229 13:45) --

>Your experiments on conflict sound great. Would it be too much to ask for
>a copy of your HyperCard stack?

Thanks. I will BinHex them and send them to you tommorrow (I have to bring them in to work to do the transfer; I also want to pretty them up a tad so that you can use them better. But I'll be available for help if they are still arcane).

> I promise not to do anything with it to
>pre-empt you (I don't have access to experimental subjects, anyway),

PLEASE PRE-EMPT ME!! Heck, with PCT we have trouble getting people to STEAL our ideas -- even when we intentionally leave them out in the open. I would love it if you would use this stack as the basis for some of your own research.

>I might be able to send you some proposed test conditions or script
>modifications that could link into (a) Bomb, (b) superstition, or
>(c) information theory (no promises on any of them).

Great. If you get interested, I'd be happy to help; I don't need publications or recognition (just money -- he he) and I would really be thrilled if you proposed some test conditions; I have the same access to subject's that you do -- just people hangin' around. All you need is a couple (maybe three) -- it works the same with everyone (assuming no gross neurological problems).

>For this kind of thing, HyperCard sounds like a good way to go.

I think it's great -- at least for some fairly simple stuff (like this) where speed and efficiency are not crucial. You couldn't build an ARM or a LITTLE BABY with it (unless you used XCMDs profusely).

>(Isn't there a translator to an HC look-alike on IBM compatibles, too?
>That would let Bill P look at it.)

I'll let him see it at the Durango meeting -- there is always a Mac there. I bother Bill with enough stuff during the rest of the year -- but he still manages to get tons of work done anyway.

Best regards Rick

Date: Tue Dec 29, 1992 6:08 pm PST
Subject: Re: the van Karijan Brandenbergs

Rick's "confession" about enjoying the performance of a Nazi deserves further comment. What was, I believe, most insidious about the Nazis and about other movements was/is their appeal to the higher and baser emotions (or perhaps perceptions?) at the same time. I confess that I find old film footage of their spectacles incredibly moving, as I do good renditions of "Deutschland uber Alles." There is nothing "wrong" with recognizing such feelings -- in fact denying them is probably bad for us -- so long as we know what is happening to us. I remember watching Reagan speak (and Bush too, to some extent) and allowing myself to get into it to the point of agreeing with what he was saying. There was something incredibly seductive about "his" ideas the way he presented them. I think that experiencing such feelings as fully as we can prepares us better to deal with and combat them. I was able to get over a Reagan speech fairly quickly, but it gave me an understanding of his magic with many people.

I also think that art can stand alone, outside of its political context. Recently I went to a conference in Newport, Rhode Island. My inclinations are as leftist as those of anyone who went to college in the early sixties, but I was able to enjoy the mansions there. I felt that another women who said that she could not bear to see or be in them because of the ill-gained wealth that had bought them was depriving herself of an experience that was for me marvelous. Sort of like refusing to acknowledge the majesty of the pyramids because of the slave system that built them.

On a lighter note, I am reminded of the joke about the former Nazi general (he had somehow escaped Nuremburh) who, upon his retirement, said that he was going to now enjoy his three hobbies: drinking fine wine, listening to classical music, and torture.

Finally, and I know that some of you can put this into a more elegant statement, human beings are not rational in the sense of having all of their belief systems in sync with each other. It is just this that, I believe, allows Naziism and other systems of its ilk to flourish, and it is this we must make ourselves aware of and deal with, not the beautiful music that is also created.

Best, Eileen

Date: Tue Dec 29, 1992 9:26 pm PST
Subject: Misc catching up

[From Bill Powers (921229.2100)} Eileen Prince (921229) --

I've just had a visit with some relatives who are into "spiritual" pursuits, a la New Age stuff. I opened a couple of the books they brought along. One was a guide to miracles, written in pseudo King James English; the other was Jaguar Woman, which seems to be about some magic worker or seer. I can't tell you what they were really about because I didn't read more than a paragraph or two from each, opened at random. I've seen that sort of thing before, however, in the same circumstances, and have been left speechless just as I

was this time. What do you say to someone who has adopted such system concepts? How can you have any sort of substantive conversation when that is lurking in the background?

The greatest mystery of the human mind, in my view, is this phenomenon of Belief. Nazis are easy to deal with, because their beliefs are threatening to our physical safety and we can flatly reject them. But what about other belief systems, invented and accepted apparently at random? Is the human mind just naturally susceptible to any belief that comes around, no matter how childish and full of holes? Is there something about our highest levels of organization that demands some belief, any belief, to fill the vacuum?

It seems to me that before we can have anything approaching sanity on our planet, we must begin to understand how belief systems get formed and how to keep them from overpowering people -- how to leave a little freedom of belief, so that knowledge about the WHOLE world of experience can play a part in forming belief systems. I haven't the slightest idea of how to do that, except by continuing to point out that different people believe different things, a fact that ought to give anyone pause who is convinced that his/her own belief system is the only right one.

Or is this a level at which we are all helpless, including me?

I second Tom Bourbon's encouragement for your writing an article
for Cosmopolitan.

Tom Bourbon (921228.1550 CST)--

RE: apology.

I've said worse to people when I've been mad. A friend is someone you can call a stupid sunuvabitch without fearing that it's goodbye forever. Say what you think; we're all grown up around here.

Oded Maler (921229) --

>I agree and even more, only a small fraction of it will be
>relevant to anything... but which fraction? [of the abitrary
system will pertain to real system]

That's why we need the experimental approach, too. The weak point of any abstract system is its premises, the things it assumes about nature for the convenience of the argument, but which may be false to fact. Pure mathematics doesn't have this problem, although Godel rather spoiled the purity of the game. I interpret his theorem to mean that all axiomatic systems, if they are not to be vacuous, have to reside on outside support.

>Each community has its own system of rewards and
>encouragements, whether it is the church of established Science
>or the cult of enlightened PCTers.

I don't like that comment. What's wrong with established Science is the establishment part, not the science part. I think we PCT types go farther than most in sticking to the science and avoiding becoming an establishment. It's only people who don't ever do any experiments with real people who think that PCT is just another belief system handed down from on high. The basis of PCT is a set of easily reproducible phenomena that conventional science has overlooked. To show that PCT doesn't follow from these phenomena, you'd have to upset everything the physical sciences have developed over the

last 350 years or so. I don't think that a system with such a strong base in phenomena deserves the term "cult."

>About Sacks's book. I still recommend it (it is very easily
>read, especially for native English speakers). It has some
>cases of people who lost proprioperception and compenstated it
>thru visual feed-back loops, etc.

I've read that book, and others like it. It's another example of the medical grab-bag of unassimilated facts. Interesting gee-whiz facts, but what do they add up to? That whole field rests on informal subjective characterizations of behavior without any model to bring the facts into order. Until someone takes a modelbased approach to characterizing what is wrong with people who have these difficulties, all we will get is more books like this with more lists of unexplainable deficits.

I fully understand your problem with making a living. That does get in the way. But there are always evenings and weekends, and my God man, you're only 36!

>Your main problems, I think, will be to define the environment,
>(Boss reality, CEV etc.). The type of results these people
>usually have is of the form "given an environment obeying such
>and such restrictions (including stochastic ones), a controller
>defined like this and that will achieve performance such and
>such (e.g., mean error smaller than something). " Apparently
>you might want to prove some properties of your infamous
>hierarchical servo-loops aka HPCT, but what kind of properties
>exactly? Does the fact they they model living systems play some
>role? I think a first step in any direction is to state exactly
>what is your current dissatisfaction with the mathematical >state-of-the-art
 of PCT. And please do it very slowly in small
>pieces, because I'm a very atypical pseudo-mathematicians.

When you recognize that the environment exists for a given person only in the form of perceptions of it (through largely unknown transforms at that), the problem becomes a little different. The engineer takes the environment for granted: the variables he perceives in it are Out There. The problem from the PCT standpoint is not to achieve a particular result in the environment, like perfectly browned toast, but to achieve a particular perception that is a function of the environment but not the same function for everyone -- like the shade of toast that is deemed perfect. And it is to explain how such results can be reproduced over and over even though different actions are used each time.

I would like to know the mathematics behind perceiving forms and shapes, which I call configurations and think I can see in many sensory modalities, not just vision. The question is, how do you get a signal that says "cube" when the object in question is rotating and changing size and position? And how do you get a signal that varies in magnitude as the presented shape begins to depart from being a cube? The nearest I can get is "orthogonal trajectories" as I mentioned in a recent post. But the kinds of invariance we need sound more as if they require tensor algebra, a subject that I know discouragingly little about, or differential geometry, or some such beast. What kind of computing network could extract such invariances? I don't think that perceptrons have taken us very far toward an answer.

And that's only level 3 in my scheme. I think we're in the position of early physics, where we can identify a lot more phenomena, naturalistically, than our mathematical tools can handle.

PCT does define the environment -- but it defines it as a set of perceptions. Engineers usually assume that things like intensity, color, form, relationship, sequence, and so on are properties of the outside world. In HPCT we are attempting to explain, by modeling, why the world appears that way to human beings -- and at the same time, to point out that these things are human perceptions. Physics does not explain why the world appears to us in these categories. In fact, the world that physicists seem to have uncovered has very little to do with such things.

I would like to know if there is any way, even in principle, to establish a firm connection between the experienced world and the world that exists independently of experience. That might be the kind of problem a mathematician would enjoy -- unless Godel gets in the way.

I suspect that Godel will get in the way. But even knowing that the world an individual perceives has no unique relationships to whatever lies outside would be important.
---------- Rick Marken
(921229) --

Your experiment with conflict is fundamental and new to our repertoire. It is neat and beautiful. The fact that the model reproduces the human behavior says that the real control systems don't reorganize much until the task starts to become impossible.

Can you give us some numbers from the experiment?

Best to all, Bill P.

Date: Wed Dec 30, 1992 8:04 am PST
Subject: Re: Apologies

(Tom Bourbon (921228 15:50 CST)) --

>my earlier flippant and hurtful post did more to damage the process I
>wanted to praise than I care to imagine.

Tom, I winced when I read the earlier post, and my heart goes out to you now. I remember with chagrin times I have made similar gaffes. Knowing I am too liable to screw up who hears what I try now to avoid saying privately what I would not want heard publicly.

And what this brought to mind (by way of going up a level, after a fashion) was what gossip does for us--a recent topic. What I give up if I keep private and public communications (more) consistent with each other is this: If I say to you privately something that you know I would not say publicly, I thereby affirm the intimacy of our relationship. By entrusting you with something that I would not want publicly attributed to me I offer a token of my vulnerability. By disclosing it you could disrupt my public face. (Can you tell I've been re-reading Goffman lately?) I expect you to reciprocate, or perhaps it is I who am reciprocating an earlier offering of yours. Of such reciprocations are alliances made.

Despite the cost of giving up gossip, it does seem better to try to make one's private face and public face more congruent to each other, not by restricting private communications to a public standard, but by seeking honorable and healing ways of being forthright in the same ways in both spheres. Needless to say, that's a goal, not an achievement--I'm not adept at controlling that perception!

Underlying this are ancient mammalian processes of communication. Bateson proposes that play originates in acting as if fighting, but not hurting. There is no negation in pre-language communication (or in imagination). To communicate friendship, one appears as though acting as an enemy but in the same moment refrains from acting as an enemy. Or one may become vulnerable to the other (e.g. wolf rolling on back belly up) and experience the other not attacking. Just so then gossip. Losing that function of gossip is the cost of cleaving to a single standard. But there are other ways of achieving reciprocal exchanges of trust and vulnerability.

Thanks, Tom.

Bruce bn@bbn.com
Date: Wed Dec 30, 1992 9:48 am PST
Subject: Beliefs/Conflicts

[From Rick Marken (921230.0900)] Eileen Prince (921229)

>I also think that art can stand alone, outside of its political context.

Yes, I agree now -- after long years of being amazed by the ridiculous beliefs embraced by some of the artists I've loved best.

Bill Powers (921229.2100)

>The greatest mystery of the human mind, in my view, is this
>phenomenon of Belief.

I agree. We should explore this from a PCT perspective. The problem, of course, is that, when it comes to many of one's own beliefs, they are not treated as beliefs but as knowledge. I think many of our most tenacious INTRA - personal and INTER-personal conflicts are the result of controlling perceptions that based more on beliefs (replayed reference signals) than Boss Reality.

I think it would be worthwhile to say what beliefs are in the context of the PCT model; describe examples of the everyday beliefs that people are walking around with (from the divine, like religious beliefs, to the profane, like beliefs about the "right" foods to eat); also, it would be nice to discuss the difference (from a PCT perspective) between belief and knowledge. I know this is a difficult discussion to have -- precisely because beliefs are so important to people. With Bill I ask "WHY is this so? Why do people "fight and fight to prove that what they do not know is so?" There must be a reason that this species has been willing to persecute itself for millenia over fantasies. It must be an aspect of our nature as control systems. What is it? I think that this could be a very satisfying (and even theraputic) investigation.

>Or is this a level at which we are all helpless, including me?

No. I think people, like you (and me?), who are willing to consider the possibility that ANYTHING we think may be just a belief and, more importantly, are willing to wonder what a belief is, are not helpless victims of our beliefs (at least, when we are able to keep our awareness "above" the levels that create those beliefs -- something that I don't do nearly as often as I would like). I think it requires some effort to defeat some of the insidious consequences of belief -- but it can be done, I think.

>It's only people who don't

>ever do any experiments with real people who think that PCT is
>just another belief system handed down from on high. The basis of
>PCT is a set of easily reproducible phenomena that conventional
>science has overlooked.

Here, here.

>Rick Marken (921229) --

>Your experiment with conflict is fundamental and new to our
>repertoire. It is neat and beautiful. The fact that the model
>reproduces the human behavior says that the real control systems
>don't reorganize much until the task starts to become impossible.

Thank you. Thank you.

>Can you give us some numbers from the experiment?

Yes, once I set it up to get them. Right now I'm just using the inter- ocular trauma test. I plot the x-y position of the mouse over some period of the experiment; I was using a sine wave disturbance at first so the mouse movements (due to the coefficients of the conflict) are an elipse. When you plot the model mouse movements over the human mouse movements, they fall on top of each other (though the human's are a bit more ragged). I will get measures of fit of model to human with different values of D (conflict) over the weekend. I'm not planning to do any fancy parameter estimation -- but based on my manual approach (and visual test) I would say that the error of prediction (as percent of maximum possible deviation of model from human) is not more than 5%.

Best Rick

Date: Wed Dec 30, 1992 12:23 pm PST
Subject: Belief Systems

from Ed Ford (921230:1320) (from Bill Powers 921229.2100)

>It seems to me that before we can have anything approaching sanity
>on our planet, we must begin to understand how belief systems get
>formed and how to keep them from overpowering people...how to
>leave a little freedom of belief, so that knowledge about the
>WHOLE world of experience can play a part in forming belief systems.

Amen!

It seems to me that belief systems are formed by living control systems as they try to establish harmony within themselves as a result of their attempts to find satisfying experiences from the environment in which they find themselves. The choices we make and the standards we've set ultimately evolve into systems of ideas, or the way we think things ought to be. I think this harmony, this internal peace or internal integrity, is what the LCS is continually striving toward. Obviously, our knowledge of what's available is limited by our perception of the environment in which we find ourselves plus what becomes available to us through reorganization. What we create out of what we perceive is what ultimately becomes what we are.

I think humans tend to accept the systems concepts of those who they perceive love them and whom they love or admire (if that happens to exist, and to the extent that it does). Obviously, if there is internal peace and harmony where we live, then the prevailing systems concepts of our parents/friends is most likely to be perceived as acceptable. Those systems are ultimately tested when children (and later adults) are faced with choices which are in conflict with the prevailing or accepted systems concept. But, to me, the ultimate test of a systems concept is that first it brings internal harmony or peace to the person.

I don't believe a belief or value system (systems concept level) overpowers a person. I believe many people choose systems and elements of that system and create their own standards from how they perceive those systems to justify the choices they're making in their attempt to find that elusive peace and harmony that all LCS's are trying to establish. I believe it was somewhere in Shakespeare the famous line "even the devil can cite scripture to his means".

It is when a person does harm to another LCS that brings the systems concept into disrepute. And this shouldn't be. G.K.Chesterton once said "it isn't that Christianity has been tried and found wanting, it hasn't been tried". I don't think it's right to blame Christianity for the acts of those who, claiming to be Christians, do harm to others anymore than it's fair to blame any system of ideas on those who claim to be adherents but who go about harming others.

The second important test of any systems concept is the respect shown to those "who don't belong, who don't believe". Therein lies the critical test of any systems of beliefs, namely, that everyone is shown respect, as having value as a person, and that to me is the real test of a valid systems of beliefs. If from a systems concept I am able to establish standards and make choices that bring me the internal peace and harmony within my system AND at the same time that systems concept leads me to see value in others and respect their right to make choices, then the system has value. In short, when we harm others, we harm ourselves, and in the process the very harmony and peace we're seeking is lost.

When a person is in conflict and uses a systems concept to justify actions which bring harm to others, I don't think the systems concept is wrong, I think the person is wrong. And I don't think the belief system overpowered them, they merely used the system "to justify their own means". I think people tend to overpower themselves by setting impossible standards or goals, by trying to change things over which they have no control, or by making ineffective choices in a desperate attempt to bring harmony or peace to their system.

Because I'm an LCS by design, my systems concepts are very unique to me. No one quite perceives things the way I do. As Bill reminded me several months ago, I'm not a Republican, I'm a control theorist. And I think the test for whether our systems of beliefs are valid, is our own internal harmony and peace and the respect and value we assign to others.

Finally, I think the very nature of the LCS demands it be open to the experiences it continually encounters as a way of adjusting and improving on the efficiency and effectiveness of the entire system, especially at the systems concept level. PCT is a prime example of this. I've been exposed to this stuff for over 10 years and I'm still learning. The very nature of this concept demands a willingness to be open and to adjust your ideas and change. And letting in some fresh air never hurt anyone.

Happy New Year, Ed

Date: Wed Dec 30, 1992 1:07 pm PST
Subject: Re: Beliefs/Conflicts

[Martin Taylor 921230 14:45] (Rick Marken 921230.0900)

>I think it would be worthwhile to say what beliefs are in the context
>of the PCT model; describe examples of the everyday beliefs that
>people are walking around with (from the divine, like religious
>beliefs, to the profane, like beliefs about the "right" foods to eat); also,
>it would be nice to discuss the difference (from a PCT perspective)
>between belief and knowledge. I know this is a difficult discussion to
>have -- precisely because beliefs are so important to people. With Bill
>I ask "WHY is this so? Why do people "fight and fight to prove that what
>they do not know is so?" There must be a reason that this species has
>been willing to persecute itself for millenia over fantasies. It must be
>an aspect of our nature as control systems. What is it? I think
>that this could be a very satisfying (and even theraputic) investigation.

At this moment I don't want to speculate about what beliefs "are." That impinges on the question of consciousness. But I would like to make an analogy that could be helpful. (Since writing this, I succumbed, and added a final paragraph in which I do so speculate. I plead irresponsible festivity).

The argument is that normal reorganization leads to superstitious actions, defined as actions that are neither helpful nor hurtful to the control of the perceptions in the ECS (Elementary Control System) whose output leads to those actions. Superstitious actions are more likely to persist at higher levels of the hierarchy than at lower. The speculation is that we believe that what we do is what we *should* do, in the absence of evidence to the contrary, and that much of what we do is superstitious in the sense I just defined.

What does reorganization do? It modifies connection patterns and strengths within the hierarchy, makes new ECSs that control for previously undetected patterns (e.g. configurations, intensities, sequences, principles--it doesn't matter which level, I use "pattern" as the input to any Perceptual Input Function (PIF)), and perhaps modifies existing PIFs. We assume that reorganization occurs as a consequence of error, particularly growing error, in some intrinsic variable. When a reorganization event occurs, it is a random event, subject to some (unspecified here) constraints. If the reorganization event succeeds in creating connections that result in behaviour that reduces the error in the intrinsic variable, reorganization stops, leaving the connections as they are "forevermore."

In a complex hierarchy, each output of an ECS branches many ways to contribute to the reference inputs of lower ECSs. Another way of saying this is that many lower-level behaviours are actions that form part of the feedback loop involved in the control of any perception. These lower level behaviours are independent of each other, except insofar as the nature of the world (and of their support structure in lower ECSs) creates conflict. Ignore conflict, for now. But consider--the reorganization that linked these lower-level actions to the ECS output was random. That means that there can be actions that neither support nor conflict with actions that support the perceptual control performed by the higher ECS. Nevertheless, when the higher ECS is generating output, these actions are performed in addition to the ones that actually provide the negative feedback. I call them "superstitious" actions. A golfer's waggle before starting the

swing might be an example. Things work when you perform a superstitious action in support of a "higher purpose," but not because of the superstitious action.

But would you believe the superstitious actions had nothing to do with your success? Not unless you tried to control the higher-level perception without using those actions, and why would you do that? Reorganization has left you with a perfectly workable system of output-to-reference links, and there is nothing to tell you that one action is important and another pointless--WITHIN the construct of control for that higher ECS.

At low levels of the hierarchy, any ECS may be part of the actions of many higher ones, and if it is irrelevant in the context of one higher-level control loop, it may be important in the context of another. So its connections may be reorganized without affecting the behaviour that happened to result in control of the intrinsic variable we first considered (in the previous two paragraphs). One might expect much of the low-level superstitious behaviour to be washed away by the random currents of reorganization. But not a high levels, because there is less opportunity for the kind of conflict among system-level and principle-level perceptual control than there is at lower levels. There are (I would speculate) fewer of them, and they operate more slowly (it makes no evolutionary sense to reorganize a control system more rapidly than it can operate).

What this seems to result in is that high-level superstitious behaviour is likely to persist. If we make an assumption that we generally believe that what we do is what we should do, provided that it doesn't lead to internal conflict, we arrive at the proposition that people might be expected to have and to maintain belief systems that are unsupportable by perceptual evidence. (This paragraph is even more speculative than the rest, but it's the fextive season, so why not indulge?).

Martin

Date: Wed Dec 30, 1992 1:53 pm PST
Subject: Re: Apologies

[Martin Taylor 921230 16:30]
(Bruce Nevin 921230 10:53 and Bill Powers 921229.2100) in response to
(Tom Bourbon 921228.1550 CST)

Both Bruce and Bill, from different viewpoints, argue that you should be able to say the same things publicly or privately. As the one to whom Tom was mainly apologizing, I don't think I agree. Bruce's argument was essentially that one should be circumspect in both cases, and not say anything privately that one would not wish to be made public (and that it displayed trust to do otherwise--an independent point). Bill's point is:

> A friend is someone
>you can call a stupid sunuvabitch without fearing that it's
>goodbye forever. Say what you think; we're all grown up around here.

Bill has said that before, on similar occasions. The problem arises not with the friends, but with the lurkers who may not realize that these things are said among friends, or if the insult is simed at someone who may not be sure that the insulter thinks of the insultee as a friend. To my friends and those whom I trust, I can be very insulting because I know that they will take it in the proper context. But I don't normally do it in public, because one of the aspects of social interaction is the

perception that third parties have of one's relationships. Courtesy is important, except in private, and then only if the discourtesy is taken properly.

Now I admit to being at fault, not following my own prescription on several occasions. Like Bruce, I sometimes forget that we are not a closed community. When I teased Rick with a comment I won't repeat because he did find it insulting, I was thinking within a closed community consisting of Rick, Bill P., Tom, Bruce, and one or two others, forgetting that there are over 100 other people, some of whom may not know the depth of Rick's understanding of PCT. I said the sort of thing I would have said to Rick face to face over a beer. That is not appropriate on a public mailing list.

Tom's comment likewise was intended to be private. (I would have preferred it to be Cc-ed to me--I like to know what people think of me, even if it isn't complimentary). I answered him privately, and, I hope adequately and not insultingly. I do not think Tom's comment should have been public, and he didn't intend it so. Within the small closed community, I would have no problem with it being public. As Bill said, "We're all grown up around here." But there are listeners who might be tempted to chime in, and the very last thing we want on CSG-L is a flame war like those that occur on so many mailing lists and Usenet groups.

Martin

Date: Wed Dec 30, 1992 3:01 pm PST
Subject: Belief Systems

[From Rick Marken (921230.1400)] Ed Ford (921230:1320) --

>I don't believe a belief or value system (systems concept level)
>overpowers a person.

A belief and a system concept are not the same thing. A belief in PCT (I think) is an imagined perception: this means that beliefs can occur at any level of the hierarchy (except for the lowest); we can believe that the sky is blue (sensation), that it will rain (fluid transitions?), that we're loved (relationship), etc. We can also have beliefs that are system concept level perceptions -- I can believe that I am a control theorist.

Beliefs (by my definition) can also differ in terms of one's ability to produce or experience them as perceptions (rather than just as imaginations). I believe my car is in the lot and I can produce that perception; I believe that Mozart was the means by which god spoke to humanity -- but I can't produce that perception (I can certainly produce the imagination).

Our ability to "believe" is, I think, one of the things that makes life fun; it makes it possible to be entertained by stories, plays and such. It think it also makes life a bit more tolerable (as Ed said, it helps us "find that elusive peace and harmony that all living control systems are trying to establish"). It does this by "filling in" the unachieved aspects of the perceptions we are controlling; we believe that we are "loved", for example -- and we create a perception that is based mostly on Boss Reality but that is "filled in" a bit by belief (imagination) so that our control seems a bit better than it might actually be.

But you can see that what is good about belief is what could also make it a problem; belief makes stories fun because we treat the imaginations as though they were "real" perceptions; but what happens when we forget that they ARE NOT AND NEVER WERE REAL

PERCEPTIONS? I think we get what we see -- people willing to die or kill to control for imagined perceptions

I think it is interesting that when the "filling in" done by belief gets to be a bigger part of perception than the part constrained by Boss Reality, we call that "insanity". But when the "filling in" is TOTAL -- so that there is no constaint of Boss Reality -- just belief based on made up stories (the Bible, the Koran, etc) we (some of us) call that "wisdom". I suggest that we call it what it is -- "total insanity"

Happy New Year Rick

Date: Wed Dec 30, 1992 3:58 pm PST
Subject: unnoticed

[From: Bruce Nevin (Wed 921230 15:52:04)] Ed Ford 30 Dec 1992 13:18:04

Once Mohandas Gandhi was asked by a reporter what he thought of Christian civilization. "I think," he said, "that it would be a very good idea."

Here is the Chesterton quote:

> "The Christian ideal," it is said, "has not been tried
> and found wanting; it has been found difficult and left
> untried."
> _What's Wrong with the World_ (1910)

Re beliefs and imagination, a twist with a holiday theme:

I just heard with half an ear on NPR something about a recent study of obesity at Columbia and Cornell. Those who failed to lose weight turned out not to be reporting accurately either how much they actually ate or how much they actually exercised--and they appeared not to be registering these perceptions accurately themselves. They were eating roughly twice as much as they recorded themselves eating. The added calories and reduced exercise accounted quite well for the differences in weight loss (or gain) with respect to those who did well on the test diet.

Is it simply a matter of not attending? Or of attending to imagined perceptions at the expense of actual ones? Or some combination of phasing out at critical junctures, and then substituting imagined perceptions that are comforting? (Perhaps "comforting" means "satisfying control of higher-level perceptions?)

Simply paying careful attention when one finds oneself indulging in a "bad" behavior (you're trying to quit smoking, say, and suddenly become aware that a cigarette is somehow in your fingers and that you have just lit it). Simply paying careful attention, without judgement or emotional reaction, seems often to be a prerequisite to subsequent change. Maybe we can now explain this phenomenon.

All the "religious" practices that seem to me to be serious (as opposed to being mere social and cultural institutions) have at their heart some form of individual practice that could be called meditation. Coming to one's senses, in place of customary fantasies. The distinction between religious experience and religious institutions is fundamental. Ideally, the latter support the former. But given the former, you don't need any of the latter. And in the absence of the former, the latter are worse than empty shells.

When we pay attention to our active perceptions, it seems to me that there is a lot more going on than our philosophies or imaginations dream of. It does make sense that imagination would simplify things by leaving levels below the imagination loop out of account.

Bruce bn@bbn.com

Date: Thu Dec 31, 1992 9:20 am PST
Subject: Happy 1993

[From Rick Marken (921231.0900)]

Well, it's pretty quiet out there. It's either the lull at the end of the year of Bill Powers' vax is still down.

Anyway, let me be the first to wish Avery Andrews a Happy New Year; since he is the first on CSGNet (I think?) to experience 1993 -- How is it, Avery?

May 1993 see the end of many more cherished beliefs (like trickle down economics).

Better yet, may 1993 be the year when people start enjoying their beliefs as beliefs -- and stop mistaking them for knowledge and understanding.

Feliz Ano Nuevo Rick

Date: Thu Dec 31, 1992 1:33 pm PST
Subject: Re: Apologies

From Tom Bourbon (921231 15:14)

This is probably the final time I will use the "reply" function!

To Bruce Nevin, Bill Powers, and most of all to Martin Taylor, I am happy to see that all of you "went up" what appears to be several levels after my fat-fingered error. Martin, the ideas you discussed in your post (921230) went directly to the heart of my concerns in the apology: I use remarks much stronger than anything in the wayward post in conversations with friends, when we are in private places. And I use far stronger remarks to characterize my own actions when I find them wanting. (You might have been interested in my comments to myself upon seeing a private post sitting in csg-l basket!)

My first concerns were that so many people are listening in the background, and that Martin might (understandably) take the remarks as insulting. I was not sure enough about how he perceived our interactions that I could assume the post would pass as a casual and careless remark.

I have no experience with other nets and interest groups, but from all I hear and see, the tone of csg-l is different from many others. I value the content and the tone of the material on this net. And even more than before, I value the sensitivities and kindnesses of the participants.

Until later, Tom Bourbon

Date: Thu Dec 31, 1992 2:04 pm PST
Subject: Bad day at the library

[From Rick Marken (921231.1400)] Tom Bourbon (921228 9:23) --

>If you want to get an idea of what behavioral ald life science
>are like without the realization that living things are in a high-gain
>negative feedback interaction with their environment, try these two
>recent collections:

> American Psychologist, Vol. 47, No. 11, November 1992.

>22 articles, an introduction, and a historical note make this a rich
>source for anyone who needs up-to-the-minute evidence that radical
>behaviorists say the things we (would-be PCT authors) attribute to them

Forget the radical -- this is conservative, conventional, state of the art psychology talking here. I looked it over briefly; very few references to PCT, indeed. I guess they figure they can do just fine without us.

Interestingly, the author of one of the articles is Charles Catania, with whom I've started an off-line conversation about purpose in behavior. I sent him my little quote on the importance of determining purpose in order to know what an organism is doing (as an alternative to the Skinner quote I posted a couple days ago). He was interested in how one might determine an animal's purpose; I referred him to my "Behavior in the first degree" paper. He suggested that our discussion might move into PSYCHOLOQUY -- now where have I heard that name before?

> All in all, these two thick collections gave me a renewed appreciation
>of the degree to which the idea of high-gain negative feedback control
>has penetrated the behavioral and life sciences -- not one bit.

Beautifully said; I nearly fell off my chair laughing when I read it. I think your estimate is accurate to the forth decimal place.

Love and joy Rick

Date: Thu Dec 31, 1992 7:24 pm PST
Subject: HAVE A PLEASANT, PRODUCTIVE AND HAPPY 1993

Dear CSGers,

I have enjoyed the conversations and encourage the continuation of of them.

Next year I plan a comment on "beliefs" which I think is crucial to the further development of PCT; until PCT can deal with "beliefs" its success at convincing others that it is a viable approach is seriously hampered. This I believe sincerely!!!!

May our work next year bring further comprehension of social life and a reduction of conflict and suffering for everyone.

With deep respect and sincere appreciation to everyone,

Chuck