

Date: Tue Mar 01, 1994 3:59 am PST  
From: CHARLES W. TUCKER  
Subject: Mindread program

Dear Dag,

You are correct; I did not wait 20 minutes for tables to compute. I have a 486sx w/o math coprocessor. I will try the program on my DX machine and see what happens; I like the idea of not having the figures wander off the screen. I will let you know what happens later today.

Regards, Chuck

Date: Tue Mar 01, 1994 10:25 am PST  
Subject: Defining information

[Bill Powers (940301.0900 MST)] Martin Taylor (940228.1915)

>The definition of information, in Information Theory terms  
>is quite straightforward.

There are two kinds of definition here. In one, there are symbols on both sides of the equal sign, as in

$H = -\sum(P_i \log(P_i))$  (the  $i$  is a subscript)

In the other, there is a symbol on one side and an experience of a perception, which in communication we must indicate by other symbols that refer to it, on the other side:

[What it is to perceive information] =  $H$ .

In the first kind, there can be no argument because the definition is merely a substitution rule: wherever you see the symbol  $H$ , you can use the more complex expression on the right, and vice versa.

The second kind of definition is not so protected from argument. The statement is that the symbol  $H$  (or by substitution the expression  $-\sum(P_i \log(P_i))$ ) is defined in terms of some experience which is not just another symbol or symbol string.

The second kind of definition leaves us with a choice: we could equally well say

[What it is to perceive the  $i$ -th probability] =  $P_i$ ,

which, through the substitution rule, also defines  $H$ .

This duality of definition shows up in many places. For example, we have

$IQ$  = score derived by rule from answers in an intelligence test,

and

[What it is to perceive intelligence] =  $IQ$ .

or

$G = a*\lambda_1 + b*\lambda_2 + c*\lambda_3$ , and

[What it is to perceive green] = G.

Many symbols are defined in terms of other symbols, which in turn are directly associated with experiences. But there are many symbols which are defined in terms of other symbols, which in turn are defined only in terms of still other symbols, without ever reaching the level of

[What it is to perceive X] = X.

I define "meaning" to be the experiential side of the above equation. The experience that we symbolize as X is the meaning of X. In contrast, the definition of X is another symbol string for which X stands.

Meaningful words have both experiential denotations and subjective connotations; the denotations link the word to a specific experience, while the connotations work through memory associations with other words involved in the symbolic definitions. Meaningless words have only connotations; they denote nothing but the general background sense of meaningfulness that comes from all the connotations.

We learn words from context, from hearing and seeing how they are used by others. Unless one is alert to the distinction I am drawing here, it is easy to confuse meaningless words with meaningful ones. A doctor whose name I forget (Brower?) once wrote a beautiful science-fiction story (his only one, I think) called "The Gostak and the Doshes." In this story, the proponent finds himself in a civilization where a furious and violent debate was going on. One side says that the Gostak distims the Doshes, while the other side violently denies that assertion: the Gostak by no means distims any Doshes, never has done so, and never will do so. When the proponent objects that none of these words has any meaning, he is turned upon by both sides for hinting that the Gostak does not exist and that there are no Doshes. A Dosh, clearly, is that which the Gostak either does or does not distim. The Gostak, of course, that that which distims (or does not distim) Doshes. And distimming, as any fool knows, is what can be done to a Dosh, although whether the Gostak does or does not do it is a matter for torchlight parades and rock-throwing to settle. I read this story several times in anthologies; only the later readings had really poignant meaning for me.

As a teenager fresh out of the Navy and somewhat nervously contemplating college, I decided to try to get a head start by reading or re-reading some of the hard stuff, like Socrates and Kant. I discovered, to my great satisfaction, that I was a natural-born philosopher: this stuff was easy! I read copiously, and within a month or two was holding forth at parties on any subject anyone wished to raise. I recall one small party with a couple ten years older than me (younger friends of my parents), who had invited one of their contemporaries to make a foursome, where I kept everyone up far past a reasonable hour, ignoring all attempts to speed the parting guest, answering all questions from left and right with total assurance. Oh, God. I would not be that age again for anything.

Then I went to college and quickly ran into courses by Oliver J. Lee and S. I. Hayakawa (yes, the same one) on General Semantics, and before long my new-found profession lay in ruins. What I had discovered was that I was extremely proficient with words and word-associations, and miserably ignorant of meanings. I was great with maps but didn't know the territory. Later, when I went back to read yet

again from that font of wisdom I had so easily soaked up, I found that this was not entirely my fault, but only a manifestation of a very common problem. I found that a great part of my vocabulary was useless, because it referred to nothing at all but other words. My style of communication swiftly became a lot simpler as I looked at one of my favorite words after another and discarded them as empty shells, along with the learned treatises in which I had found them.

The weeding paradigm was simple. Given a word or symbol X in the relation

[What it is to perceive X] = X,

find the left side of the equation. If there is nothing in experience on the left side but other words, X is meaningless, at least for me.

So this brings us back to information. In

[What it is to perceive information] = H, or  
[What it is to perceive the i-th probability] = Pi,

what is on the left side of either equation? If there is no clear communicable experience on the left side, then we have only the other kind of definition,

$H = -\sum(P_i \cdot \log(P_i))$ ,

which by itself is only a substitution rule without meaning. This substitution rule can be used in complex arguments and deductions along with other similar rules, and one can prove theorems and talk about right and wrong manipulations, but the totality of the structure boils down to a tautology (if no mistakes have been made), and has nothing to do with observing the world.

I have not been able to find in my own direct experience anything that corresponds to H or Pi, except for a rather confused set of indefinite images that change with circumstances. So I do not believe in information or probability: they are just words, for me. I can observe nothing in my world that corresponds to them.

Best, Bill P.

Date: Tue Mar 01, 1994 11:05 am PST  
Subject: Re: Uncertainty of disturbance

<Martin Taylor 940301 12:00> >Rick Marken (940228.2200)

>OK. IT advocates believe that IT is a CONSEQUENCE of PCT.

Who said anything related to that, ever?

>But this is no different than what I meant by "IT is anticipated by PCT".

>Here's a diagram to make it easier:

>

>PCT--->IT

>

>Note how IT is "anticipated by" PCT, temporally or logically, and how,

>therefore, IT follows from or is a temporal or logical CONSEQUENCE of PCT.

All right. Arithmetic is a logical consequence of control theory, Information theory is a logical consequence of perceptual control theory. Fourier analysis is a logical consequence of the workings of filters. What point are you trying to make? That any inanity can be proposed as a fact of science?

=====

>How does one measure the uncertainty of a disturbance given the perceptual  
>signal??

Well, if you ever asked THAT question before, I guess I missed it. That's an easy one, with one caveat that Shannon discusses in detail (see my posting of extended quotes from Shannon some weeks ago). I shouldn't have to write the following, since it was all in what I quoted from Shannon (who you really ought to read, rather than asking me for tutorials all the time. He's a good writer, and the seminal book is an easy read.)

You take the values of the perceptual signal and the values of the disturbance signal and plot them against one another. If those values are sampled, you have a scattergram, but if they are continuous, you get a kind of scribble.

(Extra point, which you keep as a flag for later--you should do this for all possible delays between the disturbance signal and the perceptual signal, making it a 3-D plot. But we'll forget that for the moment).

When you have your scattergram or scribble, project it onto each axis, so that you then have two plots of how often the disturbance signal and the perceptual signal take on any particular value. If you had a scattergram of sampled values with quantized measurements, you have a discrete plot; if you had a scribble, you have a continuous plot.

The two plots you have allow you to determine the uncertainty of the two individual signals, using the formula

$[-\sum_i p_i \log(p_i)]$  or  $[-\int p_i \log p_i di]$

where  $p_i$  is the probability of finding the value at time  $t$  in bin  $i$ , and  $p_i$  is the corresponding probability density of finding the value to be  $i$ .

depending on whether you had a quantized scattergram or a scribble.

Now you have two values,  $U(\text{perceptual})$  and  $U(\text{disturbance})$ . What you want is what the perceptual signal gives you about the disturbance.

Take your 2-D diagram, and for each value of the perceptual signal, generate a plot like the one you just made, but this time for only that value of the perceptual signal. To see how this works, suppose that your original plot looked like this:

```

      4 |           *   **  *
      7 |           *  *** *  **
      9 |          * * ** ** ** *
      7 |          ** *****
      7 | - * *** *** ----- one value of the perceptual signal
perceptual 5 | * *** *
signal      4 | *** *

```

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3 |***
3 |** *
1 | *
  |_____
  253444143242302113101
  disturbance

```

You get the uncertainties of the two independent signals by summing the  $p \log p$  values. You get the uncertainty of the disturbance for a GIVEN value of the perceptual signal (such as the value marked by the dashed line) in the same way, but the probability concerned is the probability of getting disturbance value  $i$  if you have perceptual signal  $j$  ( $p(d=i|perc=j)$ , or  $p(i|j)$ , for short). That uncertainty is

$$U(\text{dist}|perc == j) = [\text{sum over } i \text{ where } perc == j (p(i|j) \log p(i|j))]$$

or the corresponding integral if you have a scribble rather than a scattergram.

Finally, the uncertainty of the disturbance given the perceptual signal is

$$U(\text{dist}|perc) = [\text{sum over } j (p(j) U(\text{dist}|perc ==j))]$$

The information about the disturbance given the perceptual signal is

$$I(\text{dist}|perc) = U(\text{dist}) - U(\text{dist}|perc)$$

There's a lot more I would want to know, in any particular circumstance, but the approach is the same in each case. For example, as I said above, I'd do this for all possible delays between the disturbance signal and the perceptual signal, and look at the maximum value of  $I$ . Then I'd like to add the contingency of prior values of the perceptual signal, which amounts to asking about the predictability of the disturbance (as in your sinusoidal tracking experiment). The methods are easily extrapolated from the above.

> Now I want to do the research. How do I measure the  
>uncertainty of the disturbance given the perceptual signal?

I don't think you have direct access to the perceptual signal, so you have to do it indirectly, and make some assumptions that might not be correct. In a simulation model, of course, you can do it directly.

My first approach would be to determine, if you can, what environmental variable corresponds to the perceptual signal in question. You have the Test for this. Then I would try to find a controlled variable that uses this one as an input under conditions in which this one cannot be controlled. In other words, do a psychophysical experiment on it, where there is no mixing of output with disturbance in the CEV. This part of the study indicates the information rate that can be achieved between the CEV and the perceptual signal, and it can be used in particular to determine trade-offs between time and perceptual resolution for minimal fluctuations of the CEV. You still can't observe the perceptual signal directly, but you can determine an upper bound on the rate at which it can acquire information from the CEV under conditions where fluctuations in the CEV are completely due to disturbance. One experiment I mentioned some long time ago (by Jan Schouten), and that was described in a reprint I sent you, gave an information rate of 140 bits/sec or thereabouts for the perception of which of two moderately

bright lights was turned on. The same methods can be used for most variables that the experimenter can affect.

The possibly incorrect assumptions are that the perceptual signal used by the subject in the psychophysical study is the one that is used in the control study, that there is the same division of attention (whatever that means) in the two studies, and that the output is not a limiting factor in the psychophysical study. There may be others, but these spring to mind. The only way I can think of to deal with them is to do converging studies, studies that address what ought to be the same question from different directions. They work only within the framework of some theoretical construction (such as a specific proposed control hierarchy), so they also may be vulnerable to unwarranted assumptions. But if they all give much the same answer, both the assumptions and the answer gain credibility, since properly chosen converging studies are dependent on different assumptions.

>I want some disturbance uncertainty measures and I want them NOW!!

If you have data, do as I describe above, and you've got it.

Martin

Date: Tue Mar 01, 1994 11:52 am PST  
Subject: Re: Defining information

<Martin Taylor 940301 13:45> >Bill Powers (940301.0900)

>I have not been able to find in my own direct experience anything  
>that corresponds to H or Pi, except for a rather confused set of  
>indefinite images that change with circumstances. So I do not  
>believe in information or probability: they are just words, for  
>me. I can observe nothing in my world that corresponds to them.

I believe you. I also believe that I have been able to find nothing in my direct experience that corresponds to an atom, or an electron, or a jet stream, or an artificial earth satellite, or a radio wave. Unlike you, however, that does not induce me to disbelieve in them.

I also believe I have never cut something in half. Half is just a substitution operation, vaguely like something that does happen, but different under different circumstances. Nevertheless, I do not disbelieve in the operation of halving as a mathematical ideal, even though I find nothing in my experience that corresponds to it except in these vague ways.

I have never seen a Euclidean plane, but I do believe that the operations of Euclidean geometry are useful in this vague and approximate world. And that is despite the fact that the even more esoteric geometry of Reimann applies more closely to our real world. A "perpendicular pair of lines" is, after all, only a set of words, with no true correspondence in real experience. And "parallel lines," what are they?

I have never seen an object that maintains its velocity after being pushed once, but I accept that Newton's equations better describe the way our world works than do Aristotle's ideas. Many people don't accept this, as experiments on "intuitive physics" show. The attitude of those people toward Newtonian physics strikes me as

very parallel with your attitude toward Shannon. His mathematical ideal uncertainty is not exactly your intuitive one, any more than the intuitive trajectory of a stone released from a sling is the mathematical Newtonian one. You include some concept of the personal value to you of an event in your perception of uncertainty. These moral constructs don't enter into the mathematics of uncertainty, any more than air resistance figures in the conic sections of classical Newtonian dynamics.

"Perception" in PCT is not the perception I experience every day. Should I therefore "not believe in" the PCT version of perception? I have never had direct experience OF a neural signal, even though my experience may be a consequence of lots of them. Should I disbelieve in them? "They are only words, for I can observe nothing in my world that corresponds to them."

Martin

Date: Tue Mar 01, 1994 5:14 pm PST  
Subject: Re: Uncertainty of disturbance

[From Rick Marken (940301.1500)]

Me:

>How does one measure the uncertainty of a disturbance given the perceptual  
>signal??

Martin Taylor (940301 12:00) --

>Well, if you ever asked THAT question before, I guess I missed it. That's  
>an easy one

... lots of details...

>The information about the disturbance given the perceptual signal is

> $I(\text{dist}|\text{perc}) = U(\text{dist}) - U(\text{dist}|\text{perc})$

Thanks. That's how I THOUGHT you computed it.

>If you have data, do as I describe above, and you've got it.

I have data. But before I take the trouble of calculating  $I(\text{dist}|\text{perc})$  why don't you tell me:

What you would expect to find from calculating this  $I(\text{dist}|\text{perc})$ ??

In particular, what do think would be the relationship between  $I(\text{dist}|\text{perc})$  and some measure of control, such as RMS error? Why do you think that such a relationship would exist?

If you answer the above questions, I will make you a present of an "I(dist|perc) Calculator" stack, the perfect companion to the "Logic Renunciator" stack.

Also, turn to page 72 in "Mind Readings" and note the measures of

control (in terms of RMS deviation) in conditions labelled "closed" and "open". In those two conditions,  $I(\text{dist}|\text{perc})$  was ALWAYS the same (for each row) because cursor movement in the "open" condition was a replay of cursor movement in the "closed" condition. But control is ALWAYS at least twice as good in the "closed" condition as it is in the "open" condition. So here we have a HUGE difference in behavior when there is NO difference in the information about the disturbance,  $I(\text{dist}|\text{perc})$ .  $I(\text{dist}|\text{perc})$  doesn't look like one of those real important measures of control to me.

Bill Powers (940301.0900 MST) --

>I do not believe in information or probability: they are just words, for  
>me. I can observe nothing in my world that corresponds to them.

Gee, Martin, looks like I'm not the only one who doesn't see the obvious relevance of information theory to PCT.

Best Rick

Date: Tue Mar 01, 1994 5:28 pm PST  
Subject: What Bill (might have) said

[From Rick Marken (940301.1600)] Martin Taylor (940228 19:15)

>Even Bill P. in Durango went so far as to say that I was almost the  
>only one who did NOT say that his theory encompassed, foreshadowed, or  
>otherwise took in PCT, but instead acknowledged that his own theory could  
>be seen as a consequence of PCT.

Bill P., because he is such a sweetheart, often leaves out the second part of his thoughts. I'm sure Bill did mean that you are, indeed, "...almost the only one who did NOT say that his theory encompassed PCT"; what Bill judiciously left out is "but you are another in a long line of people who mistakingly think that their theory is consistent with PCT. In fact, information theory is NOT consistent with PCT; it is completely unrelated to it (at best) and contradicts it (at worst)".

Martin, EVERYONE thinks that THEIR theory is consistent with PCT. You are not the only one.

Best Rick

Date: Tue Mar 01, 1994 5:50 pm PST  
Subject: Re: Uncertainty of disturbance

<Martin Taylor 940301 12:00> >Rick Marken (940228.2200)

In my response to Rick's direct question:

>How does one measure the uncertainty of a disturbance given the perceptual  
>signal??



I gave a direct answer. But I think I neglected to point out that the question itself was inappropriate for a control system. It makes sense in the absence of control, but a more useful question when there is control is

How does one measure the uncertainty of changes in the disturbance given changes in the perceptual signal.

The methods are the same, but the value of the result is quite different. Over any reasonable period of time, the VALUE of the disturbance is more or less independent of the VALUE of the perceptual signal, as we all know. But the derivatives are not independent, at least not for a correct value of the time delay associated with the perceptual input function. So, when there is no control, the value question makes sense, but not when there is good control. For poor control, both questions are probably useful.

Martin

Date: Tue Mar 01, 1994 6:43 pm PST  
Subject: Re: Uncertainty about I(dist|perc)

[From Rick Marken (940301.1730)] Martin Taylor (940301 12:00)

>Over any reasonable period of time, the VALUE of the disturbance is  
>more or less independent of the VALUE of the perceptual signal, as we  
>all know. But the derivatives are not independent, at least not for  
>a correct value of the time delay associated with the perceptual input  
>function.

Are you speaking ex cathedra, Martin? Or are you just making up that stuff about the derivatives NOT being independent for a correct value of time delay? Can you show me the calculations-- or the formula that results from those calculations--which predict this result?

As we PCTers always say "Being a PCTer means never having to make up facts".

I still anxiously await your answers to my question about how I(dist|perc)-- which I suppose is now I(ddist|dperc), where ddist and dperc are the derivatives of dist and perc, respectively-- relates to the ability to control in a tracking task. Also, measures of I(ddist|dperc) at ANY delay are still exactly the same in the "open" and "closed" conditions of the experiment reported on p. 72 of "Mind Readings". So we still have a BIG difference in performance with NO difference in the uncertainty of the derivative of the disturbance given the derivative of the perceptual signal. HMMMM. Curiouser and curiouser, AGAIN.

Best Rick

Date: Tue Mar 01, 1994 8:53 pm PST  
Subject: Rick reading my mind

Hello, Martin --

>I am considerably disturbed by the most recent interchange with  
>Rick, especially if he reads your mind correctly.

I don't think that anyone ever understands anyone else completely, much less reads their minds. I try to understand your point of view, and Rick's, and to guess what the failure of communication is. But I can only guess. I have enough problems in trying to mesh your way of thinking with mine.

Our universes intersect, often very satisfyingly, but they are far from congruent. You believe in a whole constellation of ideas that mean little to me. To me, abstractions are not real; experience is real, and abstractions are only pale echoes of experience, attempts to formalize what comes to us unformalized. This is why I insist on testing abstractions by making them do something, so I can relate them to an observation, an experience, not just to the internal logic of some mathematical system. I will say more in responding publicly to your second post.

Between you and Rick, I assign a good part of the blame to Rick, in some respects the greater part. He has pigeonholed you. He suspects that you believe disturbances to cause behavior; that whether you know it or not, you're defending the S-R picture of behavior, in which behavior is incomprehensible if it can't be traced back to external causes. He has, I think, simplified your views; instead of saying, as I do, that I don't understand your approach, he feels (unjustifiably, in my opinion) that he does understand your approach and knows that it is wrong for PCT. He has turned your rather indefinite statements (as they seem to me) into definite, and much simpler, statements which you did not make.

I think that the only way you will ever convince Rick is to show him exactly what relationship you see between a disturbing variable and a controlled variable, and the other variables in a control system. You often speak of computations that could be done, but until you actually do them, and show how you came up with them and how someone else could reproduce them, Rick will continue to believe that you're pulling a fast one (and I will continue to believe that you haven't made your case).

I think there's little point in arguing either with Rick or with me. You know the kind of prediction that I am impressed by, and that Rick obviously wants. If you can eventually come up with convincing quantitative predictions or retrodictions, there will be little more you need to say. If you can't, then it's unlikely that your approach will be adopted either by Rick or by me, even though others might find your results perfectly satisfactory. If you're sure you're right, you shouldn't allow irrational arguments against you to cause you any anxiety.

I think you understand PCT very well, well enough to represent it to others and to teach it very effectively. I think also that in some practical applications you haven't yet seen all the nuances (for example, you only very recently understood that the integration factor  $k$  in the model was associated with the output, not the input function). Your rather Pythagorean attitude toward mere simulations and demonstrations doesn't exactly fill me with confidence that you are familiar with all the interesting relationships between variables that flow from the model. But that could be an appearance only. How can I know what you understand, except about specific things?

PCT is certainly a work in progress, and you will no doubt make contributions to the growing structure. No matter what we disagree about, I would hate to see PCT deprived of your participation.

Bill

Date: Tue Mar 01, 1994 10:29 pm PST  
Subject: Re: Defining information

<[Bill Leach 940301.22:50 EST(EDT)] >[Bill Powers (940301.0900 MST)]

Bill; would I be far afield if I guessed that you enjoy and appreciate the writings of Ben Franklin?

-bill

Date: Tue Mar 01, 1994 10:35 pm PST  
Subject: Meanings and experience; measuring I(dist|perc)

[From Bill Powers (940301.2200 MST)] Martin Taylor (940301.1345)

>>I can observe nothing in my world that corresponds to [H or P].

>I believe you. I also believe that I have been able to find  
>nothing in my direct experience that corresponds to an atom, or  
>an electron, or a jet stream, or an artificial earth satellite,  
>or a radio wave. Unlike you, however, that does not induce me  
>to disbelieve in them.

I use a three-level distinction here: I believe, I do not believe, I disbelieve. I do not believe in information or probability; neither do I disbelieve in them. I simply find no connection between these quantities and anything I can experience.

Atoms, electrons, jet streams, etc., are symbols which can be reduced to other symbols by formal rules, and in the final reduction the symbols have direct experiential meaning: the instrument-readings of experimental science.

I admit that the distinction I'm trying to draw is not always clear.

>A "perpendicular pair of lines" is, after all, only a set of  
>words, with no true correspondence in real experience. And  
>"parallel lines," what are they?

Perpendicular lines are those that look as if they meet at a right angle, as near as we can see. And parallel lines are lines that look as if they run in the same direction. I can point to places in the world where I see things that provide meanings to go with these words. The meanings are not those of geometry or logic; that is a problem of definition in a formal system, not meaning.

>I have never seen an object that maintains its velocity after  
>being pushed once, but I accept that Newton's equations better  
>describe the way our world works than do Aristotle's ideas.

"Velocity" is a term that has a meaning for me: the meaning is the perception of motion. I also have a meaning for "force," a clear perceptual meaning. These meanings tie velocity and force to the world of experience. Given symbols for  $v$  and  $f$ , I can accept that they have meaning to begin with, and so have a way of interpreting symbolic operations using these variables -- whether they express

Newton's or Aristototle's theories. I can also arrange for more formal observations of  $v$  and  $f$ , now that I am convinced they have counterparts in experience. I am not talking about theoretical relationships among variables; I am talking about accepting the very existence of those variable as having real-world, experiential meaning.

>"Perception" in PCT is not the perception I experience every day. Should I therefore "not believe in" the PCT version of perception?

I suspected not. For me, it is nothing else. Perception IS the world I experience every day, every second. What I mean is that

[What it is to experience a perception] =  $p$ .

This gives the symbol for a perceptual signal meaning for me. I can then go on to say

$p$  = neural signal at  $x$  impulses per second,

where the statement on the right is theoretical. In the model I can manipulate expressions involving  $p$ , with confidence that at any time I can interpret what the equations say in terms of what it is to experience a perceptual signal. Whatever the model says about  $p$  must be true about the experience of a perception, or the model is wrong. If the model predicts that  $p$  is stabilized near a particular value, then if it is to have anything to do with reality, I must experience the world as being stabilized near some state.

>I have never had direct experience OF a neural signal, even  
>though my experience may be a consequence of lots of them.  
>Should I disbelieve in them?

There is nothing to believe or disbelieve about neural signals: they are entities in a model defined by a series of equivalence statements, expressions showing what can be formally substituted for what. The symbols we use to designate neural signals, however, must be given an experiential meaning before they can be said to pertain to the world. This is done by reducing the mathematical expressions to forms in which the only variables left must be parts of meaning-expressions, not equivalence-expressions. If it is not possible to find such a form, the mathematics has no meaning.

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Rick asked

>>How does one measure the uncertainty of a disturbance given  
>>the perceptual signal??

>Well, if you ever asked THAT question before, I guess I missed  
>it. That's an easy one ...

But it turns out that it is an easy one for Rick to carry out, not for you to carry out. Actually, you are sitting on a large amount of raw data right now, and it would be no trick at all for you to implement the calculations you describe to Rick, and come up with the numbers yourself (when the current experiment is finished). From our analyses so far, the optimal delay can probably be taken to be about 1/6 second -- that should be close enough. Any conclusions that are drawn will be only mildly dependent on getting the exact delay.

Since the value of the reference signal comes out very close to zero when control is good, we can take the state of the controlled variable to represent the perceptual signal 1/6 sec later.

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I am pleased to see that you are actually reducing the concepts of IT to observables. Instead of probability, a term that means little to me, you now are speaking in terms of how often the values of variables take on particular values. We can obviously compute a formal measure,  $p \log p$ , where  $p$  is defined as the number of times the variable had a given value divided by (the total span of values times the total number of trials) (is that right?).

Your thought-experiment plot of disturbance against perceptual signal is rather optimistic, in that it shows a strong slope up and to the right. The actual plot you will get will be more like an elliptical distribution centered on the origin, with the long axis almost horizontal and the short axis being much smaller than the long axis. We already know that the correlation of the controlled variable with the disturbance will be very low when tracking is good: 0.2 or less, for medium difficulty (say, a bandwidth of 0.4 Hz and simple compensatory tracking). The ability to predict the disturbance given the perceptual signal will be very low. I have seen many runs in which this correlation was less than 0.01.

Rick is no doubt setting up to do these plots and calculations right now. I think you should do them, too, to check that he is doing it right.

Embellishments can come later. Let's do this simple calculation first. What we learn from it will tell us what's worth doing next.

Best, Bill P.

Date: Tue Mar 01, 1994 10:45 pm PST  
Subject: PCT PCT PCT PCT

<[Bill Leach 940301.23:07 EST(EDT)] >NET

Sorry everyone but looks like am not going to be a bit later with my first posting of my compilation of a dictionary for the PCT net. Excuses but have a couple of engineers over from Europe and they have been demanding a bit of my already short time.

-bill

Date: Tue Mar 01, 1994 10:56 pm PST  
Subject: Re: Rick reading my mind

<[Bill Leach 940302.01:01 EST(EDT)] >Bill Powers 01 Mar 1994 21:50:18

Fortunately, that was typically (I believe) yet another thoughtful and considerate postings. This is especially fortunate since what was obviously intended to be a private message was instead a public post.

For the record, I agree with you in that demonstratable, repeatable evidence needs be provided.

-bill

Date: Wed Mar 02, 1994 4:11 am PST
Subject: European Meeting, Learning

[From Marcos Rodrigues (020394.12:00 GMT)]

Two things about our CSG European Meeting, June 23-27, Aberystwyth, Wales:

- a) I expect to hear about funding to support our meeting tomorrow, as the committee meets today. I'll let you know the outcome.
b) The official letter of invitation (for those of you who have made a request) will be in the post before this Friday.

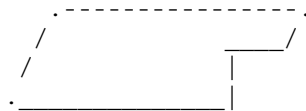
Learning:

There is an exciting oportunity to apply PCT in a learning situation as described below. I would appreciate any thoughts you may have.

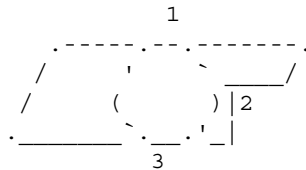
The task: a robot manipulator is to grasp an object with unknown shape either using a two, three, or four-finger gripper. Changing the gripper is simple; the real problem is to determine which gripper would be more appropriate for a given shape, and the exact gripping points.

Given: we have a vision system that can automatically select a shape and determine the exact position of the centroid. Therefore, the dimensions of the shape are known, and it could be determined if the object can be grasped (that is, whether the selected grasping or contact points are between the tolerance existing between a full open gripper and a full closed gripper).

The approach: consider an object with a generic shape as follows.



As said, the vision system can highlight the object and detect the centroid. The approach to determine how many contact points and where they are localized is: starting from the centroid, try to fit a circle so that we have equidistant points of contact (2, 3, 4, or more points). The figure below does not reflect it, but suppose each of the contact points (1,2,3) is 120 degrees apart from its neighbouring point. If that is the case, then the gripper to be used is a three-finger, and the gripping points are exactly as indicated.



The robot then will rotate its end effector so that the orientation of the gripper finger is as above. No problem with that, once you know where the points are.

I want the points to be automatically detected when any shape is presented to the vision system, and would like this circle fitting to be described as a control of perception process. There are several control variables: the size of the circle (minimum and maximum are given) the number of contact points (minimum 2, maximum 4), equidistance, the centre of the circle does not necessarily must coincide with the area centroid (that is, the above circle could be translated to the left or right), more than one possible fitting (in that case, look for smoother contact points) etc.

In special, I would like to be able to explore learning paradigms, such as reinforcement learning a la PCT, or Hebbian learning, or any other.

Any thoughts on the method above, alternatives, learning, etc would be greatly appreciated.

Kind regards, Marcos.

Date: Wed Mar 02, 1994 7:31 am PST  
Subject: Re: What Bill (might have) said

[Dan Miller (940302.1000)] <Rick Marken (940302.0239) Rick and Martin and Bill):

I have been working hard to read all the posts on Information Theory and Perceptual Control Theory. It has not been easy. Not only are the ideas slippery, but the scurrilous quality of the posts distracting.

Rick, in your last post you said:

<Martin, EVERYONE thinks that THEIR theory is consistent wi PCT.  
<You are not the only one.

If I am not mistaken, then I am included in the "EVERYONE" clause. So, I suppose I should say something on my behalf. I do not think that "my theory" (whatever this could be) is consistent with PCT.

I am trying to understand human social activity. I have studied lots of theories. Most people call me a symbolic interactionist, however, that is not accurate. I embrace PCT because it is the best "theory" that I know of to help me make sense out of the range of problems I have defined. The most any of us sociologists have said about "their theory" is that Dewey and Mead had developed an early, pimitive version of a feedback model of behavior and perception. They did not develop, nor does their work presage PCT. Rather, there is an elective affinity between some sociologists and PCT.

The assumption, continually made, is incorrect.

One final note - in an earlier post (I lost it somehow) Rick made the assertion something like, PCT means never having to invent facts. Do you really believe this? How do facts happen if they are not constructed (invented)?

Later, Dan Miller

MILLERD@UDAVXB.OCA.UDAYTON.EDU

Date: Wed Mar 02, 1994 8:30 am PST  
Subject: U & I = H

From Tom Bourbon [940301.1635] >[Bill (940301.0900 MST)] >>Martin (940228.1915)

>>The definition of information, in Information Theory terms  
>>is quite straightforward.

>There are two kinds of definition here. In one, there are symbols  
>on both sides of the equal sign, as in

>H = -SUM(Pi\*log(Pi)) (the i is a subscript)

>In the other, there is a symbol on one side and an experience of  
>a perception, which in communication we must indicate by other  
>symbols that refer to it, on the other side:

>[What it is to perceive information] = H.

Bill went on to discuss his ideas about differences between the two kinds of definitions. To which Martin replied in:

<Martin Taylor 940301 13:45> Re: Defining information

Martin attempted to deflect Bill's comments about definitions of information.

Also, in

<Martin Taylor 940301 12:00> >Rick Marken (940228.2200)  
Re: Uncertainty of disturbance

Martin replied to Rick's question:

>How does one measure the uncertainty of a disturbance given the perceptual  
>signal??

Martin said, "That's an easy one," and laid out the procedure.

I have a simple suggestion for Martin. Now that you have laid out the procedure for calculating the uncertainty about a disturbance given the perceptual signal, why not use the procedure to answer Rick's long-standing "challenge" or "opportunity" and perform that calculation on the tracking data he provided to you? That way, you could demonstrate a direct application of information theoretic measures to a PCT task.

By so doing, you might provide stronger support for your idea that IT is important in PCT -- stronger support than came from your use of the simultaneous PCT algebraic equations to solve for one unknown. In that earlier demonstration, you did not use information theoretic measures in the Shannon sense. Instead, you used "information (factual knowledge) about" the known variables in the system of equations.

Can it be done?



Later, Tom

Date: Wed Mar 02, 1994 8:34 am PST  
Subject: Re: U & I = H

<Martin Taylor 940302 11:20> >Tom Bourbon [940301.1635]

>I have a simple suggestion for Martin. Now that you have laid out the  
>procedure for calculating the uncertainty about a disturbance given the  
>perceptual signal, why not use the procedure to answer Rick's long-standing  
>"challenge" or "opportunity" and perform that calculation on the tracking  
>data he provided to you?

Better than that, I hope, will be to apply them to the tracking data from the sleep study, were there are thousands of tracks rather than hundreds of data points, and where we (will?) have (well?)-fitted models for most of them. For these data and models, we should be able to get pretty good measures.

The limitation on when I can do this is imposed by my programming speed and the fact that I have only odd half-hours free to do anything interesting until the sleep-loss study is over. Right now I'm on duty again in ten minutes or so, until later evening. I may get a few short periods when nothing is happening, to check the mail and perhaps make quick responses, but there's no time for anything serious.

Martin

Date: Wed Mar 02, 1994 10:43 am PST  
Subject: Private mail goof; robot gripper; why statistics?

[From Bill Powers (940302.0800 MST)] Bill Leach (940302.0101)

>This is especially fortunate since what was obviously intended  
>to be a private message was instead a public post.

You're right and I was lucky to have been exercising a restraint that is not always present in private postings. Fortunately, Martin Taylor is not ruffled much by mere libel.

As to Ben Franklin, haven't read anything by him since I was a kid. Time to check him out again?

-----  
Marcos Rodrigues (020394.12:00 GMT) --

You seem to have made excellent progress on the vision system. I imagine that you've spent a lot of time reaching out and gripping things to see what perceptions are involved; that's what I would do (and have been doing).

>I want the points to be automatically detected when any shape  
>is presented to the vision system, and would like this circle  
>fitting to be described as a control of perception process.

>The robot then will rotate its end effector so that the  
>orientation of the gripper finger is as above. No problem with  
>that, once you know where the points are.

From the way you present the problem, and from what you haven't said, I am guessing that the gripper fingers are constrained to move together, and that they always lie on one circle. So you need to position the gripper so 2, 3, or 4 points of contact are made simultaneously as the fingers converge toward a center of grip. Or are you speaking of different grippers with different numbers of fingers in fixed angular positions, to be selected in real time according to the outline of the object? Or are you speaking of solving a two-finger case, a three-finger case, and a four-finger case as possible alternatives for a single final gripper design?

I can see that gripping can involve a problem of the slopes of the surfaces gripped (the picking-up-a-wet-bar-of-soap problem). This implies satisfying several conditions at the same time:

1. All fingers of the gripper must contact the outline of the object.
2. As the gripper fingers close, there is no tendency of the object to rotate.
3. As the gripper fingers close, there is no tendency of the object to translate.

Suppose we set up a model of the object having the same outlines. In this model, the gripper fingers slowly close until one finger makes contact with the model. A one-way control system comes into play which prevents that finger from moving through the outline of the object by moving the x and y location of the center of grip, and the angle of rotation of the gripper. There is one such control system for each gripper finger. Each control system must automatically switch polarity if the error correction tends to increase the error (this takes care of finding minima). The effect of error on the three output quantities (x, y, and theta) must always be such as to decrease the error, around each possible feedback path.

The final result, if reachable, will be that the object will tend neither to rotate nor translate as the gripper fingers are closed. The outputs of the model will be x, y, and theta that meet this condition, if possible. Failure to meet the condition (the bar of soap squirts out from between the fingers) might call for a retry from a different starting orientation.

If the gripper fingers had sensitive tactile receptors, this method could be used in real time without running a model.

That's a top-of-the-head guess at a start toward one part of solution that you want.

-----  
Martin Taylor, Cliff Joslyn --

... and everyone out there thinking in terms of probabilities, likelihoods, variety, information, etc.

I woke up this morning wondering why the language of probability is being used for describing the behavior of a basically deterministic system. The only answer I could come up with is that many people are still thinking of behavior as being

fundamentally unpredictable in detail, and are using the methods of analysis designed for that situation.

Statistics was introduced in psychology because predictions of behavior under existing models were so poor -- behavior seemed to have a large random component. I can understand using the methods of statistics for phenomena that consist of a little signal and a lot of noise. But under a different model, we can see that behavior is systematically related to disturbances and reference signals, with a lot of signal and very little noise. This is especially true in our simple tracking experiments. Why not just use a deterministic model, with noise introduced as a second-order (and largely optional) consideration?

Best to all, Bill P.

Date: Wed Mar 02, 1994 12:23 pm PST  
Subject: Why I'm such an asshole

[From Rick Marken (940302.0930)]

A post from Bill Powers to Martin Taylor that was mistakenly placed on the net gives me an opportunity to explain why I am pursuing this IT vs PCT argument with such monomaniacal tenacity.

Bill said:

>Between you and Rick, I assign a good part of the blame to Rick,  
>in some respects the greater part.

And I assign it all to me. Heck, I'm the one who's chomping down on this IT stuff full tilt. Mea culpa. Mea maxima culpa.

Bill continues:

> He has, I think, simplified your views; instead of saying, as I do,  
> that I don't understand your approach, he feels (unjustifiably, in my  
> opinion) that he does understand your approach and knows that it is  
> wrong for PCT. He has turned your rather indefinite statements (as they  
> seem to me) into definite, and much simpler, statements which you did  
> not make.

I think Bill is being a bit generous to Martin here. In fact, I have only responded to statements that are quite specific and quite specifically wrong. The claim that there is information about the disturbance in perception was shown to be clearly wrong. The claim that control systems base their outputs on information about the disturbance in perception was shown to be wrong. The latest claim is that "the derivatives [of the disturbance] are not independent [of the derivatives of the perceptual signal], at least not for a correct value of the time delay associated with the perceptual input function". [Regarding this last claim, Martin said to me, in a private post "Ask Bill P. It's the way he computes the best value of model delay for our tracking studies. Works fine. Makes sense to me, if not to you." Well, I asked Bill P. (though I didn't need to) and found that there is a comparison of the derivative of the output to the derivative of the disturbance but, of course, no comparison of the derivative of the disturbance with respect to the derivative of the perceptual variable].

All of Martin's SPECIFIC mistakes are made in the service of preserving a belief in information theory -- a theory of human behavior that Martin was attached to prior to discovering PCT. Perhaps, now that he has presented specific calculations that can be performed to obtain these information measures, Martin will see that they are completely useless as a basis for understanding anything about control. But, given the results of our demonstrations that there is no information about disturbances in perception, I am not particularly optimistic.

Which brings me to the question "Why am I such an asshole"? The reason is: I want to show the "nothing but" syndrome in action. I have no illusions about changing Martin's mind (or anyone else's, for that matter) about the relevance of their previously held theories to PCT. People who come to PCT usually come with a prior commitment to some theory; they see PCT as consistent with that theory (indeed, that's usually why they come to PCT in the first place; they see PCT as consistent with what they already believed). The problem is that these previously held theories ALWAYS (so far) contradict PCT in some way or another. Martin's mistaken beliefs about how control works are just a particularly clear (and public) example of this problem.

Believing that some existing behavioral theory is consistent with PCT is just version of the "nothing but" syndrome (as described in BCP). It keeps people from doing real PCT and it makes PCTer's who point out the inconsistencies between existing behavioral theories and PCT look like assholes -- ergo, I am probably perceived as an asshole. And why not. Here are people who are enthusiastic about PCT (and PCT needs all the friends it can get) and what thanks do they get for their loyalty?; what kind of welcome to PCT? a kick in the teeth from Marken the fanatic. I would say that that's being an asshole, alright.

The problem is, I like being an asshole (I'm so deluded, I actually think I am just being honest -- like the little creep who ran around saying that the emperor had no clothes on) . I have been doing PCT for almost 20 years now. During that time I have seen many people become enthusiastic about PCT; but they have almost always been the Carver's and Scheier's and Hyland's and Locke's and Heise's and so on. None of these people have made ANY contribution to the development of PCT science -- because none of these people would go "all the way" and admit that PCT was COMPLETELY NEW, even though it had vague similarities to "goal theory" or "drive theory" or "cognitive theory", etc.

I am an asshole, alright, but I am not doing it to change the minds of those who will remain committed to their pre-PCT theories. I know that they won't change their minds (probably not, anyway). I do it for the sake of the people who are the real hope of PCT -- the students who are not yet committed to a particular point of view. I do it for the youth. I do it to let these young people know that PCT is a whole new ball of wax; that you can't understand PCT from the Procrustean perspective of existing theories. I want these young people to know that it is IMPORTANT to learn about past attempts to understand human behavior -- to learn about Mead and Dewey and James and Hull and reinforcement and information theory, etc -- not in order to learn about human behavior from these theories (do we learn about physics from Aristotle's theories?) but to learn what we had to go through to get on the right track -- PCT. The potential gain of one young person who can really do PCT is worth (to me) the risk of alienating lots people who do not like to be wrong.

Dan Miller (940302.1000)--

> in an earlier post (I lost it somehow) Rick made the assertion something  
> like, PCT means never having to invent facts. Do you really believe this?

Yes.

> How do facts happen if they are not constructed (invented)?

I don't understand what this means. Facts are perceptions that we can have under certain well specified (is that what the constructed part means?) conditions. In the post you lost, Martin Taylor (940301 12:00) made the following claim:

>Over any reasonable period of time, the VALUE of the disturbance is  
>more or less independent of the VALUE of the perceptual signal, as we  
>all know. But the derivatives are not independent, at least not for  
>a correct value of the time delay associated with the perceptual input  
>function.

Martin is describing a perception that one could have under certain well specified conditions: the perception is of derivatives of the perceptual signal being DEPENDENT on the derivatives of the disturbance a some delay. Martin claims that such a dependence exists; that it is an observable fact. Now that he has tons of data he will be able to make the required observations; he will then find what Tom and Bill and I have already found; that there is NO such dependence. So Martin's statement above is an example of an INVENTED FACT. It is NOT a FACT at all. It is equivalent to saying that when you drop a ball from the top of the leaning tower it instantly reaches terminal velocity and proceeds at that velocity until it hits the ground. If it is "scurrilous" to point this out, then I suppose I'm not just an asshole; I'm a "scurrilous asshole".

Best Rick

Date: Wed Mar 02, 1994 12:35 pm PST  
Subject: Conditional probability calculation: addendum

[From Bill Powers (940302.1300 MST)] Martin Taylor (940301.1200)

Addendum to preliminary analysis.

The correlation of the disturbance with the delayed perceptual signal is 0.225.

The correlation of the derivative of the disturbance with the derivative of the delayed perceptual signal is 0.268.

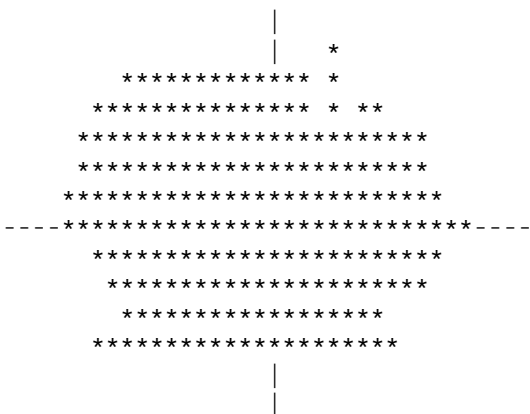
Best, Bill P.

Date: Wed Mar 02, 1994 1:23 pm PST  
Subject: preliminary results, conditional probability

[From Bill Powers (940302.1210 MST)] Martin Taylor (940301.1200)

Preliminary results in developing computation of conditional probability:

When I plot the first derivative of the disturbance against the first derivative of the optimally-delayed perceptual signal (delayed value of cursor position) for experiment 2 (compensatory tracking) and disturbance UDIS4B.BIN, I get a plot like the following:



The plot is roughly circular and is centered on 0,0.

Putting the derivatives in bins for the two axes, I get apparently symmetrical distributions centered on 0 and having roughly a bell-shaped curve. The distributions are as follows:

1. derivative of disturbance:

deriv	number
-10	0
-9	1
-8	6
-7	9
-6	21
-5	25
-4	50
-3	103
-2	237
-1	619
0	1458
1	601
2	255
3	102
4	49
5	23
6	15
7	5
8	2
9	1
10	0

2. Derivative of cursor delayed 11/60 sec (found by program):

deriv	number
-------	--------



>  
>The plot is roughly circular and is centered on 0,0.

This, Dan, is a non-invented fact. It is a useful fact because, even though it is (as you note) "constructed", the result of the construction is what you see, rather than what Martin Taylor (940301 12:00) said that you see, viz:

>the derivatives are not independent, at least not for a correct value of  
>the time delay associated with the perceptual input function.

which implies a plot that looks like:

```

                **
              ****
            ***
          ****
        ****
      ****
    *****
  ****
***
**

```

See the difference.

So, in order to convince us of the usefulness of information theory, Martin said that something is a fact (the derivatives are dependent) which is not a fact (the derivatives are independent).

I guess this is another reason why I am such an asshole. I can't count the number of times Bill, Tom and I have had reserach papers rejected by reviewers who knew the "facts" about how control works. I remember one reviewer who rejected my original "Selection of consequences" paper because he "knew" that the result of a response was always more likely to be a "better" direction than the current direction of movement (this was in the "e. coli" experiment ["Mind Readings", p. 79] that is available on Dag's PCT Programs disk). I guess having false facts thrown up as evidence of how control systems "really" work just brings back bad memories.

Best Rick

Date: Wed Mar 02, 1994 7:48 pm PST  
Subject: Article, Letter, DEMODISK, CSGintro

[From Dag Forssell (940302 1815)]

With Ed Ford's assistance, I have reworked the last part of the article, until it is as good as I know how to make it. Comments welcome. Don't all talk at once!

-----  
People interacting

Exhibit 17 shows a framework for understanding the interaction between two people, whether in conflict or cooperation. Here, two brains are shown, acting in a common



environment (outside the body, of course). Each person is controlling (acting on) some physical variable as that person wants to. If the chosen variables are related or even the same one (say the balance of a tandem bicycle), it quickly becomes obvious that a variable is subject not only to disturbances from the environment in general, (such as crosswind), but also that each persons action becomes a disturbance to the other. Even side effects of independent action become disturbances to the other. (The balance is upset if one turns around to admire the view).

Let us say that person #1 is your associate or your prospective customer, and that person #2 represents another associate (not present), or yourself, or the organization #1 is part of, here thought of as one person (compare exhibit 1). Exhibit 17 provides the framework only; the boxes are not filled in with specific understandings, wants, perceptions, output options etc. Your challenge as a leader is to improve the productivity of person #1. This can best be done by teaching effective thinking.

#### Teaching effective thinking

When you have accepted that we are all living control systems, you realize that actions depend on wants as they relate to present perception. We act in order to affect our perception of what we experience. When you find yourself in a situation where you want to teach a person (a male this time) to think more effectively because you are concerned about how he acts, your knowledge of HPCT suggests that he must work "inside his head" because only he can change his wants and perceptions, which determine his actions.

He will know that you support him from how you teach him to manage his life better in relation to others (#2).

Effective thinking means that he clearly understands his own various wants, perceptions and possible actions and those of others. He can then find a solution on his own or in cooperation with the other. (With your help if needed).

While some action is the reason for teaching, you don't dwell on it. At no time do you criticize him. Your opinions defeat the process of working in his head. His opinions are the only ones that count. You conduct the entire session by asking questions, offering advice only when it is welcome.

As you explore the things he wants, you are not limited to things he mentions. As an experienced person, you can ask about wants that may be related to the one that started the discussion, or reasons for these wants (higher understanding). For instance, if he has an internal conflict\_incompatible wants\_you can ask him about his priorities, which will help him to resolve his conflict.

A basic methodology might be as follows:

- 1) Ask for a meeting
- 2) Ask #1 about his actions and concerns (with #2).
- 3) Ask #1 about his own wants (in relation to #2).
- 4) Ask #1 about what he thinks #2's wants (rules), perceptions, and possible actions (consequences) are.

- 5) Ask him to compare. Does he see any conflict between his own wants and perceptions and those of #2.
- 6) If yes, ask him if he wants to commit to work on a way to resolve the conflict.
- 7) If yes, coach and support him as he develops a plan to change wants, perceptions, capabilities and the environment to eliminate the conflict.

A brief role-play:

Sally and Ben are working on the same team. Ben is often hours late.

1) Sally asked for a meeting.

2) Sally: Do you have any idea why I wanted to talk to you?

Ben: I guess you are upset because I have been late and the team has had to make up for it.

3) Sally: What are your priorities as far as work goes?

Ben: My personal life has been messed up lately. I have been up late nights.

3) Sally: How important is the team's success to you?

Ben: It is very important, but I've had these problems, and you probably feel that I've been lazy.

4) Sally: Ben, what are the consequences if you don't contribute to the team as we have agreed?

Ben: I'll be reassigned and my record will look bad.

3) Sally: Do you want that to happen?

Ben: No.

3) Sally: What would you rather have happen? In other words, what do you want now?

Ben: I want to work with the team and help it succeed.

5) Sally: Will your absences help you stay on the team?

Ben: No, I'll probably get taken off the team soon.

6) Sally: Well, is this something you want to work on?

Ben: I'll work on it. I want to be a part of the team.

7) Sally: What are you going to do?

Ben: I'll have to think about it. Any suggestions?

7) Sally: What are your priorities?

Ben: First, I have to take care of home, then I can work 100% with the team.

7) Sally: How can you take care of your home situation?

Ben: I really need to get my son and daughter-in-law out of the house. I'll help them find their own place this weekend. Thanks for your concern. I am glad you wanted to talk.

It is best to role-play this several times with a totally cooperative #1.

When you have learned the pattern and seen how it compels #1 to think, you can handle diversions.

Things to avoid:

- o Don't ever tell him what you think, but offer facts if he does not know them. If you impose your opinion on him, he perceives your message as an attempt to control him and he will resist. He is concerned about what he wants, not about what you are saying.
- o Don't ask about his feelings, but rather about what causes them, namely his goals and how he presently perceives that he is doing.
- o Don't take over his responsibilities and try to do his thinking for him. Living control systems must do their own thinking in order to function effectively. Your role is to ask questions only and teach when asked.
- o Don't ask him why he has behaved in a certain way. This type of question evokes an excuse and he begins to defend ineffective choices.
- o Don't bring up a negative incident from the past. It is beyond his control at this point.

A first impression may be that this approach is soft and wishy-washy, leaving everything up to your associate, and you powerless. Surely, a leader is supposed to clearly state what she wants to happen, how and when\_set clear goals!

You will find that the approach outlined here is more effective than setting goals for people. (See Soldani, 1989). Through careful and persistent questioning, you help your associates focus their attention on the issues of your choice and help their minds to come up with solutions to what they now agree are their problems. And you become their trusted friend, someone who cares.

Summary

In this brief introduction to PCT and the HPCT model, I have touched on most aspects of HPCT and indicated how much of human experience this model can explain. I have shown a questioning approach to conflict resolution and counseling which

fully respects the other person as an autonomous living control system, facilitating the development of trust, cooperation and high productivity.

In the third and final article in this series, the concept of teaching effective thinking will be expanded to show effective ways to develop team spirit and caring relationships, conduct performance coaching reviews in a supportive way, and sell without manipulation with full respect for your prospect. I will also show how HPCT defines important elements of effective vision and mission statements, and discuss the essence of TQM as a control process.

-----  
Bill P. gave me a counter-suggestion for a letter. Here is a letter, based largely on his proposal. Short and sweet. Any reactions?

-----  
Bill Powers, Head Honcho  
Control Systems Group  
73 Ridge Place CR 510  
Durango, Co 81301-8136

March 2, 1994

Dear Mr. Powers:

I am an engineer with management experience, who has been interested in Total Quality Management and other approaches to management improvement without being completely satisfied with any of them. A few years ago, I became aware of a growing movement in the behavioral sciences toward a new theory of human behavior called "perceptual control theory," or PCT. As I learned about this new approach from its developers, I became so enthusiastic about its potential for management science that I began to organize the curriculum for a seminar to teach the principles of PCT to managers.

I have found that other managers exposed to the principles of PCT become just as enthusiastic about it as I was. Some have used the principles consistently for well over a year now, with good results. This experience showed that my initial impression was correct: PCT provides a way of understanding human behavior and interactions that makes sense to non-theoreticians and has immediate practical uses in the workplace, at all levels of an organization.

You can't know what benefit you might derive from having your people learn these principles, and neither can I unless we discuss what your company needs and how PCT might help. I hope we can have a conversation in which I can answer your questions, relevant to your situation.

On the back of this letter, I have reproduced a short description of the basic application to conflict resolution, which we call: teaching effective thinking. This is the conclusion an introductory article.

I would like an opportunity to show you the principles of perceptual control in action.

signed: Dag Forssell  
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I now have all the elements for the March DEMODISK, and will finish it this weekend. It dawned on me that one way for me to resolve the difficulty with the MINDREAD table taking so long, was to nudge my own reference signal for maximum compression of the disk, and simply include the 18 KB disturbance file with maximum .1 correlation between disturbances. Now, no matter what computer you have, MINDREAD will start right away.

-----  
It does not look like the Engineering Management Journal will publish any more articles, at least not anytime soon. Therefore, it should not be listed as in press. The same appears true of the new

Gary, Please note, and edit references accordingly in CSGintro.doc:

Forssell, Dag C., "Psychological Theory: The Achilles' Heel of TQM" Available from the author (1994).  
Defining useful theory and assessing TQM.

Forssell, Dag C., "Perceptual Control: Useful Management Insight." Available from the author (1994).  
Structure and evidence for HPCT, with basic application of conflict resolution.

-----  
Best, Dag

Date: Thu Mar 03, 1994 9:51 am PST  
Subject: Re: Private mail goof; robot gripper; why statistics?

<[Bill Leach 940303.00:19 EST(EDT)] >[Bill Powers (940302.0800 MST)]

>As to Ben Franklin, haven't read anything by him since I was a >kid. Time to check him out again?

Hummm... In your "spare time" maybe? Franklin is worth reading and re-reading. He understood people well and he was quite methodical in his thinking.

-bill

Date: Thu Mar 03, 1994 9:51 am PST  
Subject: Re: Article, Letter, DEMODISK, CSGintro

<[Bill Leach 940303.08:55 EST(EDT)] >[Dag Forssell (940302 1815)]

Dag; I like the letter. It should "spark interest" the way it is now written.

>Article: Some comments and questions

I sometimes deal with people concerning emotional difficulties that they may be experiencing. What I am talking about is "non-professional" and is not related to my work. These are usually people that I have known for an extended period of time.

Often these problems are quite serious as they relate to such matters as divorce or impending divorce.

My interactions with such people (sometimes both parties to a disagreement) usually focuses on perceptions (even before joining this group). Somehow, I always realized that people make assumptions about what others are concerned with that are not valid.

To try to get to the point... I have found that there seem to be two elements that are important when first starting to work with the people that I have encountered.

The first is to get them talking about their "feelings" and the second is to help them realize that I make no judgements based upon their feelings.

It seems to me that it is necessary to get the "emotional baggage" out of the way and that to do that I have to give them the impression that I don't see anything wrong with their feelings.

Indeed, I don't. The general position that I hold is that "feelings" just ARE and that a person should not self-judge themselves on how they feel about someone or something. Such judgements appear to only get in the way of effectively dealing with problems.

I always try to impress the person with the idea that I value them as a person and a friend (which is easy in my case since that has already always been true).

>no time do you criticize him. Your opinions defeat the process of >working in his head. His opinions are the only ones that count.

I am convinced that sometimes the problem IS that people have incorrect perceptions concerning the opinions of others (though I agree with the bottom line). I have also found that a "direct assault" on feelings and opinion expressing low "self-worth" are usually useful.

>o Don't bring up a negative incident from the past. It is beyond his  
> control at this point.

Feelings about Negative incidents often are a major problem in the present. I suspect that my take on this is in a completely different form than you are talking about. My basic position is that past incidents need to be addressed when they interfere with the present. What needs to be addressed first is, again, the feelings.

Then a determination made by the subject on the question of "Is the matter over and done with or are there really other ACTIONS that should be taken?"

For want of a better term, I am trying to get them to perceive "closure" on past matters that affect the present. If they have done all that can reasonably be done about the matter (in my opinion of course), I try to get them to recognize that what is "done is done" and other than learning from the experience once they have made whatever efforts are/were available to them to correct whatever the believed was wrong it is time to "move on" and forget the past.

I wonder how this fits into your presentation?

-bill

Date: Thu Mar 03, 1994 10:32 am PST  
Subject: "Godfather" PCT

[From Rick Marken (940303.0800)] Dag Forssell (940302 1815)]

>Comments welcome.

OK.

>When you have accepted that we are all living control systems, you  
>realize that actions depend on wants as they relate to present  
>perception.

I don't like this sentence at all. Though it is basically correct, it seems misleading. Action does depend on want -- but when the want is fixed, action depends mainly on circumstance (disturbance). The sentence above sounds very "Glasserian" to me -- it suggests that we "choose" our actions based on our wants. In fact, in most normal circumstances, actions are determined mainly by disturbances to "present [controlled] perception". It seems that this would be the important point for an observer of a control system (like a manager).

>When you find yourself in a situation where you want to teach  
>a person (a male this time) to think more effectively because you are  
>concerned about how he acts, your knowledge of HPCT suggests that he  
>must work "inside his head" because only he can change his wants and  
>perceptions, which determine his actions.

This sounds pretty Orwellian to me. If "you are concerned about how he acts" don't you think PCT might suggest that YOU are the one who should learn to "think more effectively" -- rather than getting the other person to "think more effectively". Sounds like "Godfather" PCT to me.

>He will know that you support him from how you teach him to manage his  
>life better in relation to others (#2).

Thanks you, Don Corlione. I feel so much better now that you have taught me how much more effective it is for me to think of myself as a "team player" in your organization.

> Sally:  
> Ben:

Substitute "Solly (The Negotiator) Corlione" for Sally and "Benny (The Meatball) Shapiro" for "Benny" and see if it plays any different to you.

>A first impression may be that this approach is soft and wishy-washy,  
>leaving everything up to your associate, and you powerless.

Actually, that was not my first impression at all.

Bill's version of the letter is great, of course.

Best Rick (The Asshole) Marken

Date: Thu Mar 03, 1994 11:19 am PST  
Subject: Tom, Rick: ST comments coming

[From Rick Marken (940303.1000)] Cliff Joslyn (940303)

> Was there any reply to my 940224?

Here's the reply I gave -- slightly edited for spelling/grammar:

-----

Cliff Joslyn (940224.1400) --

It looks like you are more interested in a legal deposition than a substantive discussion of models. Tom and Mary's descriptions of "systems science" capture in eloquent detail my own impression of the field. If "system's theorists" want to do PCT then they are free to do it. If we find anything in systems science that helps us study or model purposive behavior, then we will feel free to use it.

You said:

>if there are any ST people who are "doing anything related to  
>understanding the nature of living systems" then they are by definition  
>doing PCT, EVEN IF THEY DON'T KNOW IT or acknowledge it as they should.

I asked:

>How could this be?

You reply:

>Easily. The idea is that (1) an ST person considers the operation of  
>living systems; (2) (s)he considers that feedback may be important; (3)  
>(s)he then uses feedback to describe some interesting result. Bingo.

I don't know what to say, Cliff. You've been on this net for a long time. You must know that there are many psychologists who are intersted in "the operation of living systems" and think "feedback may be important" and use "feedback to describe some interesting result". But, bingo, they are not even close to doing PCT. These psychologists don't understand control, don't build working models, don't test for controlled variables, don't study individuals (but, instead, use feedback language to describe average results over many subjects), take input-output relationships at face value -- in other words, they are not doing PCT at all. People are NOT "doing PCT" just because they use words like "feedback", "living systems" and "control theory". The fact that you believe that they ARE doing PCT - - after this much time on the net -- is a testament to the power of perceptual control (and more than a tad depressing).

-----

>I have a reply to Tom in the works.

I'm looking forward to seeing it. I bet Tom is looking forward to it too. Tom is having a HELL of a time posting to the net from his site. I have suggested to him that I help out by posting his stuff for him until they figure out how to hook up networking systems out there in Texas. I hope he accepts my offer; this is VERY frustrating for him -- especially since he was VERY interested in contributing to



the IT vs PCT thread of the last couple days (I think he has a couple posts in the queue). Fortunately, there has been no response yet to yesterday's (3/2/94) flurry so hopefully he can get back into the fray (in one way or another) before the dust clears.

Best Rick

Date: Thu Mar 03, 1994 11:40 am PST  
Subject: Posts from Tom

[From Rick Marken (940303.1100)]

I just received two posts from Tom B. who is certain that the versions he posted yesterday will appear on the net as soon as I post these. But I think it's actually GOOD to have repetitions of posts from Tom. Heck, I LIKE to read good stuff more than once. Here they are:

----

Date: Wed, 2 Mar 94 11:25:11 CST  
From: "Tom Bourbon" <tbourbon@heart.med.uth.tmc.edu>  
Subject: Lags

From Tom Bourbon [940302.1101]

This post summarizes a brief paper I didn't hand out at the most recent meeting of CSG. It is about correlations between handle positions or changes in handle positions, and other variables measured in a that pursuit tracking task. I calculated the correlations after I introduced various temporal lags (delays) between the handle positions and the other variables.

TIME-DELAYED CORRELATIONS BETWEEN VARIABLES IN A TRACKING TASK:  
ARE THERE HIDDEN CAUSAL VARIABLES?

W. Thomas Bourbon  
Department of Neurosurgery  
University of Texas Medical School - Houston  
6431 Fannin, Suite 7.148  
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tbourbon@heart.med.uth.tmc.edu

Something I had planned to present at  
the annual meeting of the Control Systems Group,  
Ft. Lewis College; Durango, CO; 27 July - 1 August 1993,  
but for some reason I didn't.

The question of whether control might be explained by legitimate stimulus-->effect (independent variable-->dependent variable) models arises from time to time on the Control Systems Group computer net. The question came up late in 1992. In January, 1993, I used data saved from a simple pursuit tracking task to run a few calculations in which I tested the possibility that the positions of the handle and cursor at one moment are more highly correlated with some variable, or transformation of a variable, at an earlier time, rather than at the same moment. I was looking for possible candidates for the role of "previously

undiscovered causal stimulus." I thought that perhaps change in handle position (dh) at some later time might be a "response" to a change in a "stimulus" at an earlier time. These calculations are all from actual data; there is no modeling.

The results of the calculations are shown in the accompanying Table 1. Correlations with lag = 0 are from the person's original data set during the tracking task and are like those reported many times for tracking tasks. For the undisturbed condition, when I introduced temporal offsets for the handle and cursor, their correlation (r) with the position of the target rose from .995, with no delay, to .998 with delays of 133 to 200 msec, which correspond to the perceptual lags used by Bill in his models. The r between handle and cursor declined, slowly, with increasing delays. Correlations between the position of the handle and the change in cursor- target separation were low for all delays, as were correlations between change in handle position and change in cursor-target separation.

For the disturbed condition, correlations were highest with delays from 133 to 200 msec for cursor versus target, and for handle versus the difference between cursor and target (the latter is a measure of the effective disturbance on the controlled relationship of cursor relative to target). Again, the small increase in correlation was maximal during the range of delays corresponding to Bill's modeled perceptual lag. Changes in handle position (dh) correlated poorly with the potential "change stimuli:" change in cursor-target separation, and change in difference between target and disturbance.

In these simulations, there was no evidence for a previously- undiscovered "stimulus," occurring earlier in time and triggering a change in handle position at a later time. I replicated this procedure with several other sets of tracking data. The results were essentially the same as those reported here.

=====

Table 1. Correlations from two runs by a person, whose original data are shown for lag = 0 msec, on a pursuit tracking task with a target that moved slowly, driven by a table of smoothed random numbers. In the first run the cursor was undisturbed; in the second it was randomly disturbed. Each run lasted 60 seconds and each variable was calculated or sampled every 1/30 second; the data record for every variable contains 1800 values. (For pictorial examples of the task and of similar, unlagged, data sets, see W. T. Bourbon, et al. (1990), On the accuracy and reliability of predictions by control-system theory, Perceptual and Motor Skills, 71, 1331-1338.) In the correlations, for every non-zero lag, the original data records were used, with the handle and cursor offset in time by "lag" msec relative to other variables then all correlations were recalculated. (That is, other variables at [time = z] were correlated with handle or cursor at time = z + lag].)

No Disturbance: Mean (c - t) = -0.178 pixels; SD = 2.263  
Disturbance: Mean (c - t) = 0.042 pixels; SD = 3.928

No disturbance:

Variables	Lag (msec)									
	0	33	66	106	133	167	200	533	1500	2000
c vs t &										
h vs t	.995	.996	.997	.997	.998	.998	.998	.989	.889	.758

h vs c	1.000	1.000	.999	.999	.998	.997	.995	.975	.856	.758
h vs c-t	-.141	-.178	-.190	-.202	-.214	-.227	-.239	-.350	-.518	-.550
dh vs c-t	-.303	-.480	-.486	-.496	-.508	-.511	-.511	-.397	-.137	-.068

Disturbance:

Variables	Lag (msec)									
	0	33	66	106	133	167	200	533	1500	2000
c vs t	.984	.986	.987	.988	.989	.989	.990	.984	.897	.831
h vs d	-.722	-.707	-.709	-.710	-.712	-.713	-.714	-.720	-.700	-.677
h vs c	.674	.664	.662	.659	.657	.655	.652	.621	.507	.428
h vs t	.660	.659	.658	.658	.658	.658	.657	.646	.564	.492
h vs c-t	.075	.012	-.000	-.012	-.025	-.037	-.049	-.163	-.345	-.382
dh vs c-t	-.435	-.553	-.557	-.561	-.561	-.563	-.570	-.488	-.178	-.080
h vs d-t	-.993	-.994	-.995	-.996	-.997	-.997	-.997	-.995	-.921	-.853
dh vs d-t	-.064	-.090	-.084	-.077	-.070	-.063	-.057	.013	.165	.210

KEY:

For actual positions of cursor and target on the screen, and of handle relative to center of its range, and for actual values of disturbance:

h = handle; c = cursor; t = target; d = disturbance.

For differences between magnitudes of variables, at times t and t+1:

c-t = difference (cursor - target);  
d-t = difference (disturbance - target);  
dh = change in position of handle, (time z + 1) - (time z).

=====

Is there "information" here that might inform the discussion of information in perception?

Later,

Tom

+++++

Date: Wed, 2 Mar 94 14:55:52 CST

From: "Tom Bourbon" <tbourbon@heart.med.uth.tmc.edu>

Subject: Re: U & I = H

From Tom Bourbon [940302.1444]

It looks as though all is not yet recovered from the January hacking. My posts to csg-l seem to show up on the net about 24 to 30 hours after I send them. With that delay, it is impossible to stay "in the thick of things."

><Martin Taylor 940302 11:20>

>>Tom Bourbon [940301.1635]

>

>>I have a simple suggestion for Martin. Now that you have laid out the  
>>procedure for calculating the uncertainty about a disturbance given the  
>>perceptual signal, why not use the procedure to answer Rick's long-  
>>standing "challenge" or "opportunity" and perform that calculation on the  
>>tracking data he provided to you?

>

>Better than that, I hope, will be to apply them to the tracking data  
>from the sleep study, were there are thousands of tracks rather than  
>hundreds of data points, and where we (will?) have (well?)-fitted models  
>for most of them. For these data and models, we should be able to get  
>pretty good measures.

The reproduction of my post and your reply arived at the same time. Of course, by then there had been a series of exchanges on this subject between Bill Powers, Rick Marken and you. With the full realization that my remarks are one day out of phase, and will appear on the net yet another day later, I will ask a few simple questions. What will you learn from these calculations? How will they enhance the performance of even the simplest possible PCT model, where performance is measured in terms of agreement between modeled predictions and human data? How will the calculations make a case for the idea that IT \*necessarily\* produces PCT?

My questions are not intended to be hostile. Rather, I finally see you moving close to providing some calculations and predictions from the perspective of information theory and I am eager to learn how those calculations will bear on some of the points that you have only asserted, up to now.

Later, Tom

+++++

Date: Thu Mar 03, 1994 12:53 pm PST  
Subject: Check Out the Intro

[from Gary Cziko 940303.1837]

CSGnetters (both novices and veterans) may want to check out the new, revised (although still under revision) INTRO TO CSGNET which I just posted.--Gary

Date: Thu Mar 03, 1994 1:27 pm PST  
Subject: Re: Tom, Rick: ST comments coming

From Tom Bourbon [940303.1504]

In Message Thu, 3 Mar 1994 10:18:21 -0500,  
Cliff Joslyn <cjoslyn@BINGSUNS.CC.BINGHAMTON.EDU> writes:

>I've been busy getting my dissertation out the door. Was there any  
>reply to my 940224? I have a reply to Tom in the works.

I'll be watching for it. (It seems I can receive mail from csg-1, but the problem comes when I try to post mail. Of course, Gary Cziko says that is a blessing, not a problem -- there are fewer posts loaded with figures, waiting for him to wade through.)

Later, Tom

Date: Thu Mar 03, 1994 2:15 pm PST  
Subject: Re: Le Fact -- conditional probabilities

[Dan Miller (940303)]

Rick Marken (930303.0241)

Rick notes:

>I can't count the number of times Bill, Tom, and I have had research  
>papers rejected by reviewers who knew the "facts about how control  
>works. I remember one reviewer who rejected my original "Selection of  
>consequences" paper because he "knew" that the result of a response was  
>always more likely to be a "better" direction than the current direction  
>of movement (this was in the "e. coli" experiment ["Mind Readings", p.  
>79] that is available on Dag's PC Programs disk) I guess having false  
>facts thrown up as evidence of how control systems "really" work just  
>brings back bad memories.

I couldn't agree more. I have a file folder full of rejection letters with the very same phrases and justifications for nonacceptance. Most of us do.

I have no arguments with your facts (or very few). Rather, the point I was trying to make is that facts are derived from models and purposive action. The better the model(s) works, the more useful the facts.

Later, Dan Miller MILLERD@UDAVXB.OCA.UDAYTON.EDU

Date: Thu Mar 03, 1994 2:34 pm PST  
Subject: Re: Why I'm such an asshole

[Dan Miller (940303.1000)] Rick Marken (940302):

Rick, I would like to clarify a couple of issues.  
In my post (Dan Miller 940302) I asked:  
<<How do facts happen if they are not constructed (invented)?

You replied:

<Facts are perceptions that we can have under certain well specified  
<(is that what the constructed part means?) conditions.

Yes, this is part of what I mean by the constructed nature of "facts." Certainly, facts are perceptions. I would argue that they are perceptions that we generate through our purposive actions. Facts are the perceptual consequences of purposive action. That is, we intend to perceive facts in those specified conditions. We specify the conditions, and the facts are perceived (controlled perception).

Sociologists often are "accused" of making-up facts (or data). My answer to this accusation is, "of course we do. How else could we get these perceptions in this particular form?"

I do not mean to suggest that I am opposed to facts (or data or perceptions of reality); I only mean that we, as scientists and humans, should not arbitrarily separate our perceptual constructs from our purposive actions. Knowledge of specified conditions and facts allow us to anticipate which facilitates control. This is good (for the most part).

Incidentally, do objects dropped from great heights (the stratosphere) continue to accelerate at the same rate as they approach Earth? Is a maximum speed reached, one based on mass and atmospheric resistance? I'm not certain about this, but if so, then Newton's Laws are very conditional.

Earlier in this same post I noted the difficulty I was having reading the PCT/IT discussion. Part of my difficulty was that I found the tone of the discussion to be "scurrilous."

Rick replied:

<If it is "scurrilous" to point this out, then I'm not just an asshole; I'm a "scurrilous asshole."

Perhaps "scurrilous" was too strong a word. In my post I did not intend to describe your character, but rather the quality of the discourse. I do not think you are a "scurrilous asshole." Quite the contrary. I appreciate your spirited defense of your ideas. You, Martin, and others display great passion in defense your positions. I think this is cool. I meant no offense.

Later, Dan Miller MILLERD@UDAVXB.OCA.UDAYTON.EDU

Date: Thu Mar 03, 1994 6:34 pm PST  
Subject: Feelings, Teaching Effective Thinking

[Dag Forssell (940303 1630)] >[Bill Leach 940303.08:55 EST(EDT)]

>I like the letter. It should "spark interest" the way it is now written.

Thanks.

>It seems to me that it is necessary to get the "emotional baggage" out of the way and that to do that I have to give them the impression that >I don't see anything wrong with their feelings. ...The general position >that I hold is that "feelings" just ARE and that a person should not >self-judge themselves on how they feel about someone or something. Such >judgements appear to only get in the way of effectively dealing with >problems. I always try to impress the person with the idea that I value >them as a person and a friend (which is easy in my case since that has >already always been true).

There is nothing wrong with feelings. Your general position is a very popular one, promoted by Marriage Encounter among others, and totally devoid of explanation. Have you read "Emotions" in Living Control Systems Volume II? (The chapter left out of BCP). Did you read Bill P's post on dormitive principles (931003.0030), which is excerpted in my article on theory (930102 2110). How long do you wallow in the feelings of others? It is not very productive, is it? The second part of my statement was to discuss what causes those valid feelings. That is productive!

>What needs to be addressed first is, again, the feelings.

The only PCTer with real practical experience who has written on this is Ed Ford. Did you order his book: Freedom From Stress?

>I wonder how this fits into your presentation?

Poorly. Welcome to PCT--a NEW explanation for how thought gets turned into action and feelings.

-----  
>Rick (940303.0800) alias Dr. "let them fend for themselves,  
don't teach them anything" Spock

>>When you have accepted that we are all living control systems, you  
>>realize that actions depend on wants as they relate to present  
>>perception.

>

>I don't like this sentence at all. Though it is basically correct, it  
>seems misleading. Action does depend on want -- but when the want is  
>fixed, action depends mainly on circumstance (disturbance).

Thank you for pointing out a possible misinterpretation to me.

How about:

When you have accepted that we are all living control systems, you realize that actions depend on wants as they relate to present perception--influenced by the environment, disturbances and your own actions--in the circular chain of control.

>When you find yourself in a situation where you want to teach a person  
>(a male this time) to think more effectively because you are concerned  
>about how he acts, your knowledge of HPCT suggests that he must work  
>"inside his head" because only he can change his wants and perceptions,  
>which determine his actions.

>

>This sounds pretty Orwellian to me. If "you are concerned about how he  
>acts" don't you think PCT might suggest that YOU are the one who should  
>learn to "think more effectively" -- rather than getting the other  
>person to "think more effectively". Sounds like "Godfather" PCT to me.

Dr Spock, as one who manages nobody, and cares not one whit what your kids do that will get them expelled from school, in jail, sick, or whatever because of their foolish ways, you should know. "Let the little darlings operate with infinite degrees of freedom" is utopia, not the real world. I doubt very much that you walk like your foolish talk. I cannot wait for your constructive suggestions, based on your real world experiences rather than black and white theorizing (dreaming).

Thanks anyhow, ">about how he acts, ..." above, might better be:  
...about the results of his actions, ...

>He will know that you support him from how you teach him to manage his  
>life better in relation to others (#2).

>

>Thanks you, Don Corlione. I feel so much better now that you have taught  
>me how much more effective it is for me to think of myself as a "team

>player" in your organization.

Ben would be better off losing his team position, than listening to a caring manager, teammate or friend?

Rick, as sometimes happens, you are very wrong. We do NOT live in a world of infinite degrees of freedom, where control systems with infinite amplification can control whatever they like in any way they like. You don't either, but you hate to admit it. You have to be to work on time and consider other degree-of-freedom constraining things and agreements you have made.

In the real world, not all control systems manage to satisfy themselves. Some of them have not grown up to be as capable as you are. Some of them have not grown up yet. Ed Ford shows how to help them become satisfied in a real world of agreements, work, rules, family groups, teams, and whatever where there is a need to get along with others.

Why don't you put your money where your loud mouth is and write your own book on practical applications of PCT. As you write, you just might find that you have a lot to learn still.

Don't forget to post it on this net as you go.

>Bill's version of the letter is great, of course.

Thanks, again. What else dare you say?  
-----

Does anyone else have strong, constructive views on these matters?

Applications of PCT are where the value of PCT lies, I think. Why such a silence on CSGnet on this? Surely, more CSGnetters have experiences using their PCT insights raising their kids, loving their spouses, dealing with friends, bosses and subordinates, vendors and customers. Or is it all just a logical exercise that has no relevance?

Best, Dag

Date: Thu Mar 03, 1994 7:43 pm PST  
Subject: Re: Feelings, Teaching Effective Thinking

<[Bill Leach 940303.21:12 EST(EDT)] >[Dag Forssell (940303 1630)]

>Did you read....

No not yet but at least when things slow down here a bit, I do have most of what you mentioned (with BCP on the top of the pile).

I will admit that PCT is only just beginning to change the way that I think about behaviour. I recognize that ultimately the change will be dramatic, but for now it is hardly operative.

-bill



Date: Thu Mar 03, 1994 7:52 pm PST  
Subject: Re: Why I'm such an asshole

<[Bill Leach 940303.21:01 EST(EDT)] >[Dan Miller (940303.1000)]

>Incidentally, do objects dropped from great heights (the stratosphere)  
>continue to accelerate at the same rate as they approach Earth?

Actually the accelerating force increases as they approach the earth. However, the acceleration (first derivative of velocity) do increase at a slower rate which depends upon frictional losses in air.

A maximum speed is normally reached. However, Newton's laws are not considered conditional for non-relativistic velocities. I don't recall Newton specifically mentioning "air resistance" but he did definitely consider the effects of additional forces.

-bill

Date: Thu Mar 03, 1994 11:03 pm PST  
Subject: PCT and Management

[From Rick Marken (940303.2300)] Dag Forssell (940303 1630)

>How about:

>When you have accepted that we are all living control systems, you  
>realize that actions depend on wants as they relate to present  
>perception--influenced by the environment, disturbances and your own  
>actions--in the circular chain of control.

I still don't like it. How about:

When you have accepted that we are all living control systems, you realize that people act only to produce intended perceptions; how people act in order to do this is determined mainly by circumstance. People control the results of their actions (their perceptions) not the means used to produce those results (their observable actions).

Me:

> If "you are concerned about how he acts" don't you think PCT might  
>suggest that YOU are the one who should learn to "think more effectively" --  
> rather than getting the other person to "think more effectively".

Dag:

>Dr Spock, as one who manages nobody, and cares not one whit what your  
>kids do that will get them expelled from school, in jail, sick, or  
>whatever because of their foolish ways, you should know. "Let the little  
>darlings operate with infinite degrees of freedom" is utopia, not the  
>real world. I doubt very much that you walk like your foolish talk. I  
>cannot wait for your constructive suggestions, based on your real world  
>experiences rather than black and white theorizing (dreaming).

I see. The "real world", that place where "the rubber meets the road", is a place where we must control people or be considered "foolish". You described a situation where you were "concerned about how [someone] acts". Your concern indicates an error -- there is a discrepancy between an image of how the person should act and how they are acting. You want the person to learn to "think more effectively" so that you will no longer be concerned about his behavior (you will no longer have an error signal). You are framing this in terms of helping the other person by getting them to "think effectively" -- but would you really stop being concerned if the person's way of "thinking effectively" involved taking a fake spill down the stairway, suing for workman's comp and then staying at home with the in-laws. Probably not, because you have references for what you consider a reasonable solution to the other's person's "problem"; your concern is REALLY about what YOU perceive because YOU are a controller -- we are all controllers. That's what PCT is about.

What your little "role play" described was one possible way to get a person to behave in a way that would stop causing you concern; it was a demonstration of control; and one that worked unrealistically well.

There is nothing wrong with control; the point of PCT is that EVERYONE controls-- we can't help it. We can no more stop controlling than we can stop breathing. That's why the manager in your "role play" is "concerned". It's because the manager is a control system -- only control systems can be "concerned". And one of the things that human control systems are almost always concerned about is the behavior of other people. So people try to control the behavior of other people; you do it, I do it, we ALL do it because we are all controllers. This IS the real world. If you think I am advocating that people stop trying to control then I can see why you think I am being unrealistic. That's not only unrealistic -- it's IMPOSSIBLE according to PCT. People "know" the way things "should" be because they are a mass of reference signals -- and nothing can stop them from trying to make their perceptions match those reference signals. And many of those reference signals specify the "right" level of perceptions of other people's behavior: the right amount of time to do homework, the right amount of drinking and smoking, the right this and that.

The problem is that PCT shows that we CAN'T really control the behavior of another human controller. We might luck out (like your manager) and have people act as we want --but PCT shows that it's just that -- LUCK. The more common result of controlling other people is CONFLICT -- because the controllee is always controlling the controller right BACK.

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Side note: I think those role plays are lousy ways of illustrating human interaction. They are completely ridiculous from a PCT perspective -- because the actions in the role play (unlike those in the REAL WORLD) always produce the expected result (compliance from the other person, for example). I can demonstrate the virtue of any management strategy using this technique. For example, here is an S-R management technique that I just "role-played" with my daughter:

Me: Lise, please clean up your room right now.

Lise: But Daddy, I have homework to do.

Me: I said clean up your room right NOW.

Lise: OK Daddy. I'll do it right now. My homework can wait.

Me: Thank you Lise

Lise: You're welcome, Daddy.

Pretty good, eh. And I used a real situation with a REAL teenager. We may not be talking about where the rubber meets the road but we're sure right there where the lipstick meets the lips.

Leave "role plays" to the third graders, Dag.

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Dag continues:

>In the real world, not all control systems manage to satisfy themselves.  
>Some of them have not grown up to be as capable as you are. Some of them  
>have not grown up yet. Ed Ford shows how to help them become satisfied  
>in a real world of agreements, work, rules, family groups, teams, and  
>whatever where there is a need to get along with others.

The kind of "helping" I saw described in your post looked a lot like the kind of helping we saw described by Tom's student at the last CSG meeting. The kind of "helping" you describe comes very close to being controlling. It is very hard to help another person without interfering with that other person's efforts to control; but a good example of the way NOT to help a person was shown in the "role play" you described. I don't have any great ideas about how you can help another person -- but I would imagine that it starts with the "helpee" expressing a need for help; the person in your role play did not obviously want help -- at least you didn't say that he did. The person who had the problem in the role play was the manager -- the person who was "concerned" about the worker. That's why I said that the manager should seek help; and it's why I called it "godfather" PCT. You obviously think of it as a situation where you are informing a person of a possibly undesirable consequence of their action -- being chronically late leads to demotion or firing. But if that's all it is, then just give the information and let the person deal with it as they wish. The way you presented it, it was more like an ultimatum: find a way to work things out at home or get demoted.

I think that people who want to be helped can be helped -- using techniques based on PCT. But I don't think that managers need to learn to help people in this way. I think the problems of managers usually have to do with coordinating the efforts of many WILLING workers; managers who spend a great deal of time "helping" problem workers are probably rare. So the application of PCT to management should deal mainly with what PCT says about how to successfully coordinate the efforts of many people in order to achieve common goals. Here are some ways that I think PCT can be applied to management:

1. Managers must recognize that people control what they perceive. Thus, an important task of management is to try to insure that everyone is able to perceive the common goal in terms of all the perceptual variables that make it up. Description of the dimensions of the the common goal is important; what are the variables you want the workers to control? These variable can be given names -- but education is needed to insure common perception: what is "quality", "efficiency", etc.

2. Managers must try to establish common reference levels across workers for the intended states of the controllerd perceptions. Once workers know what perception(s) to control they must know the appropriate levels at which to control them. Once we know that we are to control the "closeness of joins", we have to know just how close we want them.

These first two points are about the importance of "training" to achieve cooperation; an important component of this training is the education of perception; teaching worker's what variables to perceive.

3. Manager's must be aware of the fact that all workers are controlling a whole constellation of perceptions; the perceptions to be controlled at work are just a subset of the perceptions people control. This means that you can't expect to be able to arbitrarily tell a person to "do this" (meaning "control this variable at this reference level"). Manager's must be sensitive to the fact that control of certain variables will conflict with control of other variables. A manager must always be willing to be flexible about who does what, when and with whom. The manager in your story might have been a bit more flexible about when the worker shows up -- especially if the worker is otherwise getting things done -- unless the main goal of the company is to produce people who "show up" at a particular time.

4. Manager's must understand that worker's control perceptions, NOT actions. Manager's are there to help people understand what results are to be produced -- NOT HOW they are to be produced. PCT shows why "micro- management" is so manifestly unsuccessful.

These second two points are about the importance of "flexability" in management. A manager who is committed to "doing it the GM way" or "doing it the Japanese way" or even "doing it the Deming way" (if the later is conceived of as a particular set of activities that must be carried out in order to have a successful company) is committed to eventual failure. What works now will not necessarily work in 5 years. We might not always know WHY something no longer works -- but we can tell THAT it is no longer working when we are no longer getting the results we want. PCT suggests that managers (leaders) must always be willing to try something new when things are no longer working; leaders committed to doing it "the right way" will eventually be "history".

So training and flexibility are two important practical implications of PCT. Managers who provide workers with training and flexibility will succeed; those who don't, won't (in the long run).

One last implication of PCT is that you DON'T need to provide managers or workers with control -- they already have it. If a manager wants a worker to show up at a particular time, he can be counted on to try what he can to get it to happen (like the manager in your role play). Manager's don't have to learn how to control workers -- nor do they have to learn to help workers control (they will just be interfering). What managers have learn is to be aware of their OWN controlling; they must be aware of the fact that they are controllers. Such an awarness would have helped the manager in your role play understand his own controlling -- and might have kept him from acting like such a jerk. He would have known that his "concern" comes out of his own nature as a controller and his goals about what the worker "should do" might not help the worker at all. A manager aware of him/herself as a controller can spend more time worrying about coordinating efforts for the common good and less time trying to "help" (euphemism for control) people for their own "good"

Best Rick

PS. Lise did NOT clean her room; but she did do her homework (as usual) and gave me a big kiss for being such a silly daddy for doing the stupid role play.

Date: Fri Mar 04, 1994 9:32 am PST

Subject: Invented facts; Newtons's laws; management philosophy

[From Bill Powers (940304.0800 MST)] Dan Miller (940303.1000)

I think you misunderstood Rick, Dan. The "invented fact" mentioned was data assumed (imagined) by Martin in illustrating how to do a calculation. The actual data found in a real experiment look considerably different.

This is normal. We invent data when we make predictions from theories: we say "here is what is going to happen," and we describe the expected observations. That description is invented data. Then we actually do the experiment, and see what DOES happen. If the invented data are close to the actual data, the theory is supported. If the difference is too great -- back to the meditation chamber.

An interesting example, more akin to sociology: Allan Greenspan, who heads the Federal Reserve Board, has a theory that raising interest rates will slow inflation. This is an imagined effect, stemming from the theory of economics in which he believes. He believes in this imaginary fact so strongly that he is talking about raising interest rates even without any signs of inflation, to prevent future inflation which he imagines to be caused by too high a growth rate.

If we look at the historical record, the actual observation is that raising interest rates has tended to (1) slow economic growth, (2) put people out of work, and (3) leave the inflation rate unaffected or increase it. So, we can be confident that when Greenspan examines the historical record, he will find that the actual data differ from his invented data, and will change his theory. Won't he?

>Incidentally, do objects dropped from great heights (the  
>stratosphere) continue to accelerate at the same rate as they  
>approach Earth? Is a maximum speed reached, one based on mass  
>and atmospheric resistance? I'm not certain about this, but if  
>so, then Newton's Laws are very conditional.

To expand on Bill Leach's correct but slightly misleading answers:

The acceleration of an object due to gravity rises as an inverse function of the distance to the center of the earth (not the surface). An object 3 miles in the air experiences only  $(4000/4003)^2$  as much acceleration due to gravity -- 0.9985 -- as an object just above the surface. The radius of the earth is roughly 4000 miles.

The object reaches terminal velocity when it is falling just fast enough so that friction with the air creates a braking force equal to the force of gravity on the object. At that speed, velocity is constant and acceleration is zero.

But Newton's laws still apply as precisely as we can measure. Newton said that an object will continue in its state of motion in the absence of forces, and that its acceleration will be the sum of all applied forces divided by its mass. The falling object is subject to two forces, one from gravity and one from air friction. As long as the force from air friction is less than the force from gravity, the object accelerates (goes faster and faster). As the difference in

forces decreases (as the rising velocity creates rising air resistance), the acceleration becomes smaller, and eventually becomes zero when air resistance equals the force of gravity. Then the object is falling at a constant speed. At all times it remains true to the sixth decimal place that the acceleration of the object is the sum of all forces acting on it divided by the mass.

Newton, in illustrating his laws, showed that a projectile fired in a vacuum would rise and fall in a parabolic path (actually an ellipse, but he was simplifying). This is what is taught in highschool physics. However, the fact that projectiles fired in an atmosphere follow a different path does not "disprove Newton's laws" as I have heard some people claim. It simply says that in the real case there are forces beside gravity acting that must be taken into account to apply Newton's laws correctly. The retarding force due to air friction is a complex function of a projectile's velocity and shape, but it can be computed fairly well, and measured even more closely. When that is known, Newton's laws can be used without change to predict the non- parabolic path of the projectile.

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Rick Marken (940303.2300) --

I liked your restatements of what managers must learn. Control is a fact and we all do it. Managers are controlling for getting the people who work for them to behave as the manager wants them to behave. The basic fact is that if they don't comply, the manager will find someone who will comply. A worker who fails to satisfy the manager doesn't have the option of staying on the job.

As you say, this is the basic framework within which all interactions between managers and workers take place. Dag acknowledges this.

So what can "management philosophy" do to smooth the path of progress? The first step has to be taken when a worker is hired. The manager has to explain that his/her role in the company is to see that certain things are achieved, by assigning objectives to those who contribute to the achievement. The worker being hired has to agree to satisfy the goals set by the manager, to meet the standards set by the manager and by the time specified by the manager. The manager might indicate an openness to negotiate details and consider suggestions, but basically the responsibility for achieving the manager's goals is the manager's, and the worker is hired only to achieve those goals. It is up to the worker whether taking the job under those conditions is agreeable. If this agreement is explicitly reached, it forms the basis for all future interactions.

Unfortunately, this simple and rational approach runs into all sorts of snags. Basically, many workers would like to get paid without doing any work, and many managers would like to get the services of workers without paying any wages. The goal of maximizing profit means minimizing costs; the goal of living the good life means obtaining the goodies without onerous labor. Under normal conditions, there is a built-in labor-management conflict. This conflict is made worse by pressure on the manager from above, and pressure on the worker from the needs of daily living.

There is no formula that can resolve all the conflicts inherent in the labor-management structure. What PCT can do is to teach people about human nature, so they will recognize conflict when it happens and not be surprised or outraged. The only ultimate solution is for all the people in a company, from CEO to sidewalk-sweeper, to recognize that they're all in the enterprise together and depend equally on its success (this also has to be TRUE). They all have to

understand the structure of the company and what their role is in keeping it viable. This means understanding how people work, and especially how they don't work -- they don't respond well to forcing. It means understanding that each person in a company has needs and interests other than the job, and that to enable either workers or managers to function well, all these needs have to be taken into account.

All of that's easy to say. What's hard is to generate the good will that's required to make any of this understanding useful. My own feeling is that simple understanding of PCT is about the best help we can offer; it's better than the superstitions, prejudices, habits, and falsehoods under which most business and other human affairs are conducted today. I go on the principle that it's better to understand what's going on than not to understand it or to understand it incorrectly. What you do differently with that understanding is pretty much up to each person to work out.

I don't really much care how people like Dag Forssell and Ed Ford go about teaching PCT, as long as they're teaching it. Those who pay attention seem to get a lot out of it, even without being told what they should get out of it. In contrast to what exists now, that becomes pretty obvious to people. People who don't pay attention won't get it, of course, but I think we have to focus on a quality few rather than trying to mass-produce PCTers. If each one teaches two, PCT will spread all by itself. All we have to do is keep the message from degrading.

Best to all, Bill P.

Date: Fri Mar 04, 1994 10:20 am PST  
Subject: Got all messages from TX; Wonderful Rick

FROM CHUCK TUCKER 940304.12:04

Yes, I got the messages and can't wait until Tom is directly on the net so we can talk about the Lazare research as well as the research on "helping" by (I'm sorry but I can't recall her name); that along with Tom's own research is worth discussing on the net.

Don't believe a word that Rick says about himself; he is a very poor judge of character especially his own. I am the expert on Rick not only as a character but his character; he is a wonderful, sensitive, caring, intelligent, compassionate and dedicated human being (I just have not figured out how he can write so much on the net and still have a job!?!). He is strongly committed to every aspect of PCT - he seems to live and breathe it - he is a TRUE BELIEVER with one minor flaw - he will listen to what he considers to be (constructed not invented) FACTS. So, I would suggest that those of you who have some problems with PCT rather than arguing with Rick on the net just (I am in the process of doing this so I'm not suggesting some- that I won't do) start replicating some of his research. He will gladly supply you with the references and will discuss any of the research with you. By doing the research you can discover PCT and supply some more data for (or against) the model.

Here is another suggestion. In Bill's Demo1 there is a section where you can "control for" 5 different types of RS. I don't know of any research done with these different RSs. I know that I have never gotten higher than .79 on the

"sound" yet I can get mostly .95+ with the "crossed line." I wonder what one would find if you did it 50 times? I wonder what you would find if 25 people did each one 50 times? I wonder if everyone or ever one person would keep the same "gaze point" throughout each "run?" I think one could learn much be doing such research.

Sorry for the preachy tone of this post.

Regards, Chuck

Date: Fri Mar 04, 1994 2:20 pm PST  
Subject: Feelings, Management

[From Dag Forssell (940304 1030)]

Bill L. 940303.08:55) Rick (940303.2300) Bill P. (940304.0800)

Just a quick acknowledgement with thanks for the interest and support you have provided me.

Bill L., your comments are very useful to me in that they remind me of how a person who has not yet understood PCT can interpret the article. I will work in some additional clarification.

Rick, thanks for putting your creative juices to work in response to my challenge. I can see you stayed up late. Very useful gray-scale considerations came out of it, which I shall shamelessly consider and incorporate.

Bill P., thanks for your measured perspective, as always.

I agree that:

>Those who pay attention seem to get a lot out of it, even without being  
>told what they should get out of it.... All we have to do is keep the  
>message from degrading.

The efforts I am posting are about getting people to pay attention. I find that it is harder to write an introductory article than to teach PCT once you have the attention. The article must live up to so many requirements: Attracting attention, convey the usefulness, be accurate, be short, be understandable without any understanding of PCT... a nearly impossible set of conflicting requirements.

I appreciate getting substantive suggestions. I shall as always consider, reorganize and rewrite, doing my best to put things in a proper perspective and then re-post. I count on you all to react again.

Best to all, Dag

Date: Fri Mar 04, 1994 2:29 pm PST  
Subject: Re: PCT and Management

<[Bill Leach 940304.08:01 EST(EDT)] >[Rick Marken (940303.2300)]



Rick, if my attempts at a contribution are only "noise", just say so. I fully recognize that I am too new to all of this to appreciate much of what is said and is going on.

I was quite uncomfortable with Dag's "Role Playing" example and with some of the discussion. I don't feel qualified to comment much about the discussion but the "Role Playing" "being a 'real life' sort of example" is something that I can identify with. I think that a "role" example could be used but it would need to be much more involved than the one given.

I am well aware that my own perceptions concerning human behaviour can sometimes be faulty or be correct but based on incorrect premises. Through experience, I know for example, that the Meyers/Briggs "Type Classifications" definitely have some validity.

I personally do not see any conflict between such "personality typing" and PCT and even suspect that maybe at some time in the future PCT will provide the rational basis for such things. I even suspect that any "PCTers" that have already given thought to "personality types" may already have some plausible and rational explanations.

The fact that we all ARE control systems does not change the fact that some control systems would refuse to acknowledge such in the face of any amount of facts and proof.

Dag's posting appears as though it is a gross over-simplification of human interaction (and I don't mean to imply that he either intends such a presentation nor that he discounts the difficulties that can be associated with human behavioural interaction).

As I think about "Type Talk", I realize that knowingly or otherwise, the authors recognized some important PCT concepts but they also recognized some concepts that seem to be "missing" from Dag's presentation. They seem to recognize that while you often can force or coerce a person into doing what you want that such conduct creates conflict within the person so being manipulated and in the long run is counterproductive.

They also, OTOH recognize that individuals have different "goals" or "desires" (perceptions, yes?) and that trying to understand, recognize and relate these individual desires to the organizational goals is the most successful technique for management.

It seems to me that PCT does recognize that in human interaction there are many perceptions and references involved. If anything, PCT asserts that if you don't relate effectively to the other person's references the interactions are doomed to failure. Since obviously you can't "see" the other person's references, you then have to undertake a careful "probing" to try to discern just what perceptions this other person IS trying control.

While I think that the idea is straightforward, I think the process is anything but... Among the many problems are the idea that the subject is not only controlling perceptions but is likely trying to provide perceptual variable to you that the subject thinks will control for some desired outcome. If the subject thinks that their own desires are inconsistent with yours or that maybe you will

"think poorly" of them if you knew their real wants, then they may go to amazing lengths to try to keep you from determining what they actually want.

Again, I am perfectly happy to conclude that a PCT approach is more likely to succeed in such situation than any other approach but assuming that a logical presentation of what is desired or needed will work as a universal method with all people is a serious mistake.

An effective manager MUST actually be concerned about the employees. In some cases, perceptions by some of the employees of his concern is in itself sufficient for cooperation. BUT even where that is not the case, a manager that is unwilling to try to understand the needs, wants and concerns of the employees will be unable (except by luck as already mentioned) to manage. If I read this correctly from a PCT standpoint then the manager that does understand PCT AND the individual employees will try to provide the environment necessary for each of the employees to control their own perceptions without excessive conflict and still be working toward the goals of the organization.

Understanding PCT then would mean that such a manager would have the tools to both create and test such environmental changes. This manager would be able to adapt the environments to correct for errors in his judgment and the inevitable changes in the employees themselves.

-bill

Date: Fri Mar 04, 1994 6:57 pm PST  
Subject: Re: Feelings, Management

<[Bill Leach 940304.16:34 EST(EDT)] >[Dag Forssell (940304 1030)]

You are welcome of course and I will look forward to your next post. I don't think that you are right in your perception that I don't understand PCT. I do think that you are right in that I don't begin to appreciate the implications and significance of PCT.

I have always felt that "compulsion" was fundamentally bad. Often necessary for various reasons but never as satisfactory as "win-win", willing cooperation, "shared goals" or whatever you want to call the situation that exists when another person does something that you want done because they want to do it.

As far as I am concerned, PCT explains WHY this 'HAS' to be true. It does not "discount" the complexity of human interaction but appears to me to have the potential to remove a great deal of the mystery.

As PCT "makes its way" into the world, the term "Abnormal Psychology" may disappear. We will still have "abhorant" behaviour and will still have problems but just maybe we will have an effective and "humane" means of dealing with problems.

-bill

Date: Fri Mar 04, 1994 8:26 pm PST  
Subject: Scientific Facts

[From Rick Marken (940304.0930)] Dan Miller (940303.1000)

>Certainly, facts are perceptions. I would argue that they are  
>perceptions that we generate through our purposive actions. Facts  
>are the perceptual consequences of purposive action. That is, we  
>intend to perceive facts in those specified conditions. We specify  
>the conditions, and the facts are perceived (controlled perception).

Scientific facts are NOT controlled perceptions; the swirl of points that Bill Powers posted was not brought to that state by Bill's actions. After setting up the proper conditions and doing the proper computations, the swirl of points you saw is what resulted; a different shape of points COULD HAVE resulted; the shape could have been the one predicted by Martin -- but it was not.

When a scientific fact is found to be a controlled perception (of the scientist) then that fact is quite rightly deemed to be a non-fact and the scientist is exposed as a fraud. Cyril Burt treated his correlations as controlled perceptions --resulting in his being rightly subjected to posthumus contumely.

>Sociologists often are "accused" of making-up facts (or data).  
>My answer to this accusation is, "of course we do."

Then sociology is a fraud (I would have said only that it is built on the wrong foundations -- "fraud" seems a bit strong, even for me).

>Incidentally, do objects dropped from great heights (the stratosphere)  
>continue to accelerate at the same rate as they approach Earth?

Yes. They are then called "satellites". The moon is an example of an object that maintains constant acceleration as it falls toward the earth.

>Is a maximum speed reached, one based on mass and atmospheric  
>resistance? I'm not certain about this, but if so, then Newton's  
>Laws are very conditional.

The laws are always the same. You have to make sure that you take all of the variables into account properly when you apply the laws; the conditionality of Newton's laws are the conditions to which they are applied. For example, constant acceleration only occurs in a vacuum; in a medium you have to include the resistive force of the medium (that force being one of Newton's laws) in the calculations.

Best Rick

Date: Fri Mar 04, 1994 8:27 pm PST  
Subject: Non-biodegradable PCT

[From Rick Marken (940304.1000)] Bill Powers (940304.0800 MST)

> All we have to do is keep the message [of PCT] from degrading.

That is my highest order goal (it's obviously not to win friends and influence people).

Love ya Rick

Date: Sat Mar 05, 1994 10:20 am PST

Subject: Re: Scientific Facts

[Dan Miller (940305.1200)] Rick Marken and Bill Powers:

Thanks for correcting my physics. I simply wanted to make it clear that the conditions specified by Newton (i.e., the vacuum) did not exist for him. His facts were imagined (constructed, extrapolated). His laws work well. Very bright guy.

Regarding scientific facts (data), I would make distinctions between constructed, invented, and imagined objects. We agree that scientific facts (data) are objects, right? They are made by scientists for a purpose. Simply put, I am saying that facts are scientific constructions. They are social objects constructed and used purposively. Knowing how they are constructed seems to me to be as important as the facts themselves. Indeed, I have great difficulty separating the consequents from the act.

Bill, your example of Alan Greenspan is informative. Mr. Greenspan is a true believer in his theory and its contribution to the health of the U.S. economy. He believes more in his imagined facts than in other economists' constructed facts. Still, I would want to know how the economists (all of them) construct their facts and for what purposes.

It is not likely that Mr. Greenspan will modify (or drop) his theory of interest rates and economic "health." Even as a true believer who pays little attention to other facts, his facts have worked for him for quite some time. Arguably his laws do not work well. He's no Ike Newton.

Rick says:

<Scientific facts are NOT controlled perceptions; the swirl of points  
<that Bill Powers posted were not brought to that state by Bill's actions.

Then how did they get there? Did they just appear from thin air? Or were they a consequence of his purposive actions? He set up conditions, plugged in his model, typed a sequence of instructions, and scientific facts appeared that conformed to his predictions. But these facts do not owe their existence independent of his purposive actions. His theory works. Just where is there no control?

Cyril Burt was a fraud. His facts (data) were imagined, and he LIED to us about he constructed them. He wrote that he used a certain set of procedures on a specific sample of twins, but he did not do this. From his point of view he did not have to because he already knew the truth. He was a true believer. He wanted to make sure that everybody knew and agreed that intelligence was largely inherited.

If I am not mistaken Gregor Mendel imagined some of his facts. However, he established and used a specific set of procedurers that could be replicated. He was correct, of course. His theory worked.

Some sociologists are fraudulent (as in other disciplines). I suspect that lots of facts and data are imagined. All of it is a construction.

Rick, it is interesting that you think that sociologists have the wrong focus (unit of analysis?). You see a world of independent individuals engaged in controlling perceptions. I see a world of people in social relationships, groups, organizations, interacting with one another. These relationships, groups, and organizations are composed of individual organisms who control perceptions, but I suspect that we are going to need more than this to adequately understand these phenomena. If the upper levels of the hierarchy are socially emergent and developed in social interaction (as I think they are), then at some stage in the hierarchy (categories?) the most basic unit of analysis is going to involve (at least) the dyad.

An imaginary experiment (NSF will not fund this): Let's take an infant at birth, keep it alive in social isolation (tubes, wires,...) for twenty years. No human contact for twenty years. Will this organism control perceptions? Will it act purposively? At what level?

I am getting weary, and there is a perfectly beautiful day outside.

Later, Dan Miller MILLERD@UDAVXB.OCA.UDAYTON.EDU

Date: Sat Mar 05, 1994 10:39 am PST

Subject: Information--one more try

[From Kent McClelland (940305)] Rick, Martin, Bill P., Tom

I had hoped in making some remarks about information earlier this week (940228) not to provoke people to go back over old ground but to encourage them to take a fresh look at the subject. However, it seemed to me that most of the comments on my post had little to do with the substance of my suggestion that the colloquial understanding of information can be given a meaningful definition within the PCT worldview. I think that Rick, Bill, and Tom are probably right in their off-repeated argument that a control system does not get any information about the disturbance when it operates in what Bill has called a "quasi-static" mode, that is, using a constant reference signal. But I doubt that they really mean to imply that living control systems are hermetically sealed off from getting any information (in the colloquial sense) from their environments, which is one way you could take that argument.

I've been trying this week to figure out a way to put my ideas on the subject a little more cogently. Here's another try:

The experience of "getting information" happens when a dynamically operating control system changes its reference signal (on some hierarchical level) to achieve better control of its perceptions (reduce error) and in that way to resist disturbances (at a higher level) more effectively. Information is in effect continually "constructed" by a hierarchical control system in the process of its dynamic operation, and it constructs this information (a change in reference levels) out of the raw materials of environmental changes in disturbances as "interpreted" by the already-learned structure of the control system, that is, by its perceptual input and output functions and the "repertoire" of reference-value settings available in memory. To put it more simply, a change in the pattern of disturbances comes along and the hierarchical system must change its lower-level reference signals to keep some higher-level perception in control. By this process, the control system acquires "Information" about the environment, and the

"in-forming" specifically consists of the new pattern of lower-level reference signals within the organism.

When a changes in lower-level reference signals take place while the higher- level system is working in imagination mode, I would say that information has not been acquired, because the change in perception has nothing directly to do with any disturbances in the environment. I'd venture to say, moreover, that one-level control systems (like e. coli?) never discover any "information" at all (in my sense) about their environment. They just control by means of random changes in their reference signals.

In general, I have the impression that PCT spokesmen have been giving a little too much emphasis to the "quasi-static" operation of control systems while neglecting to discuss system dynamics in depth. One reason may be to avoid any S-R implication that the environment "causes" changes in the system, and another reason may be that in order to focus on dynamics we have to consider a multi-level hierarchy, which makes the model more complex. I guess I'd like to see more discussion on the net of when control systems change their reference values and why.

Best to all Kent

Date: Sat Mar 05, 1994 12:09 pm PST  
Subject: Re: WTP & DME-RKC

<Bob Clark (940305.1330 EST)> Bill Powers (940221.0930 MST)

Apparently you find my attempts to "unpack" inadequate. I am taking your remarks as indicating what you are looking for. 16 items can be distinguished. These items are used as a guide for describing the way the DME fits with PCT.

1. how to classify "events I perceive within myself"

In an earlier post, Bill Powers (940131.1115 MST), the "principles of awareness" are summarized:

>based on empirical but subjective observations.

Your discussion in that post is in terms of the first person singular. In many ways, as I have pointed out in "PRONOUNS - RKC," [Bob Clark (9401202.1540)] these pronouns are very nearly synonymous with the DME.

What I have done is propose the name, "Decision Making Entity," as a label for a concept "based on empirical but subjective observations."

More important: Who or what is "aware?" I call it the DME. Who or what controls awareness? Again, the DME. Make that the first person singular, if you like. I think there are advantages to the more abstract term for formal discussion.

In the following, page numbers refer to B:CP.

2.

>If the DME can "select" reference signals from memory, this implies  
>that it must be able to perceive them and do something (which needs

>to be specified) that is called "selection."

Perception from memory is covered in "Imagination" pp 222 - 224, and p 284. Nowhere is it stated who or what perceives the "imagined" signals. For convenience, I call it the DME.

"selection" requires a "comparator." This is defined on p 26 and in the Glossary, p 283. As defined in B:CP, the comparator is restricted to comparing perceptual signals with reference signals.

However, the concept of comparator can just as well be extended (slightly) to include comparison between two sets of perceptual signals. There are four possible cases: 1) comparison between present-time perceptions and present-time reference signals -- the original definition; 2) comparison between present-time perceptions and imagined past-time perceptions; 3) comparison between two sets of imagined past-time perceptions; 4) comparison between an imagined perception and a reference perception.

The first case is the original definition.

The second case is when one is "reminded" of some past event. Some portion of the set of present-time perceptions may be identical to some portion of a stored past event. The similarity results in awareness shifting to the imagined past event and making more complete comparisons. Alternative imagined events can also be compared with the present-time situation. The DME is aware of each of these sets of perceptions, as well as the corresponding degree of match.

"Anticipation" is included in this case. If an imagined event includes changes over a period of time, it can be regarded as offering a form of prediction, or "anticipation." The "imagined event" may be very complex and sophisticated, or as simple as "that's the way it happened before."

The third case is when the DME perceives similarities between two, or more, sets of imagined perceptions. The DME can use the differences for further search.

The fourth case is when the DME compares imagined sets with other imagined sets that are being used for anticipated objectives.

"Planning" is the assembly of a series of imagined situations/events so as to achieve (imaginary) anticipated objectives.

3.

>If it is to "apply" them, it must have some means for directing them  
>to particular subsystems,

When the imagined signals have been selected, the DME "applies" them by changing awareness from "imagining," p 284 to "remembering" p 287. The DME controls awareness.

4.

>which implies in turn that it must know where the reference inputs  
>to those subsystems are,

These inputs are included in the memory, and activated by "remembering" p 222. The "reference inputs" are included in the memory (remembering p 287).

5.

>as well as what those subsystems accomplish.

Through imagining, the DME can become aware of any recording at any level. Thus the structure and actions of the subsystems can be examined. In this sense, the DME "knows" what those subsystems accomplish. When changing to remembering, the subsystems are automatically included and activated accordingly.

6.

>And it must be able to operate routing switches.

See 3, 4, and 5.

7.

>The recognition capabilities bother me the most, because they seem  
>to duplicate what is already in the hierarchy. How could a DME know  
>what the content of a memory location means? It would need a  
>recognition system just like the one that normally (automatically)  
>handles that kind of perception.

The DME does not "duplicate" the hierarchy. The major difference is that the B:CP hierarchy is "automatic." The DME can perceive any set of perceptions existing within the hierarchy. As to "meaning," the DME has access to whatever set of perceptions are involved in "meaning." The existing hierarchy is not duplicated. The DME perceives the hierarchy through imagining existing recordings. The DME uses its access to imagined past-time events as a means for anticipating and planning for future-time objectives.

8.

>It must be able to perceive memories as they exist in storage,  
>either without activating them or by addressing and activating them  
>while simultaneously preventing the resulting signals from becoming  
>reference signals for lower systems.

Isn't this synonymous with "imagining" (p 284) them, as contrasted with "remembering" (p 287) them? Simultaneous imagining and remembering the same recording seems to directly contradict the definitions.

9.

>It must be able to judge the relevance of each examined memory to  
>accomplishment of a specific goal at a higher level.

The "higher level goal" is an "anticipated" set of future-time perceptions. This goal has been selected on the basis of feasibility -- the goal must be related to the present-time situation and consistent with other long-term high level goals. The relevance is determined by the selection process.

See 2.

10.

>It must be able to weigh possible outcomes and judge them in terms  
>of multiple higher-level goals.

"judge" and "weigh" are synonymous. See 9.



11.

>And then it must be able to connect one of the set of possible  
>reference signals to an appropriate lower-level comparator or set of  
>comparators."

See 3 and 4.

12.

>In my system, memories are examined by the next level up, not a  
>separate entity."

Does "examined" include "awareness?"

On Page 200 I find:

"It seems that the behavioral hierarchy can proceed quite automatically, controlling its own perceptual signals at many orders, while awareness moves here and there inspecting the machinery but making no comments of its own. It merely experiences in a mute and contentless way, judging everything with respect to intrinsic reference levels, not learned goals."

As discussed in number 1 above, your "awareness" closely resembles my DME. The main difference is that your awareness is restricted to intrinsic reference levels, while I extend the concept to include means for establishing and changing learned goals.

The only means in your hierarchy for establishing new goals and/or changing old ones is through operation of the reorganizing system. Yet such additions and modifications of learned goals seems to be a very common experience -- subjective, perhaps, but you seem to find "empirical but subjective observations" a useful basis for decision. How does your automatic hierarchy account for your own decision making?

13.

>in my model of this process, perceived memories can be composed of  
>memory signals from many lower-level systems, the same ones that  
>normally supply multiple inputs to the same perceptual function  
>during real-time control.

The DME concept does not imply any change in this relationship. The DME is concerned with anticipation of future situations/events at some level as the basis for operation of the next lower level. That level, in turn, uses the lower level structure in exactly the same way as done in present-time.

14.

>If you were to remove from your DME all capabilities having to do  
>with perception and control of classification, etc, etc, system  
>concepts, the only perceptual function left would be the generalized  
>one I call awareness. No "decision-making" would be required -- all  
>decision-making would then consist of applying learned rules in  
>learned ways to lower-level experiences.

Given your hypothesis, your conclusion follows. But the only way this learned hierarchy can be modified is through action of the reorganizing system. Your

hierarchy is intrinsically automatic. It includes no way to take "rational considerations into account," other than those already learned. Having learned arithmetic, how can the hierarchy learn algebra? Does learning algebra depend on intrinsic error?

Please note: the DME does not include separate, much less duplicative systems. It has the ability to direct its awareness throughout the hierarchy, making full use of the learned systems. See 13.

15.

>All deviations from well-learned rules would amount to random  
>reorganization, which takes no rational considerations into account."

Any deviation from "well-learned rules" would result from "anticipation" controlled by the DME. The DME, using its access to the learned hierarchy would have selected the "deviations" on the basis of relevance to the present-time situation, combined with anticipated outcome. This is far from random. Unexpected side-effects are minimized. The outcome may not fit the external actuality because the contents of the learned hierarchy may be incomplete or even at variance with the external realities.

16.

>And you also seem to include in the DME the capacity for awareness  
>and random reorganization, thus stuffing my reorganizing system into  
>the same package."

Although the DME includes awareness of intrinsic signals, it is not limited to them. In this sense, the DME includes the reorganizing system.

If the DME is unable to find an acceptable outcome, the only remaining alternatives are doing nothing or acting randomly. This may or may not result in reorganization, and it may or may not result in improvement of the present-time situation. In this extreme situation, the operation of the DME has been reduced very nearly to the operation of the original reorganizing system.

Regarding the reorganizing system in comparison with the DME, they differ in two major ways:

1. The DME has access to ("can become aware of") any existing perceptual signals, whether present-time, past-time imagining, or future-time imagining. The reorganizing system is limited to intrinsic signals.

2. The DME can establish anticipated goals, "imagined outcomes," based on past-time experiences. Such experiences would include whatever reasoning skills that may be available from memories and learned systems. Thus the DME has available whatever rational skills the individual may have acquired. The reorganizing system has no such capability.

I hope these responses help clarify the DME concept.

Regards, Bob Clark

Date: Sat Mar 05, 1994 12:20 pm PST

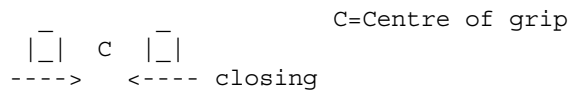
Subject: Re: Private mail goof; robot gripper; why statistics?

[From Marcos Rodrigues (030394.14:30 GMT)] Bill Powers (940302.0800 MST):

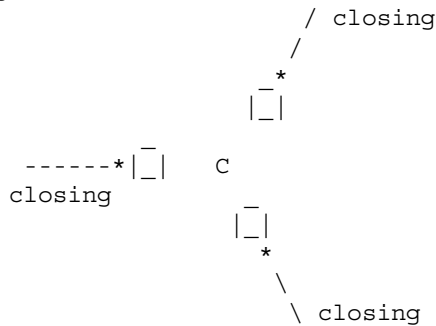
>From the way you present the problem, and from what you haven't  
 >said, I am guessing that the gripper fingers are constrained to  
 >move together, and that they always lie on one circle. So you  
 >need to position the gripper so 2, 3, or 4 points of contact are  
 >made simultaneously as the fingers converge toward a center of grip.

Yes, that's the right assumption. I cannot modify the gripper design at this stage. A top view of the three available grippers look like the following:

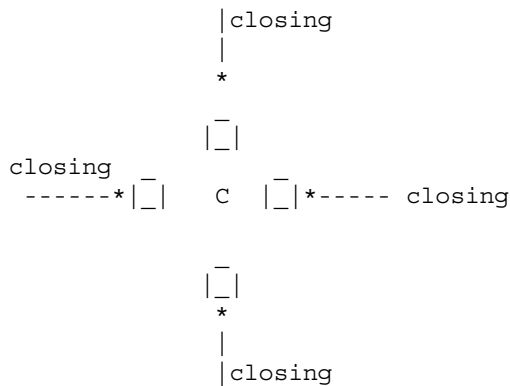
Two fingers:



Three fingers:



Four fingers:

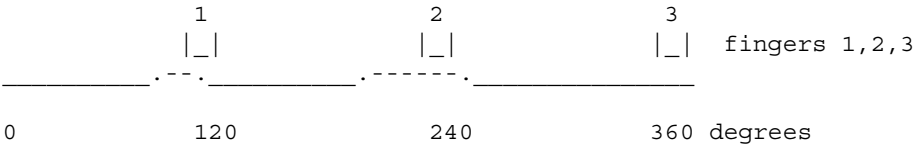


So I'm talking about having 3 different grippers. Looking from the centre of grip, two fingers are separated by 180 degrees, three fingers by 120 degrees and 4 fingers by 90 degrees.

>I can see that gripping can involve a problem of the slopes of  
 >the surfaces gripped (the picking-up-a-wet-bar-of-soap problem).

>This implies satisfying several conditions at the same time:

This is precisely the problem I would like to consider using a learning algorithm. If the contact points are in slope which is not convenient for grasping, the search should continue until smoother contact points are found. Suppose the object I want to grasp is circular, and I'm using a three-finger gripper. Flattening out the object's outline and the circle of grip (with the fingers) we would have:



This clearly is not the best position for fingers 1,2,3 due to irregularities on the object's outline. The learning algorithm should then move the fingers to the left (or right) until a better position is found.

But this is the next step. I have to start by determining control variables and their reference values so that the initial position for the fingers is determined. The circle-fitting problem. The grippers do not totally close even if an object is not picked, so that there is a minimum and a maximum diameter inside whose limits the grip circle can grow or shrink. If there are 2, 3, or 4 points on the circle that are "touching" the object outline at the same time AND if they are equidistant (180, 120, or 90 degrees) then they are contact points.

Thanks for your thoughts,

Marcos.

Date: Sat Mar 05, 1994 12:36 pm PST  
Subject: The nature of information theory

<Martin Taylor 940205 15:00>

I'll try to make this quick, since the sleep study leaves me very little time for CSG-L.

Last week Rick asked me a straightforward question about how one measured uncertainty and information, and I gave him a procedure. Then I noted that I had forgotten to point out that the measure he asked for would naturally give a value of zero in a well-functioning control system, and pointed out that the derivatives of the perceptual and disturbance signal were more interesting items for which to take the uncertainty and information measures.

I was taking my few available moments to develop a posting to indicate the procedure and expectations for what to measure and what to expect of the measures in a control system, but before I got half-way through, Bill P. produced a scattergram of disturbance derivative versus perceptual signal derivative, which looked pretty much as expected (i.e. a big blob).

Rick then weighed in with a largely predictable diatribe about the new proof that information was irrelevant to control theory. At this point I stopped participating in the public debate, and have sneaked in a few moments to initiate

a technical discussion with Bill P., based on the analysis of real tracking data. We both hope that the results will bring us to a common understanding that we will be able to share publicly. (It may not be what either of us currently believes, but that's fine by me if it happens).

=====

This posting is not about information theory and control. It is about the nature of information theory itself, provoked by two diametrically opposed views in recent postings:

>Rick Marken (940302.0930)

>All of Martin's SPECIFIC mistakes are made in the service of preserving a  
>belief in information theory -- a theory of human behavior that Martin was  
>attached to prior to discovering PCT.

Bill Powers (various--paraphrased because I can't find an original)

>Information theory is a mathematical set of forms that may well be internally  
>coherent, but has no grounding in things I can perceive, and for which I can  
>see no relevance.

I hope I characterize Bill's position correctly--I thought I had kept the latest posting with this kind of comment, but in a quick scan I can't find it.

Bill (if I understand him properly) has a very reasonable position. Information theory has the same kind of status as Fourier analysis or the Calculus of Variations. It is a coherent set of mathematical operations, in which certain constructions recur in many places. In Fourier analysis, the direct and inverse Fourier transforms are constructions that are sufficiently frequent to be worth giving names. Nobody has ever experienced a Fourier transform, but a lot of people are familiar enough with them to be quite happy to consider them as unitary operations on extended variables. I see them as "twisting" the space in which signals are visualized or described. Short-term Fourier transforms only partially twist the space. But that's my perception, not part of the mathematics.

Shannon entitled his book "The Mathematical Theory of Communication," not the Theory of Information. His perceptual reality, in the application toward which the book was aimed, was the ability of someone at one end of a line to know what had been happening at the other end of the line, if the line was unreliable. (After my first posting quoting Shannon, I was informed that the original application was wartime cryptography; useful mathematics finds many applications).

Central to Shannon's work was a formula " $\sum p_i \log p_i$ ," which he demonstrated to be the only formula that satisfied three simple criteria for a construct that we might call "X". [ $p_i$  is the probability that the  $i$ th even in a set of  $n$  possibilities is the one that occurs.]

1. H should be continuous in the  $p_i$ .
2. If all the  $p_i$  are equal,  $p_i=1/n$ , then H should be a monotonic increasing function of  $n$ . With equally likely events there is more choice, or uncertainty, when there are more possible events.
3. If a choice be broken down into two successive choices, the

original H should be the weighted sum of the individual values of H.

Shannon's perception was that these criteria for construct "X" satisfied his concept of the term "uncertainty." Many other people have had the same perception, including myself. As the interchanges on CSG-L have shown me, not everyone shares this perception, and neither do they share the perception that what we normally call "information" corresponds to a reduction in "uncertainty." Enough people did, however, that the "Theory of Communication" became colloquially known as "Information Theory."

There's no problem in the fact that not everyone agrees to the meaning of any particular word. There can be a problem if someone thinks that there does not exist a construct useful in the real world corresponding to a word other people use for a construct meaningful to them. One or other is likely to be wrong.

One of the "powers" of PCT is its approach to "reality." Reality is what relates output to perception. If output has a random effect on perception, that is the same as if the perceptual signal being (ineffectively) controlled for is not a "real" entity in the world. Perceptual control depends on the existence of some stability in the way output affects perception, which in turn is as if the CEV had some objective reality. I think this is the closest we will ever come to knowing what is "out there." We perceive CEVs that interact with their environments in ways we can influence with some moderate consistency. Other CEVs get reorganized out of existence, in the sense that we cease perceiving them and perceive something else.

This argument applies to mathematical abstractions as readily as to telephones and chairs. If you can manipulate them in ways that affect your perceptions in some consistent way, they are in some sense "real." If they are a part of affecting perceptions of non-mathematical things, like airborne masses of winged metal, they are even more "real." My sense of what Bill P is saying about information theory in the following quote is that it may be real in the first sense, to him, but not in the second.

>Bill Powers (940301.2200)

I use a three-level distinction here: I believe, I do not believe, I disbelieve. I do not believe in information or probability; neither do I disbelieve in them. I simply find no connection between these quantities and anything I can experience.

What Bill and I are, or hope to be, doing, is to test out the second sense of reality, in which the "control system" construct takes the place of the airborne mass of winged metal. At least that's my perception of what we hope to do.

Rick's comment is diametrically opposed to Bill's view. He takes "information theory" to be a theory of human behaviour, to which I have been attached. As such, it just HAS to be in conflict with a control-system theory of human behaviour, presumably because it is a different theory of human behaviour.

Information theory is not, and never has been, a theory of human behaviour. Just like Fourier analysis and the Calculus of variations, it is a mathematical tool that has had (to some people) obvious application in the study of human behaviour. I have used it myself for this purpose, to good effect.

I "believe in" information theory because the theorems do not seem to be mutually contradictory, and because it follows from the normal rules of algebra and

calculus. I "believe in" its usefulness for analyzing human behaviour because its fundamental assumptions depend ONLY on there being at least two variables, one of which might influence the other. This being the case in control systems, it seems a priori reasonable to me to use information theory in examining the behaviour of control systems. I also "believe in" its usefulness as a tool because it has been useful in analyzing psychophysical results.

Whether control systems are relevant to human behaviour is independent of the question of whether information theory is an appropriate mathematical tool to examine the behaviour of control systems. I happen to think both are true, but it is reasonable to question either--independently.

It may well be the case that I make mistakes in applying information theory to control systems. It may even be the case that one will learn nothing new from the correct application of information theory to control systems. It CANNOT be the case that it is "wrong" to use information theory in analyzing control systems, or even to use it in looking at human behaviour. To say it is wrong to do so is as ridiculous as to say that it is wrong to do spectral analyses of control systems. Well, to do spectral analyses may give you misleading or useless results, and it will, if there is any non-linearity in the control system, but it cannot be "wrong" to do the analyses.

To say that "The claim that there is information about the disturbance in perception was shown to be clearly wrong" is a falsehood, but I presume not a lie. It comes under the category of "what I say three times is true." What was shown by demonstration is that there is almost enough information about the disturbance in the perceptual signal to permit the disturbance to be reconstructed, and under special circumstances the information is complete. It was also agreed, though not demonstrated, the complete reconstruction of the disturbance signal is not possible when the feedback function or the output function is variable. What was shown in Bill's recent, if somewhat premature, presentation, is that even in real, human, control, there is SOME information even about the disturbance value in the perceptual signal. In fact there is quite a bit more than I would have expected to see.

To claim that there is no information about the disturbance in the perceptual signal is to say that there is no influence of the disturbance on perception or on the changes in the perceptual signal. This situation can occur ONLY in an infinite gain system with zero transport lag.

The question that is interesting is the one Tom Bourbon keeps asking. Can we learn more about the workings of control by using uncertainty analysis than by using signal-processing techniques alone. I think we can, but I accept that this proposition has not been demonstrated satisfactorily so far. And until it has, the possibility exists that it may be false.

I shall probably not return to this discussion until Bill P. and I have something useful to say about real data.

Martin

PS. Don't expect much from me over the next few weeks. I steal a few moments now and then to deal with this stuff. I calculate that my commitments next week will leave me about 8 hours to do what I usually have the full working week to do. So

postings here will be hurried. As someone (Voltaire?) said: I apologize for the length of this letter, but I didn't have time to make it shorter.

Date: Sat Mar 05, 1994 1:27 pm PST  
Subject: Facts; Information in perception

[From Bill Powers (940305.1135 MST)] Dan Miller (940305.1200)

>We agree that scientific facts (data) are objects, right?

Well, they are perceptions. A fact is a perception that you can reliably reproduce, either just by repeated observation under repeated (observed) conditions, or by acting on the environment in particular ways. A fact can be a relationship between two perceptions (the orbit of the moon and earth about a common center of gravity). It can be a temporal sequencing (the punch that knocked the fighter out came after, not before, the bell). They can be principles (he is playing chess defensively). To reduce all facts to "objects," it seems to me, is to eliminate many things we call facts.

>Knowing how they are constructed seems to me to be as important  
>as the facts themselves.

When you want to relate a fact to something else, this knowledge is important. In other cases it is not. You can know that you have a toothache without knowing how the perception of pain was constructed. The toothache is still a fact. You can control this perception without knowing why putting a little white pill in your mouth makes the pain lessen. On the other hand, if you consider the fact that a voltmeter reads 5.32 volts, knowing what to make of this fact depends on knowing how the voltmeter works, at least to the extent of knowing that it is working normally and that using it is not altering the voltage being measured. Knowing the meaning of this reading in terms of an external situation depends entirely on the theory of electronics.

To me, what makes a fact "scientific" (i.e., reliable) is that anyone can observe it and within a common framework of principles and methods describe it and control it in the same way.

>Indeed, I have great difficulty separating the consequents from the act.

Heck, that's easy. When I open door, I do it by pushing on the door. The opening of the door is the consequent; the act is the exertion of a force. Entirely different things. I don't understand what you mean.

>Mr. Greenspan is a true believer in his theory and its  
>contribution to the health of the U.S. economy. He believes  
>more in his imagined facts than in other economists'  
>constructed facts. Still, I would want to know how the  
>economists (all of them) construct their facts and for what  
>purposes.

Mr. Greenspan and most economists agree that inflation is indicated by a series of measurements which lead to a number representing the rate of change of purchasing power of the dollar (the change in the price index). He and most economists have the same understanding of the meaning of "rediscount rate" (on which the prime



rate is based). The fact that is in dispute is neither one of these, but the relationship between them. Greenspan believes in a proposed fact, which is that increasing the rediscount rate lowers the rate of inflation. The actual relationship is perceivable by anyone who consults the historical record, in which previous inflation rates and rediscount rates are recorded. The actual relationship is the opposite of the relationship in which Greenspan apparently believes, and in which most economists apparently believe.

Another example of imaginary facts is "mass hysteria," an imagined feature of crowd behavior. I refer you to Clark McPhail on this subject.

>It is not likely that Mr. Greenspan will modify (or drop) his  
>theory of interest rates and economic "health." Even as a true  
>believer who pays little attention to other facts, his facts  
>have worked for him for quite some time.

No, they haven't. They have been imagined to work. They are not even facts, because there is nothing behind them that anyone else can perceive.

>Rick says:

<Scientific facts are NOT controlled perceptions; the swirl of  
<points that Bill Powers posted were not brought to that state  
<by Bill's actions.

>Then how did they get there? Did they just appear from thin  
>air? Or were they a consequence of his purposive actions? He  
>set up conditions, plugged in his model, typed a sequence of  
>instructions, and scientific facts appeared that conformed to  
>his predictions.

There was nothing inherent in my actions determining that the experimental points lay where they did. I set up the framework in which points could be plotted according to their values, and Martin Taylor set up the mathematical manipulations that derived those values from the observed behavior of a disturbance and a person moving a handle. To that extent the presentation of the facts was constructed. But neither Martin nor I had any control over what the experimental data would turn out to be -- that depended only how how the person in the experiment moved the handle as the disturbance varied. If that person had moved the handle differently, the points would have been differently distributed in the plot. The discussion was not about the method, but about the distribution of the points. That was not a consequence of either my purposive actions or any theory. It was a consequence of how the experimental subject actually behaved.

This is an important point about controlling perceptions, which crops up repeatedly. Once you have decided on the mode of perception -- whether to perceive the fullness or the emptiness of a glass of water -- the state of the selected kind of perception depends on what the outside world does. If someone pours water into the glass, what you perceive will depend on the mode of perception AND on what is happening outside. You will perceive the emptiness decreasing, or the fullness increasing.

So perceived facts really have two independent dimensions. One dimension is the kind of interpretation that converts raw sensory inputs into a particular kind of perception. The other is the way that kind of perception then behaves as events

unfold in the outside world. The organism determines the first dimension, but the second is imposed by something independent of the organism.

All this, of course, assumes that you are actually discussing perceptions based on sensory inputs. If you are only imagining the perceptions -- manipulating their states internally -- then you are not dealing with facts.

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Kent McClelland (940305) --

>Rick, Martin, Bill P., Tom

>I had hoped in making some remarks about information earlier  
>this week (940228) not to provoke people to go back over old  
>ground but to encourage them to take a fresh look at the  
>subject. However, it seemed to me that most of the comments on  
>my post had little to do with the substance of my suggestion  
>that the colloquial understanding of information can be given a  
>meaningful definition within the PCT worldview.

The problem with the word "information" even in its colloquial sense is that it implies knowledge of two things: 1, the representation or perception that is said to contain information, and 2, that which is represented or perceived. The first part is OK, but the second implies some way of knowing the external counterpart of a perception other than by perceiving it through the usual channels. When you say that a perception gives you information about something, the "aboutness" is assumed, and depends on what model of the external world you use.

If we were all always careful, we could use and understand the term information to mean a perception that we assume corresponds to some aspect of a model of the external world. But that is not how it is generally used, especially not colloquially. You can be arrested on the basis of information about your illegal activities even if you haven't committed any. To use the term "information" rather than "allegation" is to assert an objectivity that doesn't exist.

>I think that Rick, Bill, and Tom are probably right in their  
>oft-repeated argument that a control system does not get any  
>information about the disturbance when it operates in what Bill  
>has called a "quasi-static" mode, that is, using a constant  
>reference signal. But I doubt that they really mean to imply  
>that living control systems are hermetically sealed off from  
>getting any information (in the colloquial sense) from their  
>environments, which is one way you could take that argument.

You are overinterpreting. To say that an organism can't get information about a disturbance from its perceptions doesn't mean that it can't get at least alleged information about the controlled quantity from its perceptions. Remember that "disturbance," in PCT, does NOT mean "change in the controlled quantity." It means "change in an influence that tends to alter the controlled quantity." The disturbance is an influencing variable, not the variable on which that influence acts. That influence does not necessarily have any effect on the controlled quantity -- it could be cancelled by the system's actions, a second influence on the same quantity.

Imagine that you're playing the rubber-band game in the role of the control system. Attached to the knot is a piece of string that disappears through a tiny

hole in the wall. In the next room, the disturbance exists and acts by pulling on the string. You, however, can't perceive the disturbance itself. To see what this means, consider two different ways of applying the disturbance, in the other room.

1. The other end of the string is attached to a rubber band, and another person is pulling on the free end of the rubber band.
2. The other end of the string is attached to five rubber bands, and five people are independently pulling on them by different amounts.

Clearly, even if the person being the control system can perceive and control the position of the knot, there is no way for that person to guess what kind of disturbance or disturbances exist in the other room. This is what we mean by saying that the perception contains no information about the disturbance. Even if you know there's only one person in the other room, you can't tell how far that person has pulled back the end of the rubber band, because you don't know how thick or linear the rubber band is.

Even if you redefine the disturbance to mean only the tension in the string, the control system still can't perceive that tension. The perception represents only the position of the knot. In order to perceive that a deviation in position implies a certain tension in the string, it would be necessary for a higher-level system to perceive both the deviation of the knot and the amount of force being applied by the controlling system to keep the knot where it is. If that higher-level control system understood control theory, or at least Newton's laws, it could deduce that the force being exerted to keep the knot from moving must be matched by another unobserved force due to the tension in the string. That knowledge, however, would exist only in the higher-level system, and it would amount to an inference, not a perception. That higher system does not have to exist in order for the lower-level system to control the position of the knot at either a fixed or a variable reference level.

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>The experience of "getting information" happens when a
>dynamically operating control system changes its reference
>signal (on some hierarchical level) to achieve better control
>of its perceptions (reduce error) and in that way to resist
>disturbances (at a higher level) more effectively.
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I think you're trying to get at what I just described. Notice how you must have at least two levels of control! If you consider only the higher level, you are back to the same problem.

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In a more technical, or at least mathematical, vein:

I've been working with Martin to apply his mathematical method for computing the amount of information about the disturbance in the perceptual signal (or in the controlled quantity, assumed to correspond to the perceptual signal). I'm using data from a real experimental run. I'm not at all sure that I've programmed the computation correctly, but I do get a number: about 2.2. This does me very little good, because I can't assign any units to that number: it just says that we have 2.2 of something. I assume this will be explained to me.

However, in doing these computations I have realized something about formal definitions of information, uncertainty, and probability, and have been led to ask a question I've never asked before. What kind of information is allegedly there

in the perceptual signal about the disturbance? That is, what exactly can we learn about the disturbance by analyzing the perceptual signal for information content?

One step in the computation is to construct a matrix of probability values. This is done by scanning through the record of disturbance values and values of the perceptual signal or controlled variable, both obtained from the experiment. There are 3600 time-slots, and I have divided the amplitude range into 51 by 51 cells (to keep the size of the matrix manageable). The procedure is to scan simultaneously through the two data tables, and for each pair of data points, increment the corresponding cell in the matrix by one. What you end up with is a matrix in which the number in cell  $x,y$  indicates how many times during the run the value of disturbance was  $x$  and the value of the perceptual signal was  $y$  (both scaled for a total range of 51 units).

What's important to see is that if the entry in a cell is 250, this says only that this particular pair of values of disturbance and perceptual signal occurred 250 times during the run. There is no indication in that number as to WHEN any particular pair occurred. In computing this matrix, therefore, we have discarded all knowledge about the perceptual signal and the disturbance as functions of time. We will not, therefore, get any result from that point forward that refers to the temporal characteristics of either the perceptual signal or the disturbance. Whatever 2.2 units of information refers to, it does not refer to information about the waveforms of the perceptual signal or the disturbance. Actually, the computation used the first derivatives of both variables, but it is still true that there is no information about WHEN those values of derivatives occurred during the run. So the waveforms of those derivatives are discarded.

Assuming that the number 2.2 isn't just a mistake, what could those 2.2 units of information be about? They can only be about the aspects of the data that are preserved by the mathematical operations used to create the matrix.

The entries are converted to probabilities by dividing each one by the total number of data points, which is only a scaling factor. There are several aspects of the data that are preserved. We find that there is a distribution of probabilities with a strong peak occurring very near the cell  $dd/dt = 0, dp/dt = 0$ . The most probable position in the matrix implies zero rate of change of disturbance and zero rate of change of perceptual signal. So this is one piece of information we can get from the perceptual signal: given a perceptual signal of zero, the most likely value of the disturbance is about zero.

Another piece of information comes from the distribution of probabilities. Given a value of the perceptual signal, we can deduce the corresponding \_spread\_ in the disturbance values: the uncertainty in the disturbance, given the perceptual signal. This is information, too. So whether I have obtained the right number or not, it is clear that there is information in the perceptual signal about the disturbance.

This information, however, is only about the \_whole run\_. Because we have discarded all data about when any particular entry occurred, we can speak only of characteristics of the perceptual signal and disturbance that apply to all the data points, not to any particular pair of points. To say that a zero value of perceptual signal implies a zero value of the disturbance (with a certain probability) is not to say that the disturbance is zero, or even that whenever the perceptual signal is zero, the disturbance must be zero. We can't speak of "when"

any piece of information obtained this way is true. Those 2.2 units of information are not true of any one part of the run; they are true only of the entire run.

This kind of information is not useful in explaining how a control system works, even though it is, indeed, information. Exactly the same measures would be obtained if the perception-disturbance pairs occurred in any other sequence. In this sense, information measures are much like correlations. They are handy ways of characterizing an entire set of data points, and we do get some useful information out of such treatments. But for either correlations or computations of information, we do not get the kind of information that will tell us how the system works, or that can be used to predict the time-course of the variables involved.

Best, Bill P.

Date: Sat Mar 05, 1994 2:13 pm PST  
Subject: Re: Information--one more try

[Dan Miller (940305)] Kent McClelland:

I agree, wholeheartedly. We need to see living control systems as they work in more worldly situations.

Dan Miller

Date: Sat Mar 05, 1994 2:20 pm PST  
Subject: Re: computations of Information in perception

<Martin Taylor 940305 17:00> >Bill Powers (940305.1135)

Bill has been working on computations of uncertainty and information.

>However, in doing these computations I have realized something  
>about formal definitions of information, uncertainty, and  
>probability, and have been led to ask a question I've never asked  
>before. What kind of information is allegedly there in the  
>perceptual signal about the disturbance? That is, what exactly  
>can we learn about the disturbance by analyzing the perceptual  
>signal for information content?

>...

>Assuming that the number 2.2 isn't just a mistake, what could  
>those 2.2 units of information be about? They can only be about  
>the aspects of the data that are preserved by the mathematical  
>operations used to create the matrix.

>...

>This information, however, is only about the whole run. Because  
>we have discarded all data about when any particular entry  
>occurred, we can speak only of characteristics of the perceptual  
>signal and disturbance that apply to all the data points, not to  
>any particular pair of points.

>... We can't speak of "when" any piece of  
>information obtained this way is true. Those 2.2 units of  
>information are not true of any one part of the run; they are

>true only of the entire run.

>...

>Exactly the same measures would be obtained if the perception-  
>disturbance pairs occurred in any other sequence.

All of this is quite correct. But I omitted part of a paragraph:

> In computing this matrix, therefore, we have discarded  
>all knowledge about the perceptual signal and the disturbance as  
>functions of time. We will not, therefore, get any result from  
>that point forward that refers to the temporal characteristics of  
>either the perceptual signal or the disturbance. Whatever 2.2  
>units of information refers to, it does not refer to information  
>about the waveforms of the perceptual signal or the disturbance.

This is also true, but it depends on the statement "In computing THIS matrix." It would not be true in computing another matrix in which the temporal characteristics were incorporated. Just as a perceptual signal is only a value, and the value says nothing about what "kind" of thing is being perceived, so an information or uncertainty value is only a value, telling you nothing about what "kind" of information it is concerned with.

A perceptual signal is of a "kind" determined by its perceptual function and the perceptual functions of all lower control systems that contribute to its input. If you want to know the kind of perception that has a value of 65.2, you must enquire what those functions are. Similarly, if you want to know what "kind" of information those 2.2 units refer to, you must ask what conditions the data arise from. In the case Bill is talking about, the data are of the value of the model perceptual signal at some time after the disturbance signal has a particular value (or maybe it is vice-versa), but either way, as he says, there is nothing there that relates to the waveforms, and the points could have occurred in any order. There's nothing in that computation that specifies order.

But there could have been, if Bill had wanted to compute information of THAT kind. He could, for example, have looked at the distribution of disturbance values at time  $t+\tau$ , given that at time  $t$  it had some value and the derivative had some value. From the three-dimensional matrix that arises for each value of  $\tau$ , he could compute, using the same analysis procedures, the information about the earlier state of the disturbance that remains in the later state. From that, he could determine not a specific waveform, but the sorts of waveform that the disturbance followed, as well as information rates, which are where the action is when we are dealing with control systems.

Likewise, instead of using the disturbance at  $t+\tau$ , he could use the perceptual signal at  $t+\tau$ , and take as conditionals values for the disturbance at  $t$  and the perceptual signal at  $t$ -lag, or any other group of values. All these things will give you numbers. Numbers don't tell you what "kind" they are, because they are only of kind "number". To know their "kind" you must know how they were obtained. And if temporal data are not there initially, the results will not refer to temporal characteristics.

I don't know if this clarifies anything, beign very quickly dashed off-- Bill's posting arrived as I had my coat on to go home. It's a nice Saturday, and that's where I'd rather be.

Martin

Date: Sat Mar 05, 1994 8:59 pm PST  
Subject: Systems Theory and PCT (long)

[Cliff Joslyn (940305, all day long!)]

Sorry for the delay. As I said, I've just put my dissertation in the mail to my committee. Couldn't think of anything else there for a few days. Wish me luck! Anyone want to talk about possibilistic information theory or qualitative modeling? ;->

This dialog, although invigorating, is also a bit wearing. I might not be able to continue it indefinitely, and I take no joy in fighting with some of you people. I'll try to discipline myself to write less and say more. I have a tendency to pick nits and argue every point, which gets tedious. But I don't like it when people ignore MY comments. And I HATE to lose an argument. ;->

In order to avoid some repetition in the replies, first I lay out some points in labeled paragraphs, clarifying my position, so we're all punching at the same targets. Then there are specific replies to Tom, Bill, Rick, and Mary as appropriate, referring to those paragraphs.

Also, let me recommend the liberal use of the ;-> mark to help our civility all around.

=====

[A] I'm still a bit amazed how defensive "you people" are. You act like I've attacked PCT, which I never did. Remember, I didn't ask for this argument, and I'm well aware that the whole thing is tangential to the core of PCT and the reason for CSG-L. I was merely defending MY profession against a scurrilous, ignorant, and (because he claims ST doesn't even have anything to do with PCT) completely gratuitous attack (Rick has yet to admit any of this, by the way). I had no intention of championing ST with respect to PCT, or claiming any kind of primacy for ST over PCT. But it seems y'all were EXPECTING me to, and my defense sprung an ambush.

[B] I'll stop trying to be so legalistic. It's my natural inclination, but Rick, you REALLY piss me off. Your style is very annoying to me. Your points may very well be correct, and I have complemented your work. But this isn't the "jar loose your old ideas" GOOD kind of annoyance, not just challenging my ideas. I may join others who think that your style detracts from your ability to communicate with and convince others. That's too bad. I hope I don't go to such "legalistic" excesses with other people, but maybe I do. And no, I'm sure you're not an asshole. Just a bit of a hothead.

[C] I'll stop trying to defend my "Bingo" remark that anyone who uses feedback and does good science is "doing PCT". Any victory I'd have here would be purely semantic, concerning the meaning of "doing". I don't really care about this, and looking back on it, I'm not even sure what I meant. I agree with y'all's basic idea: ALL the scientific communities, including ST, psychology, etc., have completely ignored PCT. OK? (Unlike some other people around here, I admit when I'm wrong, and don't just ignore legitimate challenges.)

[D] I am trained as a systems theorist, one of the few who really has been. And I am quite an unusual ST person, not least of all in being a PCT adherent. Perhaps that's because I HAVE been so trained. Also, I hold ST and Cybernetics (ST/Cyb) to be two aspects of ONE discipline, like electricity and magnetism (this is a whole other argument, but one I think I could win pretty handily). Not that you people care anyway (all you care about is PCT), I'm just trying to make it clear.

[E] I agree that the current state of ST/Cyb is generally moribund, and, with exceptions, has been declining steadily since the late 1960's. The ISSS in particular is so weak right now that they've given over things mostly to the "global managers" and spiritualists. There are VERY few people doing good ST/Cyb right now. As bad as you people think you have it, we're worse of substantively. All we have left is really the inheritance of our old institutions: a few journals, a few professors, a few societies.

[F] I regard PCT as one of those few places where good ST/Cyb is still being done. In my 940224, I specified how PCT is a part of ST (e.g. concerned with organization, not substance). In many ways, but not completely, PCT represents a rare legitimate continuation of the entire ST/Cyb approach. When I read BCP I had to rework all my ideas about biosemiotics in order to include the Powers model. But all it did was strengthen my ST, and allowed me to better connect to and critique others'.

[G] My position should not bother any of you. I have never claimed that ST has any important consequences for PCT (at least in their current states of development), but rather the other way around: PCT has PROFOUND consequences for ST (that's because PCT is a KIND OF ST, and a successful one). But that's a problem for my (ST) community: we have to pay attention to you. There's no need for y'all to care about ST.

[H] I do NOT defend ANY of my colleagues for either their low quality work, or for ignoring PCT. I have been highly critical of them myself, and will continue to try to educate them not only about PCT, but the continuing value of the entire so-called "first order cybernetics" approach, of which PCT is a part.

[I] Since PCT is a part of ST/Cyb, ST/Cyb necessarily is concerned with more than just control systems. In particular, it is concerned with systems, like purely physical systems, which are in no way control systems; and with systems, like economies, societies, and ecologies, which are MIXTURES of control and non-control systems.

[J] I think that it is manifestly evident that the early (pre-1970's) ST/Cyb movement is the "intellectual heritage" (whatever that means) of PCT. Any history of ST/Cyb in this century would have to include the Powers' school as a prominent chapter. More on that below.

[K] The deeply unfortunate political split between ST proper and Cyb is reflected in a similar split between those who place prominence on non-control vs. control, and (relatively) closed vs. (relatively) open, systems respectively. As I suggested in my previous remark to Mary, Cyb is concerned more with closed control systems. The intellectual climate within ST is biased against closure, and towards openness, despite their mutual necessity and ubiquity; and they ignore control almost completely. REAL systems, like organisms and communities of organisms, are a mixture of the two.



[L] Mary and Rick are correct if they think that the open systems approach of ST proper is exemplified by the von Bertalanffy-Prigogine emphasis on thermodynamic self-organization. And I agree with Rick that you can't explain ANYTHING interesting with JUST chaos, self-organization, and far-from equilibrium thermodynamics.

[M] That's because these phenomena, while fascinating in and of themselves, don't get close to the phenomenon of LIFE, which is where the entire bundle of issues relating to feedback, control, purpose, and semantics converge. This is the BIOSEMIOTIC view which I propound, in which I try to link ST with semiotics and PCT.

[N] And that's not to say that self-organization etc. is not IMPORTANT for PCT in general (I'm not CLAIMING that it IS, I'm SUGGESTING that it MIGHT BE). As some have remarked, I suspect you can't GROW (as opposed to CONSTRUCT) a control system without far-from equilibrium thermodynamics. Cyb (and PCT) is concerned with the OPERATION of control systems. ST proper is more concerned with their EVOLUTION: how does the hierarchy emerge? How did the lowest level evolve from non-control? My colleagues and I are currently working with Bill (very slowly!) to clarify ideas about the origins of control systems. Let's see if ST can help there, perhaps, or help with the interaction between organisms and their physical environment, or organisms with each other. I'm not making any claims, merely exploring possibilities.

=====

>From Tom Bourbon [940223.0825]

>

>A few days ago, Rick Marken [specific references are not available to me in  
>this text editor] stirred up a hive of killer bees.

Buzz! But hey, what about honey bees? We're all friends here, I trust. Also, I should note that I usually agree with your comments.

>Then they stung him with claims that systems science, in  
>general, and cybernetic science, in particular, (1) are foundational to PCT  
>and (2) "inform" PCT.  
>informs, inspires and animates PCT.

I'll be happy to argue about the semantics of "foundation" and "inform" with you any day, but first things first: I don't think I said this. My 940220 reply contains the string "inform" exactly once, where I asked "what are you saying, Rick: ST has not informed PCT AT ALL?" I didn't define information here. "inspir" and "animat" appeared 0 times. What I really think is [J], and see below.

>lists of systems scientists whose work he should have read, but had not.

We need more :-> marks, Tom. I didn't say he "should have" read them already. In order to query his knowledge of ST, I asked him to comment on the work of ANY of THE most prominent ST people. He could not. Anticipating this, I advised him of a (relatively) short, well-written, cogent summary of the field by my teacher, and suggested a discussion based on that. I don't see that we could have an intelligent conversation about ST without at least that common level of understanding, and I cannot imagine a more civil suggestion.

>include me among those  
>who say PCT originated independently of systems science and cybernetics and  
>that neither of those two fields has yet "informed" PCT.

I think Bill's post (940224.2030 MST) has set the historical record straight. See more below.

>In 1977-78, systems scientists taught me how to do modeling and simulation.  
>the SGSR provided me a well-rounded exposure to systems research and modeling.

Well then I'm glad that we did contribute something after all.

>The IEEE people were not impressed with CST -- they saw it  
>as warmed over cybernetics (which it was not) and they were more interested  
>in optimization and models of optimal control.

I am disappointed, but not surprised. I am not impressed with the IEEE SMC people that much either. They're ST's electrical engineering descendants, and a bit too fascinated by things like standard approaches to robots, reactor control rooms, and missiles. These legitimately complex systems don't really NEED to act like people to do a good job. These people are intellectually closed.

>The SGSR people were not  
>impressed -- CST was merely cybernetics, which they all seemed to believe was  
>a very limited, mechanistic, and somewhat odd part of general systems theory.

Again, I can't defend these people. I've had the same experience, and have been similarly shocked. I simply DO NOT understand the perspective of the ST person who rejects cybernetics. Their substantive equivalence is as evident to me as anything I know, and has inspired me since I've been working.

>SGSR people were interested in complexity, Prigogine (order out of  
>chaos), stochastic models, systems dynamic modeling of political and  
>economic phenomena on a grand scale, and ending war.

Well put, and very true. See my [K], [L], [M].

>I joined SGSR and remained a member until 1988.  
>That makes two good ones and one  
>bad one, during 17 years; perhaps there were others I missed.

I'm sorry, but I don't understand why you participated in the SGSR and read all that material for so long? Did you find ANYTHING of value in all that investment of reading? Or perhaps you do more than just PCT (could it be? :->).

>During those seven or eight years, I saw  
>absolutely nothing that would pass as a standard or common "language,"  
>whether for talking about "general systems," or for modeling them.

Well here I do disagree. The Systems Methodologies track, led by my teacher George Klir, was very prominent in the SGSR in the early 1980's. You must have seen their material. I regard them at that time as ST's "last gasp". It was a standard, but unfortunately not common enough, language. But it was MATHEMATICAL, and the "global therapists" couldn't cope. That's the problem: doing GOOD ST really

requires formidable formal skills. That school was one of the foundations of ST, and flourished for a while (Mesarovic, Odum, Zadeh, Klir, a few others). This is now being passed on to computer people (Wymore, Pichler, Oren), and I hope to drag myself along with them. They can barely get themselves to one decent conference a year. It's very sad.

>Not one of them studied or modeled the  
>phenomenon of control as we all know and love it. Perhaps things have  
>changed since then.

You're correct here as well. See [K].

>A few of us attended the two  
>Gordon Research Conference on cybernetics, organized by the ASC.

Hey, cool! Who besides Bill and Mary were at the 1990 Gordon conference in Tilton? That's where I met them. I don't think that was an ASC proper event, but there were plenty of ASC people there.

[[ HISTORICAL ASIDE

I was already a training systems theorist when the News of Powers ;-> reached me slowly, through different channels. One was Continuing the Conversation, which we know as the abortive ASC bulletin. The articles praised Him like a God ;->, and I looked at it quickly, said "Hey, good old Ashby/Weiner cybernetics!", then thought, "Hmm, why don't I know anybody who's doing that NOW?"

Then a friend described Bill's style as "he just keeps repeating the same thing, and he won't listen to anyone else". I met many luminaries, including Our Leaders, and the 1990 Gordon Conference. Rather than yet another untalented half-schizophrenic ST religious nut, I saw an able demonstration of a remarkable hypothesis that nobody could respond to, let alone refute. Even then I argued with Bill that he should care to know about chaos (does anyone out there know about the possibility of chaotic activity in a (nonlinear) control system?).

Then I read BCP. It still has my bookmarks in it. I've only felt that way about a few books: von B., Ashby, Ken Sayre, Bateson, and Val Turchin. A real sense of wonder. I've never looked back.

END ASIDE ]]

>many said it  
>was aesthetically unpleasant -- ugly, is what they called it -- machine  
>-like and out of touch with life. They were more interested in Maturana  
>and Varela (autopoiesis), deconstructionism, poetry and ending war.

More agreement. Modern Cyberneticians are generally mystics, absorbed in so-called "Second Order Cybernetics" drivel and radical constructivism (I think I know most of the ones who aren't). For example, it's very sad reading "Observing Systems" and watching the transition from the Early to the Late von Foerster.

What do people think of the von Glasersfeld brand of constructivism?

>I joined ASC and remained a member until 1988.

Apparently I've just missed you in person. My first ASC conference was 1989 in Virginia Beach. A thoroughly frustrating experience.

>I do challenge a claim that  
>their work inspired or informed Bill Powers and his early collaborators, or  
>some of us whose collaborations are more recent. That claim is false.

I don't THINK I was claiming that literally. And again, Bill has clarified his view. See [A], [G], and [J].

>Further, I never saw anything to unambiguously support a claim that PCTers  
>"do general systems science" whether they know it or not.

No, I don't mean that PCTers are doing GENERAL systems science. Rather, they are doing CONTROL systems science, which is a specialized KIND OF systems science (see [F], and below). It's a specification. Does a chemist do physics? Does a biologist?

>I never saw evidence that general systems scientists  
>or cyberneticists were "doing PCT."

See [C].

=====

>[From Bill Powers (940224.2030 MST)]

>it was as much his organization as his ideas that turned me on.

So you'd put Introduction to Cybernetics as your point of departure with Ashby?

>I especially felt, later, that his drive for the utmost generality  
>was premature and based on only a sketchy understanding of  
>control systems. When I did, I gradually came to realize that  
>neither of them had learned very much about control systems.

Interesting: premature. You're probably right. Perhaps he would have gone in your direction if he had been less theoretically, and more practically (that is, scientifically) inclined. That does appear to be a curse of the ST/Cyb crowd (myself probably included).

>Unlike many other approaches, PCT does not assume an architecture  
>and then look for phenomena which fit it. It starts with the  
>simple fact that organisms can produce regular and disturbance-  
>resistant outcomes despite the fact that their motor outputs have  
>highly variable effects on the local environment. As far as we  
>know, this can be explained only if the organism is able to  
>represent the outcome inside itself, compare the current state of  
>the outcome with an internally-define intended state, and convert  
>the difference into an amount and direction of action that will  
>keep the difference small.

These utterly simple statements are SO deep, it's hard to really unpack them. The following is completely from within MY perspective.

For me, what makes a field a part of ST broadly is the fact that its results are independent of the particular type of phenomenon under consideration (rocks, organisms, economies, stars, atoms).

Some (many) of the Systems Sciences (for example, Neural Networks) start with a specific, but type-independent, engineering architecture, and then explore the phenomena which result. They usually try like hell to make them match what they expect to see.

PCT involves both (A) a type-independent phenomenon (control) and (B) a type-independent organization or architecture (feedback), and is thus a part of ST. You say "as far as we know, (A) requires (B)".

>But isn't this a platitude?

Certainly not.

>To claim on this basis that PCT is the ultimate general theory of control  
>is not legitimate and I do not make that claim.

In the paper of mine which you've read, I make an (admittedly theoretical) argument that (A) requires (B). I'm not sure if this is the same thing as claiming that PCT is the "ultimate theory of control".

At worst, (A) -> (B) is a falsifiable, scientific hypothesis, corroborable and disprovable by observed evidence. In other words, find a control system. Break it open. Is it negative feedback? If so, that's observational evidence for (A) -> (B). This is no different in principle from massing electrons or any other kind of scientific evidence gathering.

>You can show that a general theory is  
>consistent with its premises, but theorizing will not show  
>whether those premises are related to the real world or whether  
>some other set of premises would not serve just as well and will  
>not turn up tomorrow.

The problem of using induction from observed evidence to support empirical hypotheses, that is the scientific problem of how much and what kind of evidence is necessary in order to accept the hypothesis, is new to neither PCT nor ST. Nor is it different for hypotheses about (more) general systems than for any other kind of system. However, the more general the claim, the more general the measurable quantity against which it must be corroborated.

>It would be more surprising if a  
>real-world phenomenon required NO particular system architecture.

I don't understand. In ST, we think of this as the function-structure argument. In general, for each kind of activity or phenomenon (that is function, like control), there are multiple possible architectures (structures, like feedback systems) which can manifest it. The converse is generally not true: each structure acts in a certain way. Think of all the different ways to light a room, or build an adder. The idea that a particular function (control) REQUIRES a particular structure (two input, no output, single state, single loop, negative feedback) is non-trivial, and should certainly be interesting (to ST people).

>My beef with general systems theory is that while it purports to  
>apply to ALL systems, so far it has had to wait for others to  
>explain particular systems in detail before it can claim to have  
>known the result all along.

>If you can freely apply a basic  
>term to vastly different situations, you may create the illusion  
>of generality but what you actually achieve is vagueness.

Let me explain a little REAL ST (as opposed to all that self-organization  
everything-is-connected-to-everything-else Capra-esque Mindwalk crap).

It is well known in ST that as the generality of claims increases, so does their  
accuracy and applicability, but also so does their TRIVIALITY. Increased  
generality REQUIRES increased vagueness, and this can be a GOOD THING, depending  
on the level you want to work at. As in mathematics, you need to make the  
structure as loose as possible, but rich enough to have interesting results.  
Sometimes working at a slightly more abstract or qualitative level is vital.

So any ST person who ALWAYS works at the MOST general level ends up just arguing  
about the meaning of "system". Now there's a kind of value in that, but it's  
philosophical, not scientific. The ideas that ALWAYS apply to ALL systems are  
necessarily the most trivial: everything has an inside and an outside; everything  
exists in space-time, etc. Big deal.

The way that ST people actually work is by quickly moving from the MOST general  
kinds of systems to consider more specific kinds: systems with input, or output;  
memoryless or systems with memory; crisp or fuzzy systems, state systems;  
deterministic or non-deterministic systems; linear or nonlinear; hierarchical  
systems; looped or loopless systems; control systems, feedback systems, etc. etc.  
etc.

It seems to me that PCT is at an intermediate level of specification, which is  
where you really want to be to do good, cogent ST. On the one hand, it's concerned  
with a single type of phenomenon/structure. On the other, it's concerned with that  
WHEREVER it exists.

>Sure, if you want to include organisms and  
>interactions among organisms in the same category, an economy is  
>a living system. If you don't, it isn't. What difference does it make?

That was part of the semantic argument which I've dropped [C].

>I dispute whether ST is about systems of ALL kinds,

Well, that's a matter of definition: ST just IS about systems of ALL kinds,  
however it is that you then go on to define "system".

>and whether it has deduced the properties of ALL systems NO MATTER HOW THEY  
>ARE HOOKED UP.

Nobody's claiming that. That would be like saying that physics has deduced the  
properties of all physical objects everywhere for all time. It's somewhat true,  
but silly.

>It is about a certain range of systems that fall  
>within the definitions of system with which ST begins.

How could it be otherwise? It's a tautology: ST is concerned with systems of all types. What's a system? A system is X. Then ST is concerned with X of all types. What are you really trying to say?

>Or put it this way: in general statements  
>about systems, how come I can so often think of counterexamples?

I'm not sure what you mean. Can you give an example?

>Conflict arises, however, when there is competition  
>to see whose idea anticipates whose idea.

As I've said, ST qua ST is not a competing theory to PCT. ST qua ST has NO theory of animal behavior, and makes NO predictions about animal behavior. Rather, I believe that PCT is in fact that PART OF ST which IS a good theory of animal behavior, and part of a good theory of life in general.

>A common strategy, in and out of science, is for people to go up a level of  
>abstraction,

While many ST people do proceed by abstraction, and there is value in that, ST proper actually goes in both directions. Not only can we notice isomorphies between systems of different types, and then try to come up with a common type-independent theory, but we can also start with general mathematical definitions, make them progressively more complex and specialized, and then make predictions about any real system which can be modeled as a system of that type. As I've said, feedback control systems are of intermediate complexity.

=====

>[From Rick Marken (940225.0900)]

You replied to NOTHING except my "bingo" comment, from which I've retreated [C].

Simply ignoring the argument is actually quite rude. If what I've said doesn't merit a reply, then say so.

So far, the only thing I (well, really ST as a field) have received from you in this thread is a string of insults.

=====

>[from Mary Powers 940224]

I'm not sure what to say, Mary. It is certainly not my place to argue the history with you. I'm just a kid, and you both were there. It just strikes me that denial of the intellectual heritage of PCT in cybernetics and systems theory is very sad and short sighted.

Sigh. Maybe I'd feel differently if I had suffered your slings and arrows, if I had been spurned for decades as you have, if I was a practitioner rather than a theoretician. I really hope that one day my community will give yours your full

due. Because it really is the whole ST movement, more than just psychology, that PCT so critically addresses. The problem of control is perhaps the original systems problem, and not only a PSYCHOLOGY problem. The REAL problem for PCT begins WELL BEFORE the evolution of neurons. EVERY organism is a control system, maintaining its internal metabolic state far from equilibrium.

I guess until that happens, y'all will have some justification in resisting my (apparently bizarre) claiming of PCT by ST. But when you come from my side, it's as plain as the nose on my face.

I wish I knew more about the history of science. I read a book review of the biography of one of the early English chemists and head of the Royal Society (I WISH I could remember his name, Butler?), a teacher of Maxwell. He discovered many of the elements, and was instrumental in early electrochemical and electromagnetic theory. Maxwell went on to surpass him, of course, and he's passed out of history. Butler (if that's who it was) was wrong about a lot of things, and severely limited by his time, but laid the foundation for all the others. I think about Ashby and Powers that way.

Or maybe it's like this: let's say that homeopathy, chiropractic, or perhaps acupuncture really is a new, highly scientific, revolutionary form of medicine, which is spurned by the traditional medicine community. Does it make sense for them to completely disavow medicine IN GENERAL, to claim that they're not a part of medicine AT ALL, even though medicine fights or ignores them?

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O----->
| Cliff Joslyn, Cybernetician at Large, 327 Spring St #2 Portland ME 04102 USA
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V All the world is biscuit shaped. . .
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Date: Sat Mar 05, 1994 9:20 pm PST  
Subject: Re: Computations of information in perceptopon

[From Bill Powers (940305.2000 MST)] Martin Taylor (940305.1700)

>>Whatever 2.2 units of information refers to, it does not refer  
>>to information about the waveforms of the perceptual signal or  
>>the disturbance.

>This is also true, but it depends on the statement "In  
>computing THIS matrix." It would not be true in computing  
>another matrix in which the temporal characteristics were  
>incorporated.

I insist that we finish this problem before we go leaping off into other computations that might be taken up. We can take them up in due time to see if your statements about them are true. The problem at hand is how much information about the disturbance there is in the perception, and what kind of information that is. We have settled, I think, that there is essentially no information about the disturbance as an amplitude function of time contained in the perception also given as an amplitude function of time. You said so a few days ago, as a reason for proposing that there should, however, be information in the first time derivative of the perception about the first time derivative of the disturbance (that's temporal information, isn't it? But perhaps it isn't the right temporal



information). It was on that basis that I skipped the analysis for the amplitude functions and went directly to the first time derivative version.

I am simply following your computations to the end (something that you seem reluctant to do), as nearly as I can understand them, by converting them to code and applying them to real experimental data. So far I see no evidence that the first derivative of the perceptual signal has any more predictive power concerning the first derivative of the disturbance than has the amplitude of the perceptual signal concerning the amplitude of the disturbance. I have obtained a numerical value for the information in the disturbance's derivative given the reduction of uncertainty due to the perceptual signal's derivative. I don't know if this is the correct number; you must check the code I sent you (I will send the latest version by direct post tonight) to see if I have succeeded in following your directions.

I believe that what I learned while doing the calculations shows that whatever this information means, is it not information about the time course of the disturbance. You seem to agree that this is true in the present case, but I would like to hear it said explicitly.

I would like to pursue this line to the end, until we agree on whether the first derivative of the perceptual signal contains any useful information about the first derivative of the disturbance, both as functions of time. If you can show that it does, then we are finished. If, however, you cannot, then we will have shown that neither the raw time function nor the first derivative of the perceptual signal contains any useful information about the time course of the disturbance or its first derivative. Perhaps we could then agree to extrapolate far enough to say that no higher derivative of the perceptual signal will contain information about the time-course of the same derivative of the disturbance (because the computational method discards all information as to when ANY value of ANY derivative occurred).

As to introducing a time lag, I have already done that by computing the perceptual delay needed to obtain the best prediction using the control theory model. I see no reason why that should not also prove to be the best delay for the present analysis. All the computations I am doing contain this optimal delay for the perceptual signal.

You propose

>[Bill] could, for example, have looked at the distribution of  
>disturbance values at time  $t+\tau$ , given that at time  $t$  it had  
>some value and the derivative had some value. From the three-  
>dimensional matrix that arises for each value of  $\tau$ , he could  
>compute, using the same analysis procedures, the information  
>about the earlier state of the disturbance that remains in the  
>later state. From that, he could determine not a specific  
>waveform, but the sorts of waveform that the disturbance  
>followed, as well as information rates, which are where the  
>action is when we are dealing with control systems.

Please note that "Bill" did not propose this way of doing the analysis; you did. When you say "He could have looked..." you imply that I proposed this computation, and could have chosen some other. The truth is that you proposed it, and if you now want to propose a different one, that is your responsibility, not mine. I'm

only following orders. You're the one who is looking for a method of computation that will prove your point.

Tell me how to do this (when we agree we are finished with the subject of time derivatives) and I will do it. I predict that the same result will occur: we will obtain a measure of values at times  $t$  and  $t+\tau$  by a method that discards the information as to when during the run these measures occurred. Even if you try to employ temporal information, you will lose the time-dependent information as to how that temporal information applies to the data. That is inherent in the computation of probabilities, because all that is preserved in the probability-number is the total number of times that the stated condition (whatever it is) occurred during the run -- and not when any one instance of it occurred, or in what sequence any two or more instances occurred. All information about sequence is lost -- even if the measures are measures of pairwise sequences.

>Likewise, instead of using the disturbance at  $t+\tau$ , he could  
>use the perceptual signal at  $t+\tau$ , and take as conditionals  
>values for the disturbance at  $t$  and the perceptual signal at  $t-$   
>lag, or any other group of values. All these things will give  
>you numbers.

Oh, I strongly agree with that. Any arbitrary computation applied to the data will come up with numbers. The ability to compute a number, however, is no indication that the computation has any meaning. You have claimed that SOME measure of information in the perceptual signal will provide enough information to permit an essentially perfect reconstruction of the disturbance. But the only way I know of now to reconstruct the disturbance is through considering the other variables and signals in the control system as functions of time, and the functions as dynamic functions relating the signals. That can be done without computing any probabilities, and from what I have learned so far it seems that any probability calculation would discard the very information required to analyze the system of variables and functions. I am still willing to see a demonstration to the contrary, but to this point it has not appeared.

Best, Bill P.

Date: Sun Mar 06, 1994 8:05 am PST  
Subject: Systems theory, tigers, etc. (from Mary)

[Mary Powers 940306] Cliff Joslyn:

I thought your post was terrific. I really have no problem with your stance as a systems theorist - that's what you are, and that's what you do, and your interest in and support of PCT is great. I do wonder, however, whether your colleagues aren't going to view you as some kind of aberrant freak for being interested in PCT - just as Tom's former psych. department colleagues viewed him. You may well be the only ST person in the world who sees any value in PCT, and I hope it doesn't get too lonely, being interested in old-fashioned, ugly first-order cybernetics.;-)

I didn't care for your medicine analogy, vis-a-vis PCT's intellectual heritage. It's more like this: what did 19th century medical researchers like Pasteur and Koch and Lister owe to an intellectual heritage that was convinced that disease resulted from an imbalance in the four humors?

I think Ashby, Wiener, etc. had hold of a really interesting fuzzy rope - but never moved up it far enough to see that it was attached to the rear end of a tiger.

PCTers are like Calvin - they are having a wonderful time with this big tiger, which the rest of the world thinks is a stuffed toy.

Mary P.

Date: Sun Mar 06, 1994 12:39 pm PST  
Subject: So many posts, so little time

[From Rick Marken (940306.1230)]

Boy, when it rains it pours on ol' CSG-L. I hope I can cover most of the very interesting threads that have developed.

CHUCK TUCKER (940304.12:04) --

>Don't believe a word that Rick says about himself; he is a very  
>poor judge of character especially his own.

Thanks Chuck; hearing that I'm wrong never sounded so nice! If I can get through this avalanch of mail I will finish up your HyperCard experiment this weekend.

Martin Taylor (940205 15:00) says --

>I noted that I had forgotten to point out that the measure he asked for would  
>naturally give a value of zero in a well-functioning control system, and  
>pointed out that the derivatives of the perceptual and disturbance signal  
>were more interesting items for which to take the uncertainty and information  
>measures.

This is just a tad disingenuous. What Martin Taylor (940301 12:00) actually pointed out was the following:

>Over any reasonable period of time, the VALUE of the disturbance is  
>more or less independent of the VALUE of the perceptual signal, as we  
>all know. But the derivatives are not independent, at least not for  
>a correct value of the time delay associated with the perceptual input function.

This does not say that "the derivatives of the perceptual and disturbance signal were MORE INTERESTING items for which to take the uncertainty and information measures". It says that these derivatives are NOT INDEPENDENT. Martin made a specific prediction about observable data (it's was actually an assertion, but I'll be tolerant) -- an exciting event indeed.

I thought that there was NO WAY that Martin could deny the fact that the derivatives of disturbance and perception (at ANY delay) were INDEPENDENT once he was confronted with the evidence. But I considerably underestimated Martin's skill at controlling his own perception of his "rightness". Martin managed to deftly change his prediction from "derivatives are not independent" to "the relationship between derivatives is interesting".

But he didn't stop there. Watch this folks! Martin says:

>I was taking my few available moments to develop a posting to indicate the  
>procedure and expectations for what to measure and what to expect of the  
>measures in a control system, but before I got half-way through, Bill P.  
>produced a scattergram of disturbance derivative versus perceptual signal  
>derivative, which looked pretty much as expected (i.e. a big blob).

Look at that last phrase folks!! After claiming that "THE DERIVATIVES ARE NOT INDEPENDENT" Martin says that the observed relationship between derivatives is "PRETTY MUCH AS EXPECTED". But the relationship between derivatives was "a big blob", meaning that the derivatives are essentially INDEPENDENT. So Martin is saying that the actual result, showing that the derivatives are independent, is consistent with his claim that the derivatives are NOT independent. This is an amazing piece of rhetoric but Martin goes even futher and turns it into CHUTZPAH by getting mad at ME for pointing out that the data did not fit his prediction!! Martin says:

>Rick then weighed in with a largely predictable diatribe about the new proof  
>that information was irrelevant to control theory.

In my "diatribe" [Rick Marken (940302.1300)] I replotted Bill Powers' "big blob" and said:

>So, in order to convince us of the usefulness of information theory,  
>Martin said that something is a fact (the derivatives are dependent)  
>which is not a fact (the derivatives are independent).

Martin, I salute you! I love it. Implying that I am a raving lunatic because I pointed out that the data is not what you predicted. Beautiful! My hat is off to you sir!

It should now be clear to anyone capable of reading past his incomparable rhetoric that Martin has no intention of revising his beliefs based on evidence. I mention this for the sake of those like Dan Miller (940305) who want to "see living control systems as they work in more worldly situations". Martin is controlling in a very worldly situation (CSG-L). He is controlling for a certain level of "being right" and he is doing whatever is necessary to maintain that perception -- and he's doing a darn good job of it.

Martin, you and Bill P. can carry on without me. Maybe Bill can deal with your constantly varying control actions better than I can. But maybe not, viz: Bill Powers (940305.2000 MST) to Martin Taylor --

>I insist that we finish this problem before we go leaping off  
>into other computations that might be taken up.

-----

Re: Scientific facts

I said:

>Scientific facts are NOT controlled perceptions; the swirl of points  
>that Bill Powers posted were not brought to that state by Bill's actions.

Dan Miller (940305.1200) replies:

>Then how did they get there?

Bill P. answered this exactly as I would. But I wanted to try to add something that might handle some of Kent's points too. In particular, I want to say that, because the "swirl", rather than any other shape, resulted from the purposive actions that set up Bill's experiment, wrote the program, etc. the swirl is "informative". Many possible perceptions (besides "swirl") COULD HAVE resulted from the experiment; the "swirl" INFORMS the scientist (if s/he is NOT controlling for a particular result -- note how uninformative the result was for Martin) that the derivatives of perception and disturbance are independent.

Facts are uncontrolled perceptions that are informative when we look at them from a scientific perspective; that is, to see what state of the perception results under certain conditions. If the perception is under control, then it is NOT informative; it tells nothing about which of many possible states of the perception might have occurred, because a PARTICULAR state was MADE to occur. There is no information in the fact that the result of an experiment is a "swirl" of points if you contrive to make that result occur (do what is necessary to to MAKE it occur -- ie. control it).

>These relationships, groups, and organizations  
>are composed of individual organisms who control perceptions, but  
>I suspect that we are going to need more than this to adequately  
>understand these phenomena.

Why do you suspect this? What more do you think is needed to understand these phenomena?

>An imaginary experiment (NSF will not fund this): Let's take an  
>infant at birth, keep it alive in social isolation (tubes, wires,...)  
>for twenty years. No human contact for twenty years. Will this  
>organism control perceptions? Will it act purposively?

Yes and yes. We already know this because we know that all organisms, from bacteria on "up", control perceptions. To live is to control perceptions. Even embryos control perceptions (oops, that's makes me "pro life" I guess).

>At what level?

That's the interesting question. Seems, though, like it might be best to have a REAL GOOD handle on what perceptions are normally controlled, whether they are organized in levels, and stuff like that, before running your proposed experiment.

Kent McClelland (940305) --

> it seemed to me that most of the comments on my post had little to do with  
> the substance of my suggestion that the colloquial understanding of  
> information can be given a meaningful definition within the PCT worldview.

I think we had already agreed that perceptions can be "informative", in the usual colloquial meaning of that term, if they are NOT under control. This is why

disturbances can be informative to control systems that are NOT controlling the perceptions that are influenced by these disturbances.

>Bill has called a "quasi-static" mode, that is, using a constant reference signal.

"Quasi-static" does NOT mean "fixed reference signal"; it refers to the algebraic descriptions of the behavior of a control system --  $p = r$  and  $o = r - kd$  -- which assume stability and leave out dynamics (differential equations).

>But I doubt that they really mean to imply that living control systems are >hermetically sealed off from getting any information (in the colloquial sense >from their environments, which is one way you could take that argument.

Right. I propose (and Bill proposed it long ago) that "informative" refers to the state of uncontrolled perceptions -- when we are looking at those perceptions "scientifically" (in "passive observation mode") knowing that several possible perceptions might result under certain circumstances.

> I guess I'd like to see more discussion on the net of when control systems >change their reference values and why.

The argument (and evidence) against there being any information (colloquial and formal sense) about the disturbance in perception does NOT depend on the assumption of a fixed reference; it is true of control systems in general, under all circumstances (except when they are not controlling -- ie. they are open loop and, therefore, not control systems).

Bill Leach (940304.08:01 EST(EDT))--

>Through experience, I know for example, that the Meyers/Briggs "Type >Classifications" definitely have some validity.

Yes. PCT would "support" your experience of people fitting the Meyers/Briggs "Type Classifications". It's a category level perception -- in YOU. You can perceive some people as being of one "type" and others being of another. That is a real perception. But there is no such thing as a "type" in the people themselves; just as there is no such thing as "intelligence" inside a person -- although your own perception of the person's "intelligence" is as real as it can be.

PCT rejects "reification" -- taking a perception that the observer can have as evidence of something "inside" the subject that causes what is perceived by the observer (this is also called a "dormative principle"). If your perception of categories of people corresponds to the Meyers/Briggs categories than that is a FACT -- and PCT accepts it as such. But PCT shows that it is a mistake (and a possibly very harmful mistake) to assume, then, that there is something in people that corresponds to these categories.

Cliff Joslyn (940305, all day long!) --

>I'm still a bit amazed how defensive "you people" are.

We are defending the integrity of the PCT "message".

>Remember, I didn't ask for this argument,

Neither did I? I just typed some words which turned out to be a major disturbance to some system concepts. C'est la vie.

>I was merely defending MY profession against a scurrilous, ignorant, and  
>(because he claims ST doesn't even have anything to do with PCT) completely  
> gratuitous attack (Rick has yet to admit any of this, by the way).

I admit that I made a scurrilous, ignorant, and gratuitous attack on ST. I just can't admit that I did it intentionally. But if you perceived it as scurrilous, ignorant, and gratuitous then it was (for you).

> Rick, you REALLY piss me off. Your style is very annoying to me.

You're not the only one who finds it annoying, don't worry. But, again, I'm not trying to be annoying (it just comes naturally ;->). I didn't say "I'm an asshole" for nothing.

I agree with Mary Powers (940306) about the rest of your post. Your heart's in the right place, Cliff. Keep up the good work. Don't mind me. I'm just seeing to the integrity of the PCT ideas. When I get these ideas wrong, Bill Powers will chime in and correct me. Thus far, it's been a LONG spell between chimes -- though I'm sure he agrees with your evaluation of my style.

Best to all

Rick

Date: Sun Mar 06, 1994 4:35 pm PST  
Subject: Re: Systems Theory and PCT (long)

<[Bill Leach 940306.10:43 EST(EDT)] >[Cliff Joslyn (940305, all day long!)]

Originally I was going to send this private. I will add that in my opinion, for whatever it is worth, Cliff Joslyn appears to be exactly the sort of ST person that I am NOT talking about in this posting. Indeed if he were typical of ST then PCT people would be foolish NOT to associate closely with ST.

Cliff;

As a "newbe" here I don't really want to get into the middle of the ST-PCT debate. Also, I clearly do not have the "credentials" to provide support on either side. OTOH, my "outsiders'" view may be "untainted" by personal involvement on either side. I definitely do have a bias however. I have always considered the "soft sciences" to be "less" than science but recognize that some individuals within those fields are often true to scientific principles. A "scientist" that is unwilling to submit his work to "acid test" of reality is no scientist.

My own impression of ST is that it generally has less substance than even "the laying on of hands". Naturally this opinion is based largely upon the media popularization of Climate Models, Ozone Models, Population Models and the like.

PCT is to me, a breath of fresh air. Imagine! People that actually believe that a model's first and primary responsibility faithfulness to reality! ...dealing with

people that believe that when they "learn" something new (and in particular, something unexpected), it is necessary to verify this phenomenon with objective reality rather than with the theory for the model! >>What a novel concept?!<< Why, you would almost think that some of these folks had been physicists or engineers or something similar where when theory and predication fail to match measurable reality you have to rework your theory. :-)

What I see here is evidence of real science being conducted. Evidence of people dedicated more to the principles of how to search for truth than to any particular results.

If Rick's knowledge of ST as a field of research is as weak as mine (and I think that it probably is) then I can readily understand his vehement objections to relating PCT to ST. I would probably be considerably more violent than he has been.

From a "laymans'" perspective, ST IS the "Ozone Hole Model" (or at least such models are the premier representatives of the field). When one views actions of the spokesmen for such "research" and sees such things as the fraudulent (and I think criminally negligent) NASA "Warning" of a couple of years ago (based upon the "Ozone model"), it is quite easy to see why someone interested in science might want to distance themselves. I personally am not sure that I would even want to admit to being of the same biological species as the people of such a field.

Generating apocalyptic scenerios (and therefore revenue) using modeling techniques that ARE KNOWN to be inaccurate and incomplete is disgusting. That the associated "scientific community" fails to have the moral fortitude to ostracize the perpetrators places the entire field outside that set of activities that can be called science.

I say that in full recognition that there are undoubtedly dedicated ST people that have not foresaken truth and objectivity but I strongly caution them to be very careful and to look closely at the organization that funds their research. It appears that the number of scientists that have been willing to sacrifice their "position" in support of truth when a conflict existed is disturbingly small.

I could easily go on with my "tirade" against ST by adding my assertion that "they" have not only "sold out science" but humanity itself. The magnitude of the crimes by the Systems Theorists (though not exclusively only them of course) will probably never be known. I am quite certain that little effort is required to show that not only billions of dollars of real wealth has been lost but millions of lives have been and will continue to be lost as a direct result of "charlatan science". We really do not even have a scale from measuring an intangible such as suffering that results from such actions.

If you also count the effects of creating a populous that is now suspicious of science and in general is actually beginning to BELIEVE THAT SCIENCE IS LITTLE MORE THAN A MATTER OF OPINION, then the damage truly is incalculable. That many of the Systems Theorists actually believe that "the cause of 'saving the world'" is more important than truth is only an excuse for them as individuals. As far as ANY and ALL other scientists are concerned such behaviour removes ALL credibility that person may have had IN SCIENCE.

A hundred years or more from now I can just see the science historians writing about how real science was almost destroyed because of people with virtually no



ethics and morality that became the leading figures of their respective fields by being "Politically Correct" rather than honest.

It nearly impossible for me to express the magnitude of rage that I feel about such behaviour.

Me... For me, I'll take a Rick Marken anyday. He may not be as "gentle" as Bill but he has convinced me that he is dedicated to objective science (a little redundancy there, but unfortunately necessary). [and I am not trying to claim that I believe that you think of him otherwise]

After having said all of this though, it should be easy to conclude that I believe:

1. That PCT is a real science.
2. In general ST is NOT and should be.
3. But when ST is what it should be then it is not unreasonable to conclude that PCT is a "branch" (or subset) of ST.

Unfortunately, I far as I can tell, ST has a hell of a long way to go in removing the "snake oil salesman" stigma. Until ST can become "respectable" with rational people instead of "popular" with politician, PCT would be well advised to maintain its distance.

-bill

Date: Sun Mar 06, 1994 10:23 pm PST  
Subject: Oh Well!!

<[Bill Leach 940307.00:21 EST(EDT)] >NET (Particularly Rick)

Well I am to page 115 in my first reading of BCP and I can see that I will owe a lot of appologies to several people though I suspect that it will take me many more months before I even start to recognize how many many and why.

>Bill P.

I have finally engaged you in your "conversation" on control systems. It is, so far, a pleasant discourse. Your friendly discussion of the issues in understanding human behaviour and relating them to both observed conduct and apparent physical reality within the human anatomy makes "comfortable" reading. My nuclear science and electrical engineering backgrounds are quite at ease with your approach.

To this point I am greatly please at both your recognitions of the complexity of your subject and the difficulties associated with "simplification." I am, so far, very impressed with your handling of both.

>ANYONE

Unless someone seriously objects, I would like to delay further my first posting of the CSG-Dictionary. It is obvious to me that what I am learning from BCP significantly impacts any impressions that I have formed so far about the definition of terms.

I believe that maybe Dag (I have not tried to review previous posting at this point) was anxious to see something happen in this area. If a delay is intolerable, please speak up and I will ignore any concern about my present state of understanding and post for comment.

-bill

Date: Sun Mar 06, 1994 10:59 pm PST  
Subject: Re: So many posts, so little time

<[Bill Leach 940306.19:03 EST(EDT)] >[Rick Marken (940306.1230)]

Speaking of perceptions... I KNOW I should stay out of this and maybe go lay down on the couch and see if I can figure out what perceptions I am trying to control.

Rick, you are I believe, "even tempered". Someone makes the wrong statements about PCT and "off you go", they make personal attacks and you "do them one better." With style no less :-)

I almost KNOW that I am about to "step in it again" but...

I sort of accept that a "control loop" can not "know" anything about the disturbance. I'm not so sure that I fully accept the idea that the entire control system can not have knowledge about the disturbance. However, as I was thinking about this I realized that there is so much that I don't understand about the contextual meaning of terms that I read.

Probably the worst terms are "know" and "information". As I see it, the term "know" is so high level a term as to be about useless for most PCT discussions (at least as far as actual testable experiments).

As far as "information" is concerned; It seems to me that as I stated above, the "control loop" that is directly involved in maintaining a particular perception does not perceive anything about the the disturbance except that control is either is or is not established.

It also seems to me, that there is a "mode" of human thinking where a person is aware of "observing" their conduct of some action. And though it probably does not matter, I am specifically talking about the case where the action itself is not an "automatic" one but actually requires active cognitive effort. In such a mode it seems to me that IF one is considering the entire person as the subject control system then it is not incorrect to refer to "knowing" information in the disturbance.

I seriously doubt that there is any usefulness in such a view however (at least not in the current stage of modeling). I also don't know if this is the sort of "information" that is being considered (I suspect not).

Dan says:

>>These relationships, groups, and organizations  
>>are composed of individual organisms who control perceptions, but  
>>I suspect that we are going to need more than this to adequately  
>>understand these phenomena.

Rick say:

>Why do you suspect this? What more do you think is needed to understand  
>these phenomena?

Knowing everything there is to know about a single control system is not enough to understand how it will function in the real world unless you also know something about that real world. Much of the "studies" of human behaviour ARE useful to PCT.

Most previous work may often be close to useless (or maybe worse) as far as understanding HOW people function or WHY they might choose to act in a certain fashion. However, there is a huge collection of data about HOW people DID act. Obviously much of this is of no use to PCT and in particular individual studies are probably mostly of no value. The aggregate collection of information about human behaviour OTOH will likely prove quite useful when properly analyzed.

Rick:

>This is why disturbances can be informative to control systems that are  
>NOT controlling the perceptions that are influenced by these disturbances.

I probably need to be hit with your "baseball bat" again... but I as a person doing something certainly CAN be aware of the disturbance as in:

I am turning a winch to draw a boat out of the water to dock it into its' trailer. I am fully aware of any change in resistance to my turning the crank even if the crank continues to rotate at the desired rate (or the boat moves at the desired rate). Now I don't think that I have any trouble with the idea that it is "some sort" of monitoring function that actually detects this resistance and not the "control loop" that is actually causing my body to turn the crank.

Now adequately explaining how the above does not relate to the "disturbance perceptions issue" will certainly clear up a great deal for me.

>Meyers/Briggs

I think that I understand this... maybe. What I hear you saying is that there is nothing biologically, intrinsically within anyone that makes someone a "Thinking" or a "Feeling." And that is fine, as even those I have read that write on the subject are quick to admit that even people that seem to exhibit "very strong types" are in no way incapable of operating in the opposite "type."

Whether you or anyone else likes it, there are people that are generally all but unfazed with a rational, logical argument just like there are others that will just about totally ignore an "emotional plea." Now, unless you are the product of Dan's unauthorized experiment you have to have encountered such diverse people.

As I see it, "Type Talk" deals with useful "generalizations" about how different people "usually" respond in interactions with others. Maybe I missed the point of the authors but I don't see what they have to say as being a statement as WHY humans behave in these somewhat different ways OR indeed why they behave at all!

As in the case of most any "determinations" of human behaviour or characteristics, PCT could only improve the understanding what is observed and what actually may be going on.

-bill

Date: Mon Mar 07, 1994 9:01 am PST  
Subject: Re: So many posts, so little time

[Dan Miller 9940307]] Rick Marken:

There is so little time that I hope this thread grinds to a close. In our discussion of facts and observations Rick responded:

>Facts are uncontrolled perceptions that are informative when  
>we look at them from a scientific perspective; that is, to see  
>what state of the perception results under certain conditions.

Do I understand this correctly? Conditions cause perceptions? If so, then my assumptions about perceptual psychologists are correct; push them just so and the simple-minded positivist is revealed.

Previously, I had noted:

>>These relationships, groups, and organizations  
>>\_are\_ composed of individual organisms who control perceptions, but  
>>I suspect that we are going to need more than this to adequately  
>>understand these phenomena.

Rick's reply:

>Why do you suspect this?

In my imaginary experiment I proposed that we keep a newborn infant alive for twenty years with no human contact (even supposing this is possible).

>>Will this organism control perceptions? Will it act purposively?

Rick: >Yes and Yes.

Really? Again, at what level? Sensations? Intensities? How will this human control system control perceptions? How will it act purposively? Will it sit up? grasp objects? focus on particular objects? speak English? look for food? play games? My guess would be no to all. We would have a twenty year old newborn infant who could not survive because it could not control.

So, why do we need to know about social relationships, groups, and organizations along with the interaction processes that are central to them? Because we need to do so in order to understand people like you (and me). We can't be independent and autonomous human living control systems without them.

This is my way to say that we need to get on with the project. I understand that these are not your problems. The world is far too untidy for "guardians of purity":-)

Later, Dan Miller MILLERD@UDAVXB.OCA.UDAYTON.EDU

Date: Mon Mar 07, 1994 10:47 am PST  
Subject: Re: Facts; Information in perception

[Dan Miller (940307)] Bill Powers:

This discussion of "facts" is growing tedious. However, I must create one last disturbance.

We are in agreement that facts are perceptions that can be reproduced. However, they are social objects in that we use facts. Facts are constructed (as facts) for a purpose. We use them to win arguments, sway people to our way of seeing (and interpreting), convict people of crimes, and to gain control (reduce error).

I said:

>>Indeed, I have great difficulty separating the consequents from the act.

Your reply:

>Heck, that's easy. When I open a door, I do it by pushing on the  
>door. The opening of the door is the consequent; the act is  
>the exertion of a force. Entirely different things. I don't  
>understand what you mean?

The separation of cause and effect is arbitrary. It is more useful to see this as a process that begins with you imagining and desiring the door to be open. The process culminates with the perception of the door open. In this process there is applied force overcoming resistance, but I would hesitate to specify this as causal. Also, I would hesitate to suggest that your original imagined perception (reference signal) to be less than factual or less than causal if we must speak in this imagery.

Bill continues:

>Another example of imaginary facts is "mass hysteria" an  
>imagined feature of crowd behavior. I refer you to Clark McPhail  
>on this subject.

Why would I have to ask Clark about this? Does he know something about "mass hysteria" that I don't know?

Regarding Mr. Greenspan and his actions as Chairman of the Federal Reserve I noted that:

>>his facts have worked for him for quite some time.

You replied:

>No they haven't. They have been imagined to work. They are not  
>even facts, because there is nothing behind them that anyone else  
>can perceive.

Nonsense. Let's assume that Mr. Greenspan has not made his intentions perfectly clear. The political economists that I speak with tell me that his intentions are to maintain (if not increase) the concentration of wealth and power in the same people/organizations. Understood this way, I think his constructed facts make perfect sense. They work.

Regarding my contention that facts are constructed, and that the tracking experiments were designed, modified, and intended to generate "facts" supporting your contentions about PCT, Bill responded:

>There was nothing inherent in my actions determining that the  
>experimental points lay where they did. ...

Then noting the presence of a participant in the construction of his facts Bill notes:

>If that person had moved the  
>handle differently, the points would have been differently  
>distributed in the plot.

I disagree with the first contention, and wholeheartedly agree with the second. First, I must insist that you designed your experiments, the machinery, the computer program, and the instructions given to the research participants (which include you, Rick, Tom, and other close associates) solely for the purpose of generating points that lay in the plots you describe. Furthermore, you tell "naive" participants what to do, what to look for, and how to do it. Second, if they do act differently (as rebellious subjects), then the plots would look differently without doubt a true statement.

Later, Dan Miller MILLERD@UDAVXB.OCA.UDAYTON.EDU

Date: Mon Mar 07, 1994 11:25 am PST  
Subject: Re: Computations of information in perceptopon

<Martin Taylor 940307 12:00> >Bill Powers (940305.2000)

I have a few tens of backed-up messages that I have not had time to read, but I do read ones with a POWERS\_W signature, since we are collaborating on a study and the message might be related to something that needs dealing with.

I have responded to Bill on the details of this one, but the essence of my response is:

>I insist that we finish this problem before we go leaping off  
>into other computations that might be taken up.

I thought that the numbers you produced more or less finished this problem, and so proposed directions that should be pursued. In a control system, the interesting constructs relate to information RATES, rather than to amounts, though amounts will show up as related to quantities like transport lag and output gain.

Do not expect much from me on this until the sleep study is over. It and many other (paid) items of work are keeping me exhausted, so much so that I actually took this Sunday off from work and didn't come in at all. I slept most of the afternoon, and am glad I did.

It may be that I will deal with other CSG-L postings briefly as I come to them over time, but careful responses are unlikely for a while.

Martin

Date: Mon Mar 07, 1994 11:26 am PST  
Subject: Knowing

[From Bill Powers (940307.0820 MST)] Bill Leach (940307.0021 EST)

Glad you've broken the ice with BCP. When you've finished it, you'll find two chapters that my editor cut out of it in LCS II: the one on emotion and the one on the method of levels.

Bill Leach (940306.1903 EST) --

>I sort of accept that a "control loop" can not "know" anything  
>about the disturbance. I'm not so sure that I fully accept the  
>idea that the entire control system can not have knowledge  
>about the disturbance.

RE "knowing" (anything):

This is a difficult subject because it straddles two worlds: the world of objective modeling in which we try to "reverse-engineer" the nervous system, and the world of direct experience, where we experience the operation of the same system from the viewpoint of an occupant of the brain.

In the PCT diagrams of control systems that you see in BCP and elsewhere, you will never find anything labelled "awareness" or "consciousness." From the modeling standpoint, all that is required for us to say that a perception exists is that a perceptual signal be present in the appropriate pathway. Perception of a cubical shape requires that signals representing the sensory attributes of the cube (sensations) enter an input function capable of generating a perceptual signal whose magnitude indicates the degree to which a cube is perceived to be present. There is no requirement that the person as a whole have any consciousness of experiencing a cube.

The reason for this odd-sounding concept is that if control depended on conscious awareness of perceptual signals, then only those control systems containing perceptual signals of which we are consciously aware could work. Conversely, we would have to be conscious of all perceptual signals in all control systems that are actually working at a give time. Neither premise fits experience. When I am typing this stuff, I am aware of the words I choose appearing on the screen and of a sort of scrabbling of fingers over the keyboard, but I am not aware of the joint angle control systems, the velocity control systems, or the force control systems that are converting my desire that a given letter or word appear on the screen into the specific reference signals being sent to the lower-level systems, and the resulting states of the controlled perceptual signals. Yet if I wanted to, and shifted my focus of awareness, I could be conscious of at least a great part of those lower-level perceptual signals. I assume that's true of everyone.

It's the hierarchical structure that causes the biggest problem. I can be aware of control processes at a certain level, but the scope of awareness, mine at any rate, is limited. If I'm "concentrating" on how to get a program to print something out into a text file, my consciousness is almost totally preoccupied with the logical and procedural goals and the difference between what the program IS doing as opposed to what it SHOULD BE doing (the German word for reference level is SOLLWERT -- should-be). But while I'm thus preoccupied, the shifting higher-level errors are being continually translated into more specific lower-level goals, and so down the hierarchy all the way to the systems that are doing the typing for me, keeping me from falling out of the chair, and so forth. Clearly, in order to control the perceptions I am conscious of, there must be countless other perceptions at lower levels that are also being controlled. In fact, even for the higher levels of perception in consciousness to exist, the

lower-level perceptions of which they are functions must also exist. But I'm not simultaneously aware of those lower-level perceptions, for the most part.

This problem exists in the other direction, too. While I am working out the problem of printing the data to a file, I know that my goal is to get the data printed to the file, but WHY I am trying to reach that goal is not in direct awareness. When I finally solve such a problem, I sometimes have a moment of disorientation: now just why was it that I wanted to print that data to a file? Then there's a sort of "Oh, yes!" feeling as I return my awareness to the higher-level process in which solving this particular problem was only a means to an end.

If the higher-level control system hadn't been working, there would have been nothing to supply the goal to the system where my awareness was. So clearly, the higher-level control system can go right on working when my awareness of it is absent. This means it must have had an intact input function, perceptual signal, error signal, and output function, all humming quietly away, working totally automatically. Again, perceptual signals exist and MUST exist without being in awareness. Only now we are talking about perceptual signals at levels above the level of awareness.

So when we speak of what a control system "knows", we have to keep the question of consciousness separate. The knowledge in a control system consists entirely of its perceptual signal. We must also, however, remember that this is a multi-ordinal model with perhaps 11 levels in it and many systems at each level. The knowledge contained in one perceptual signal at one level is put together with knowledge in other control systems at the same level to create knowledge at a higher level in the form of higher-level perceptions. From the same body of lower-level perceptions there may be many different ways of extracting knowledge at a higher level, in many different control systems operating in parallel. A cube-signal, put together with other configuration signals, can lead to signals representing rate of spin, bouncing off other objects, spatial relationships with other objects, symbols representing the cube and its relationships, temporal functions, logical functions, principles, and system concepts -- all at the same time although at different levels.

With awareness out of the picture, we have a system that contains signals at many levels representing various aspects of a world at various levels of abstraction. It carries out all functions of a living human system, including thoughts, feelings, actions, goal-seeking, whatever. It's just a big analogue-digital computer, with no more awareness of its own internal processes than my 80486 has.

When you put awareness into this system, what you get (according to my hypothesis) is the effect of connecting a bunch of perceptual signals to some sort of receiver. This receiver needs no cognitive functions, no computing capacities, no capability for action: all it does is receive. When it does receive, we get a conscious world composed of some subset of all the perceptual signals in the hierarchy. The whole hierarchy continues to function as usual; the only difference is that we become aware of some part of its functioning. Then we feel that we are consciously doing the things that the hierarchy would be doing anyway. When control systems in the hierarchy experience an error and produce actions, we sense the error-based output, through the imagination connection, as what we are doing to achieve the goal -- we the conscious observers, not just the automatic machinery. But it is the learned control systems that are actually doing the doing, the thinking, the cognizing -- even when the doing is something as



intellectual as making a choice or a decision, or formulating a plan, or making a judgment. The observing system makes no judgments. It is simply aware.

The other side of awareness is volition: producing a change in a reference signal in the hierarchy for a reason unconnected to anything that is going on in the hierarchy. And I can make a case that awareness and volition are associated (vaguely) with the reorganizing system, so that awareness can serve to focus the process of reorganization. You'll be getting to that in BCP eventually.

All of this is a rather extreme position, saying that awareness or consciousness carries out NO functions in the hierarchy; that the hierarchy does every last thing that we call either physical or mental, and without awareness being required. Being stated so firmly, this hypothesis can easily be disproven, by finding effects on control processes that result from shifting awareness. I am quite sure that such effects can be found -- but until they ARE found, and experimentally verified, the hypothesis as it is now stands.

Out of this hypothesis, the method of levels grew. This is a method of psychotherapy in which a person is encouraged and helped to move the locus of awareness up one level at a time, each shift bringing the focus of reorganization to bear on a new level of organization, and presenting to consciousness a world organized in a new way. When carried as far as possible, as Kirk Sattley and I did experimentally about 40 years ago, this procedure leaves a person in a state of what seems to be pure awareness, with many of the operations of the hierarchy being laid out to view but with no identification of awareness with them, no participation in them. One is then what David Goldstein and I have come to call the Observer. It was through working this method with a particular patient that David was able to help a woman with multiple personalities to begin reintegrating. The woman came to understand completely what was meant by "the Observer," and from that time on, any personality could be reached through the Observer.

For all I know, the Observer is another level in the hierarchy that I haven't been able to identify. Maybe there are levels beyond that (one mystical and somewhat nutty friend once said, "Oh, there are THOUSANDS of levels!"). That doesn't much concern me. Working out this whole scheme at the levels we sort of understand is enough of a project for one lifetime.

Knowledge and awared experience are different things. Knowledge is just one perception as a function of other perceptions. It covers the whole range, from sensations to system concepts, and it requires no awareness for it to exist. It's just how we become organized to perceive and act on the world. Or so it sez as of March, 1994.

Best, Bill P.

Date: Mon Mar 07, 1994 12:37 pm PST  
Subject: Scientific facts, social phenomena

[From Rick Marken (940307.1000)]

Me:

>Facts are uncontrolled perceptions that are informative when  
>we look at them from a scientific perspective; that is, to see  
>what state of the perception results under certain conditions.

Dan Miller (940307) --

>Do I understand this correctly? Conditions cause perceptions?

No. Conditions are themselves perceptions. Here is a concrete example that comes from Piaget. He established the following conditions: two cylinders filled to the same level with water; two cylindrical metal plugs, one made of lead, the other of tin, both attached to strings. All of these perceptions were made to be as they are by control processes; someone made the cylinders and plugs, filled the cylinders with water so that the levels were equal and attached strings to the plugs. A child (about 12 years old, I should think) is handed both plugs and asked what will happen to the level of the water in each cylinder if one plug is placed in one cylinder and the other plug is placed in the other. The child is asked to make a prediction about a perception. The child can perceive (presumably) the difference in the weight (mass) of the plugs and can perceive that the plugs are the same size. Most kids predict that the water level in the cylinder that receives the lead (heavy) plug will rise higher than the water level in the cylinder that receives the tin one. That is one POSSIBLE perception. The child is then asked to put the plugs into the cylinders; this is also a control process; the child is controlling perceptions of the location of the plugs, the rate at which they descend into the cylinders (so that the water doesn't splash out) etc. But the child does not (or should not -- they never do, anyway) control the level of the water in the cylinders; the resulting perception (given the states of all the other perceptions -- the initial water levels in the cylinders, the size and weight of the plugs, etc -- ie. the perceptions that are the "conditions" of the experiment) is not controlled; it just happens.

Most kids (and many adults) find the perception that results from going through this procedure to be a BIG surprise: The water levels remain EQUAL after the plugs are placed in the cylinders. The results of this experiment are informative -- there is information in this perception because there were other POSSIBLE perceptions -- one of which (higher water level for lead plug) is expected by most people. This perceptual result is informative because it is NOT controlled; the child did not do anything to make the perception of relative water level be what was expected -- and it is usually not what was expected.

If the child was able to control the perception of the relative level of the water in the cylinders (by varying the relative size of water inflow and outflow valves, perhaps) then there would be no information about the effect of the disturbances (the plugs) on the perception of water level. The water level in one cylinder could have been kept higher than that in the other cylinder, regardless of which plugs were put into each cylinder.

>If so, then my assumptions about perceptual psychologists are correct;  
>push them just so and the simple-minded positivist is revealed.

Simple-minded, maybe, but this particular perceptual psychologist feels more like a constructivist than a positivist (as I understand these terms).

>So, why do we need to know about social relationships, groups, and  
>organizations along with the interaction processes that are central  
>to them? Because we need to do so in order to understand people like  
>you (and me).

You are treating phenomena (social relationships, groups, and organizations) as though they were explanations of something else (people like you and me). I find this odd. Yes, there are social relationships, groups and organizations -- they are important and interesting phenomena. But I would look for an explanation of these phenomena in the nature of the entities (people) that produce them. That's the PCT approach. That approach does not denigrate the importance of social phenomena nor does it take away sociology jobs. Sociologists study the unique, fascinating and humanly important phenomena that emerge out of the interaction of autonomous control systems.

>We can't be independent and autonomous human living control systems  
>without them.

This is a puzzling claim: wyou say that we can't be independent and autonomous human living control systems without social relationships, groups, and organizations. It seems just the other way around to me: we can't have social relationships, groups, and organizations unless we are independent and autonomous human living control systems. Bill Powers and Tom Bourbon have shown how various types of group phenomena can emerge out of the behavior of independent and autonomous control systems. I would be more inclined to believe that social relationships, groups, and organizations are the basis of human autonomy and purposiveness (control) if you could give a demonstration (like those given by Bill P. and Tom B.) of how this works.

>This is my way to say that we need to get on with the project.

Tell me how to get on with the project of understanding human autonomy and control by studying social relationships, groups, and organizations and I'll certainly lend a hand.

>I understand that these are not your problems.

They are not my problems because I have no idea what they mean. Again, how in the world do I go about understanding human autonomy and control by studying social relationships, groups, and organizations?

>The world is far too untidy for "guardians of purity":-)

Be as messy as you like. But remember, a mind is a terrible thing to lose. ;->

Best Rick

Date: Mon Mar 07, 1994 5:39 pm PST  
Subject: Re: Knowing

<[Bill Leach 940307.18:45 EST(EDT)] >[Bill Powers (940307.0820 MST)]

Actually, I skimmed the preface the day the book arrived. After reading just that much I recognized that I was going to need, pencil, highlighters and plenty of uninterrupted time.

This worries me (and don't let Rick hear this) but I think that after only just starting the book, I am beginning to understand him! :-)

The neural network discussions in the book actually answered the concerns that I have had about "knowing" about the disturbance. The idea that level-I sensor signals associated with level-I outputs propagate to level-II control loops is plenty sufficient for my purposes.

I (apparently incorrectly) perceived some of the previous arguments concerning lack of "knowledge" about the disturbing variables from an entirely incorrect perspective.

I believe that my error was in several areas. The first would be in assuming that the discussion was about the entire person as opposed to the portion of that person actually controlling the perception.

A second possible error is in my view of what the nature of this "information in the disturbance" was considered to be by the discussion participants. There have been several postings on this recently that satisfy me (the extra rubber band was a particularly good one).

I remain convinced (at least at this point) that at least in the fashion that I was thinking concerning "information in the disturbance" my understanding of what I perceive that I have experienced has neither been shattered by, nor is out of line with, the model (so far).

I have BCP-II also but lets get through BCP first :-)

I have printed out this latest posting (as opposed to just saving it) and am putting with BCP since I am sure that I am not far enough along to fully appreciate the message. -bill

Date: Mon Mar 07, 1994 5:40 pm PST  
Subject: Re: Facts; Information in perception

<[Bill Leach 940307.18:31 EST(EDT)] >[Dan Miller (940307)]

Easy, man!

>I disagree with the first contention, and wholeheartedly agree with  
>the second. First, I must insist that you designed your experiments,  
>the machinery, the computer program, and the instructions given to  
>the research participants (which include you, Rick, Tom, and other  
>close associates) solely for the purpose of generating points that  
>lay ithe plots you describe.

As I understand it, the "plot" that you are referring to was a produced by operating on data in a fashion as instructed by Martin. Additionally, the actual data set was produced sometime ago and with no thought to ever performing said operations on the data.

>Furthermore, you tell "naive" participants what to do, what to look for,  
>and how to do it. Second, if they do act differently (as rebellious  
>subjects), then the plots would look differently is without doubt a true  
>statement.

I would indeed be interested in seeing the results of a series of attempts to cause the plot of the first derivative relationships to "look differently" than what Bill posted. I suspect that the size of the "blob" might change dramatically but real intense effort would probably be required to actually change the shape.

-bill

Date: Mon Mar 07, 1994 8:42 pm PST  
Subject: Re: MMT:DME/ETC-RKC

<Bob Clark (940307.1620 EST)> Bill Leach (940223.18:35 EST)

You quote the summary from my post, Bob Clark (940223.10:30 EST):

>>There is no internal connection between the output of a control  
>>system and any input. The system has no direct way to perceive its output.

And then you say:

>I do see how that is actually a requirement for the mind. Indeed,  
>in the activity of contemplation, outputs must often become either  
>or both of perceptions and references.

You seem to be using "outputs" in a way that differs from the way it is used in PCT. In PCT, "output" is the action of a control system in response to its error signal. "Contemplation" is "to observe thoughtfully." (This is one among several similar definitions in my dictionary.) Thus "contemplation" could be as simple as studying a picture. Or a book. Or a memory (in imagination.) etc. But the object of contemplation produces no "output." The details of how imagination is controlled could be considered separately.

To me, this is an example of the problems that arise when words from some other field are applied to PCT discussions. This is a language problem, not a theoretical problem.

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Bill Leach (940224.07:50 EST)

>If feedback as I referred to in my posting <Bill Leach 940223.18:35  
>EST)] does exist within the human control system, then direct use of  
>part of the output signal by the comparator is not unreasonable.

"not unreasonable" if you are assembling a system of some other kind.

In PCT a specific form of negative feedback control system is defined. It is, I think, the simplest form of system that could be termed a "negative feedback control system." Other systems could be defined with other properties for other purposes. Perhaps some other kinds of system will be found necessary in PCT. Although some modifications and additions may be found desirable, the present hierarchical structure of negative feedback control systems is pretty hard to beat.

>I think that there are two different cases to consider here. The  
>first is the "simple" operation which I see as just a motor control

>issue. The second is in the realm of the cognitive.

In the general case, the "motor control issue" is far from simple. PCT proposes a hierarchical structure of individually simple negative feedback control systems with a carefully defined terminology and structure that successfully describes a remarkably large portion of observed human activity.

As to the "realm of the cognitive," my dictionary definition: \_adj\_ form of "cognition." And: "cognition, n. 1. the act of process of knowing; perception. 2. the product of such a process; thing thus known, perceived, etc." This seems to be a rather broad, inclusive concept -- that includes "perception." To that extent, it might include large portions of PCT, but doesn't seem to clarify or simplify the situation.

>Are there control loops that monitor the "impulse rate" to the  
>muscles directing my hand toward the cup? Do these control loops  
>"change the gain" of the control loop that has the goal of  
>positioning my hand to the immediate region of the cup? Or maybe  
>suppress or enhance the nerve impulses in some other fashion? If  
>so, are these properly called "feedback" (I think so.)

Each of these questions can lead to extensive discussion. My best suggestion on these topics is to learn more about the terminology and conceptual framework of PCT.

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Good luck in learning about PCT!

Regards, Bob Clark

Date: Mon Mar 07, 1994 8:50 pm PST  
Subject: Re: WHO ON FRST?-RKC

<Bob Clark (940307.1625 EST)> Martin Taylor 940224

Thank you, Martin, for explaining your "symbolic-logical hierarchy."

I can see that such a hierarchy can be defined. Indeed, I can see possibilities for defining hierarchies in various ways. Perhaps one hierarchy will include another and perhaps vice versa. I think that your symbolic-logical hierarchy can be included in the standard hierarchy. However, I think that a level of "mechanical skills" can be defined in a way that both your symbolic-logical hierarchy and much of the standard hierarchy are included. I summarized one version of this in Bob Clark (930930) with SUBJECT: DEMO X, ENDING - RKC.

Thank you again Martin, for explaining "contrast." Would this concept include, "comparing two sets of perceptual signals and using the degree of similarity to form a category, and treating the degree of dissimilarity as "contrast"?"

>If there is a homuncular entity in the parallel hierarchy, it must  
>be in the original BCP structure, too,

The term "homuncular" is so broad that it can apply to very nearly any concept of human behavior. It can certainly be applied to PCT if so desired. "Homuncular" is essentially meaningless.

Regards, Bob Clark

Date: Mon Mar 07, 1994 8:54 pm PST  
Subject: Re: (FromMary):DME-RKC

<Bob Clark (940307.1630 EST)> Mary Powers 940223

Referring to Bob Clark (940218):

>If you believe that all levels of the hierarchy are engaged in  
>making decisions, then I guess it follows that you have to have a DME to do it.

I believe that any level of the hierarchy can be involved in the process of making decisions. But not simultaneously. The DME can be aware of a set of perceptions at any level. And such a set of perceptions can be present-time, imagined past-time recordings, or imagined future-time anticipations derived from past-time recordings.

The DME can perceive the perceptual signals composing the program level. It need not. It can perceive the perceptual signals composing the principles level. It need not. Its awareness can shift rapidly from on set of perceptions to another.

>But are you so certain that "examining, comparing and selecting  
>among the available (remembered) principles" is really taking place,  
>what is really going on, at the principle level - or at any level  
>other than the program level?

All I have available in forming these conclusions, are my own internal "empirical but subjective observations."\* Plus my further "empirical but subjective observations" of other peoples reports of their own "empirical but subjective observations."

\*

This phrase is quoted from Bill Powers (940131.1115 MST) where the "principles of awareness" are summarized -- "based on empirical but subjective observations."

I find your report of your "going to town strategy," "principles," "error," "consciousness," and "conflict" interesting. These appear to be a report of your own "empirical but subjective observations."

Regards, Bob Clark

Date: Tue Mar 08, 1994 7:12 am PST  
Subject: Re: Scientific facts, social phenomena

[Dan Miller (940308)] Rick Marken:

Sorry about the invective. I must be a little off my feed, or being cloistered may have something to do with it.

Nice description and analysis of Piaget's little study. I do not disagree with anything you said about it. My little point is that what you (and he) picked as facts and presented in a theoretical package is a construction. It is purposive. I do not disagree with how you did it. I do not mean to say that it, therefore, is invalid.

You say that you do not understand what I mean by my bald assertion that independent and autonomous living human control systems are the constructed product (emerging and developing from) of social relationships, social groups, etc. - and the social processes involved.

I laid out my hypothetical experiment (infant alive without human contact for twenty years) and suggested that human living control systems do not control at birth. They may have sensations, but they do not control. This strikes me as a fundamental distinction between complex living control systems and simple ones (like E. coli).

I do not think that I am in disagreement with the principles of Perceptual Control Theory. Don't tell me to read the books. I have done that. I do have some problems, though. Since you and Bill clarify the theory so well, I want to ask you to clear up this problem for me.

I may be wrong in my interpretation of events and ideas, so be sure to correct me if I am. At last summer's meetings in Durango Martin Taylor gave what I thought was a brilliant presentation. He noted that all we human living control systems receive as input is sensations. The organization of those sensations is done by and through the hierarchy - probably at the category level and above. As I understand it, this position is in agreement with the work of Gerald Edelman (NEURAL DARWINISM and other works on neural organization) and of Francis Crick, who posits the centrality of symbols in the organization of neural functions (they both are writing of humans). If this is so, then the higher levels must be developed (through social interaction?) before control is possible. (I realize that this was a big leap.) It strikes me in an evolutionary and developmental scheme of things that human living control systems may do it differently than do simple control systems. I would argue the importance of language, here.

Yet, the guardians of PCT use the model of simple control systems when enforcing purity. Most of your examples (and interests) are of complex living control systems (as with the Piaget study). My sense is that complex living control systems are qualitatively and quantitatively different from simple ones. If so, then why are you (or anybody) defending the purity of an incomplete theory. Adding necessary complexity and modifications to a beautiful, elegant model is not treason. The model is not god, and criticisms of it are not blasphemy.

Later, Dan MILLERD@UDAVXB.OCA.UDAYTON.EDU

Date: Tue Mar 08, 1994 5:37 pm PST  
Subject: Re: Scientific facts, social phenomena

<[Bill Leach 940308.18:20 EST(EDT)] >[Dan Miller (940308)]

>Adding necessary complexity and modifications to a beautiful, elegant  
>model is not treason. The model is not god, and criticisms of it are  
>not blasphemy.



Dan, even I can respond to that one (I think). When someone "PROVES" that the model requires additional complexity to be correct or even proves that as designed it is not in accordance with existing reality, such "enhancements will be taken quite seriously.

It is obvious that I have not given the implications of PCT (as it stands) a great deal of thought. I recognize that many others have however. I also believe that Bill P. is correct when he talks about the idea that we have much to learn about the existing model.

It is my naive position that all objections that I have seen so far appear to deal with both untested and currently untestable ideas.

The DME for example, seems to presuppose that the existing model will not make "decisions" "without help." Again, honestly recognizing my inexperience with the whole matter, I still can not help but feel that we have not the foggiest idea of how the existing model would behave if we could create it at something even close to the complexity of the human brain.

-bill

Date: Tue Mar 08, 1994 5:37 pm PST  
Subject: More musings

<[Bill Leach 940308.18:56 EST(EDT)] >NET

I don't think that he was speaking from any sort of understanding of PCT but I have noticed that the late Earl Nightingale must have intuitively understood some of the PCT concepts.

For example he said:

"To let another determine whether we will be rude or gracious, elated or depressed, is to relinquish control over our own personalities; which is ultimately all that we possess. The only true possession is self possession."

I recognize that from a rigorous PCT standpoint, the foregoing is almost hogwash. For example, we can not really choose not to control nor can we in any real sense allow someone else to control for us.

However, we can choose to control such that our perception of how someone else views us is the reference, or we can choose to ignore our perceptions of someone else's view, or any combination in between.

As he implies here and states more explicitly elsewhere, if someone is rude to us, IT IS STRICTLY our own decision on how to respond. That is WE alone control our response to stimuli... if that is not fundamental PCT then I should eat the tapes.

I suspect that Earl Nightingale or Dale Carnegie did not "know" PCT but it is clear that they did believe in something that I think IS fundamental to PCT and that is that "one should not bother to try to 'control' others for any such attempt is ultimately doomed to failure." I admit that there is a massive

difference between their approach to the subject and PCT's but the immediate goals were different too.

Another thought is that the "magic" that many motivational speakers talk about, roughly: "Imagine your functioning as you wish will bring it to pass." suddenly takes on a new meaning if one views such "exercises" with respect to humans as control systems...

-bill

Date: Tue Mar 08, 1994 6:29 pm PST  
Subject: Re: MMT:DME/ETC-RKC

<[Bill Leach 940308.07:24 EST(EDT)] ><Bob Clark (940307.1620 EST)>

>To me, this is an example of the problems that arise when words from  
>some other field are applied to PCT discussions. This is a language  
>problem, not a theoretical problem.

I agree. I have finally been able to start reading BCP and many of the "problems" that I felt existed with a number of the concepts that I have have seen "put forth" here on CSG-L are disappearing. This is happening both due to a slightly better understanding of terminology on my part and due to a much greater understanding of what is not necessarily stated in discussion postings.

Even for as little as I have managed to study so far, if one assumes that the general model description given by Bill is the basis of the discussions on the NET then a whole world of doubts and misunderstandings no longer exists.

Me:

>>If feedback as I referred to in my posting <Bill Leach 940223.18:35  
>>EST]) does exist within the human control system, then direct use of  
>>part of the output signal by the comparator is not unreasonable.

Dan:

>"not unreasonable" if you are assembling a system of some other kind.

Actually Bill did use the term "feed-forward" for part of what I was positing had to exist. In any event, Bill's models appear to account for conditions where control loops can sense the actions of other control loops and interact if necessary. At least at this point in my understanding of PCT, this is sufficient for my purposes. My use of the term "feedback" may have been unfortunate.

In general, at least at this point, the more I read about PCT the more satisfied I am with the entire concept.

-bill

Date: Tue Mar 08, 1994 11:02 pm PST  
Subject: Learning PCT, end of article

[From Dag Forssell (9403082300)] Bill Leach 940304.16:34

>You are welcome of course and I will look forward to your next post. I  
>don't think that you are right in your perception that I don't  
>understand PCT. I do think that you are right in that I don't begin to  
>appreciate the implications and significance of PCT.

Bill, I appreciate your measured response. My comment was mostly aimed at the discussion of feelings. Later, it occurred to me that the idea that feelings "just are" is symptomatic of a conviction that feelings exist independently from our intellect -- in a separate dimension if you wish. PCT provides an explanation for feelings (others provide none at all) which integrates feelings with other perceptions. Feelings are *\*not\** separate.

Your last few posts are very welcome. Just be warned: Have patience with yourself! Understanding the structure of PCT is the beginning. You will spend years recognizing that this, that and the other conviction you have developed is not compatible with PCT and needs to be reevaluated. Bill P. says that he has never seen anyone internalize PCT in less than two years. It is well worth it!

You saw my bickering with Rick Marken -- alias Dr. Spock. I have the utmost respect for Rick, and know that he has lived with PCT for 12 years. Still, there are things he has not thought through (I think, of course), and I feel free to call him on it. As it turned out, it was a productive exchange. We both learned.

I am a glutton for punishment. Please tell me how the following final 3 1/2 pages read. As I post them here, they are somewhat out of context, since I am not re-posting the first 9 pages, and never posted the 13 illustrations.

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#### People interacting

Exhibit 17 shows a framework for understanding the interaction between people, whether in conflict or cooperation. Here, two brains are shown, acting in a common environment (outside the body, of course). Each person is controlling (acting on) some physical variable as that person wants to. If the chosen variables are related or even the same one (say the balance of a tandem bicycle), it quickly becomes obvious that a variable is subject not only to disturbances from the environment in general, (such as crosswind), but also that each person's action becomes a disturbance to the other. Even side effects of independent action become disturbances to the other. (The balance is upset if one turns around to admire the view).

In this illustration, person #1 can represent your associate or a prospective customer. Person #2 can represent another associate, or yourself, or your prospects associate. You can readily extend this illustration with Person #3 in another department, Person #4, #5 etc., all interacting in the same physical environment. Exhibit 17 provides the framework only; the boxes are not filled in with specific understandings, wants, perceptions, output options etc. Each person in exhibit 17 lives in a personal, subjective "world" of wants and perceptions. Besides personal variations, these worlds can be very different because of professional specialization, studies, experience, and responsibility.

#### Organization

Exhibit 1 portrayed how we often think of a hierarchical organization and how we develop specialized goals for individuals in different parts of the organization.

You have probably noted the visual similarity between this hierarchical goal structure and the hierarchical control structure shown in exhibits 5, 7 and 8. Exhibit 17 shows how, once those goals have been communicated and accepted, an entire company can more realistically be portrayed as individuals working side by side in a common environment. Development, communication and agreement on goals is not easy. Telepathy between brains is not possible. (The black line represents a barrier between brains). Everyone must interact through the environment, as exemplified by the order giver and taker in the first article (page 7).

#### Lessons for leaders

It is not necessary to understand how control works to live, because people *\*are\** control systems and control whether they understand it or not. But if you *\*do\** understand, you can be more effective as a leader. A simple understanding of PCT is better than the generalizations under which business and other human affairs are conducted today.

From the detailed insight of HPCT, leaders and managers can learn several lessons:

1. Leaders and followers alike act only to produce and maintain intended perceptions. How people act in order to do this is determined by their environment. People control *\*results\** (their perceptions), not the *\*means\** used to produce those results (their actions). People achieve consistent ends by variable means. Because associates control what they perceive, the first task of a leader is to ensure that everyone is able to perceive the common goal in terms of all the perceptual variables that make it up--what the multiple dimensions of the goal are. Description is important; what are the variables you ask people to control? In the language of TQM, we talk about the importance of shared operational definitions. --What *\*kind\** of goal?
2. Once people know what perceptions to control they must know the appropriate levels at which to control them: Leaders must establish common reference levels for the intended states of the controlled perceptions--how much is desired in each dimension? This means a clear specification of the desired result; a target or goal that can be achieved continuously as part of a process or as a step in a chain of events. --*\*How much\** is the goal?
3. All associates are controlling a whole constellation of perceptions; the perceptions to be controlled at work are just a subset of the perceptions people control. Leaders are wise to be sensitive to the fact that control of certain variables will conflict with control of other variables. A leader must always be willing to be flexible about who does what, when and with whom.

To summarize so far: Leaders can understand that associates control perceptions, *\*not\** actions. Leaders are there to help people understand *\*what\** results are to be produced--not *\*how\** they are to be produced. PCT shows why "micro-management" is unsuccessful.

4. When you understand how wants relate to understanding and priorities, you can design a vision/mission statement that comes alive with meaning and people can derive their own mission statements from it.

5. When a leader understands the source of emotions, she can show associates how to consider alternatives.
6. When a leader understands the role of individual "worlds," he can anticipate conflict and create cooperation instead by mapping wants and perceptions.

When a leader teaches HPCT to associates, everyone can use the same understanding and approach, dealing with people at all levels, inside and outside the organization.

#### Mapping wants and perceptions

We have seen how exhibit 17 represents people working side by side; brains living in separate subjective "worlds" of wants and perceptions in a common environment. We understand that actions people take are an automatic result of their current perceptions compared with the corresponding wants at the moment. Exhibit 17 shows clearly that if we want to understand (and influence) actions of others, we must "map" the blank spaces in the areas of wants and perceptions. We know that people have wants and do perceive. The question is: What are they, and how?

Mapping wants and perceptions means to explore unknown territory. Where wants and perceptions are unclear to a person, questions compel the person to consider higher understanding from which wants are derived and to consider alternative perceptions that can be derived from the same set of sensory inputs "(facts)." You can also ask what actions he has considered in his imagination, and what he thinks the results would be. Mapping can range from passive exploration to very active and challenging questions which cause a person to revise the territory of wants and perceptions as it is being mapped.

The result of mapping is self- and mutual understanding. Every person involved in a cooperative task will clearly understand the relationships between their various wants, perceptions and actions \*and\* those of others. They will then be more able to work things out and support each other.

Mapping can involve a whole team. Here, let us show how you can facilitate a simple conflict resolution between two people. An associate may ask for your assistance in order to resolve some problem, or you, as his manager, peer or friend may approach him. You work one-on-one with him alone. You help by compelling him to map the conflict and draw his own conclusion.

While some ineffective action may be the reason for mapping, you don't dwell on it. At no time do you criticize him. You conduct the entire session by asking questions, offering advice only when it is welcome.

As you explore the things he wants, you are not limited to things he mentions. As an experienced person, you can ask about related wants or reasons for these wants. For instance, if he has an internal conflict-- incompatible wants--you can ask him about his priorities, which will help him to resolve his conflict.

A basic methodology might be as follows:

- 1) He asks for a meeting (or you do).

- 2) Ask him about his actions and concerns with the other.
- 3) Ask him about his own wants in relation to the other.
- 4) Ask him about what he thinks the other's wants, perceptions, and possible actions are.
- 5) Ask him to compare. Does he see any conflict between his own wants and perceptions and those of the other?
- 6) If yes, ask him if he wants to commit to work on a way to resolve the conflict.
- 7) If yes, coach and support him as he develops a plan to change wants, perceptions, capabilities and the environment to eliminate the conflict.

The point of this approach is to ask about goals and any conflicting goals and ask him to consider outcomes of his different options until he decides on a course of action that is best for him in the context of his agreement and capability to support the organization. You can renegotiate and support as appropriate if you represent "the other". Things to avoid when asking him to map himself and others:

- o Don't ever tell him what you think, but ask if he would like to have information when you have something relevant to say. If you impose your opinion on him, he perceives your message as an attempt to control him and he will resist. He is concerned about what he wants, not about what you are saying.
- o Don't ask about his feelings, (It is not productive) but rather about what causes them, namely his goals and how he presently perceives that he is doing. (That gives him a way to deal with them).
- o Don't take over his responsibilities and try to do his thinking for him. Living control systems must do their own thinking in order to function effectively. Your role is to ask questions only and teach when asked.
- o Don't ask him why he has behaved in a certain way. He must now defend ineffective choices in the past.
- o Don't bring up a negative incident from the past. It is beyond his control at this point.

This approach is not soft and wishy-washy, leaving everything up to your associates, and you powerless. You will find that the approach outlined here is more effective than setting goals for people and talking to them about what they do. (Soldani, 1989).

Through careful and persistent questioning, you help your associates focus their attention on issues (you can raise issues related to company goals) that are important to them and help their mind to come up with solutions to what they agree are their problems. Over time, you become their trusted friend, someone who cares, and they become more creative, productive and satisfied.

Summary

In this brief introduction to PCT and the HPCT model, I have touched on most aspects of HPCT and indicated how much of human experience this model can explain. I have shown a questioning approach to conflict resolution which fully respects the other person as an autonomous living control system, facilitating the development of trust, cooperation and high productivity.

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Best, Dag

Date: Wed Mar 09, 1994 4:13 am PST  
Subject: Re: Learning PCT, end of article

<[Bill Leach 940309.05:43 EST(EDT)] >[From Dag Forssell (9403082300)]

>>Feeling "just are"...

I probably ought to be "horse whipped" for that one. I KNOW better but it seems so effective when dealing with distraught people. I definitely am not aware of any understanding on my part of the PCT approach to feelings yet.

It is my belief that one of the very great problems that many people face that I deal with is one of "false" guilt. In general when someone "feels" guilty about something that they have done that is not consistent with their belief system, this is (or at least should be productive).

I deal with people that often feel guilty about their own feeling in ways that I believe are destructive and even irrational. For example, a woman once told me about how she did not "love" her two children the "same amount." In my opinion, she was really distraught over this.

Again, in my opinion, she has been "conditioned" to believe that how she feels IS the measure of her love and that if she doesn't love them both "deliriously" she is somehow "not a good mother." In talking with her, I first emphasize that there is "nothing wrong with her feelings". Specifically in her case, I actually believe myself that there was nothing wrong with how she felt about her children but rather her feelings about how she should feel was wrong. It is my conviction that as long as she is still "beating herself over her head" she will be ineffective at dealing with the problem.

By first getting her to accept that others (me anyway) do not see anything wrong with her as a person and thus reduce her focus on the feelings themselves, it becomes possible to get her to analyze her feelings more objectively.

In general, I have almost passionate disgust with much of what "religious" groups "do to" people with regard to feelings of guilt but that is another subject.

The "upshot" of this is that I believe that feelings are mostly "reactive" and that focusing directly on the feeling interferes with dealing with the feeling. I agree that we can modify our own feelings but that doing so is a somewhat indirect process.

>o Don't ask about his feelings, (It is not productive) but rather about

> what causes them, namely his goals and how he presently perceives  
> that he is doing. (That gives him a way to deal with them).

Ah, some glimmer of insight maybe... I almost always ask about feelings but consider the feelings to "interference" for the most part. Asking a person about feeling may often be the "opening" to productive discussion. I agree that letting a person "wallow" in feelings is not productive but rather they need to be lead to the point where they realize that their own uncritical acceptance the "correctness" of their feelings may be hindering the achievement of their own goals.

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I can't help this one (well you know I can but...): My immediate thought upon reading the second paragraph is that we are looking at the classic "Three Body Problem".

Dag, I can't help but feel that what is missing from your paper that could be crucial to one that works as a manager is specific reference to "the bottom 20%." Now I know that in general from the postings that I have read here, PCTers rather dislike statistics but like them or not, they often describe "reality" that one faces in organizations.

Though "I" recognize that what you are saying is applicable to all persons in the environment, a manager reading this for the first time is probably going to immediately think 'of "Joe Dork" in shipping that sent his lunch to a customer last week, strapped his hand to a package the week before, and last month....'

It might be worth some effort if you can relate how this process may take some "additional up front time" but is "proven" to be more time efficient in the long run. This 20% (or 10% depending upon whose numbers you are looking at) usually do account for 80% of a managers time (directly and indirectly).

It seems to me that trying to tell a 30 year business veteran that this method of dealing with an 18 year old employee that seems to have half of the intelligence of Chimp and ALL the hormonal drive of a rabbit requires more than just a dispassionate assertion. I am quite sure that about all of us that have had to manage or supervise an organization have had to deal with something similar and it is likely that when trying to talk about effect management techniques, it is these cases that come to mind for your reader and not the "average" person.

I also think that it may be appropriate to emphasize that Ethics, Honesty, and Consistency are vital on the part of management itself. You can "talk" and "negotiate" until you are "blue in the face" but if an employee perceives that the management is not honest, ethical or consistent the effort is wasted.

-bill

Date: Wed Mar 09, 1994 5:59 am PST  
Subject: Conventional wisdom, article

[From Dag Forssell (940309 0525)] Bill Leach 940309.05:43

>Dag, I can't help but feel that what is missing from your paper that



>could be crucial to one that works as a manager is specific reference  
>to "the bottom 20%." Now I know that in general from the postings that  
>I have read here, PCTers rather dislike statistics but like them or not,  
>they often describe "reality" that one faces in organizations.

>Though "I" recognize that what you are saying is applicable to all  
>persons in the environment, a manager reading this for the first time  
>is probably going to immediately think 'of "Joe Dork" in shipping that  
>sent his lunch to a customer last week, strapped his hand to a package  
>the week before, and last month....'

When I included a roleplay that demonstrated dealing with a dork, you joined Rick in finding objections to it. It seems I am damned if I do, and damned if I don't.

>It might be worth some effort if you can relate how this process may  
>take some "additional up front time" but is "proven" to be more time  
>efficient in the long run. This 20% (or 10% depending upon whose numbers  
>you are looking at) usually do account for 80% of a managers time  
>(directly and indirectly). .....

I may find a way to do this, if it is not clear already. Let us not focus \*too\* much on what is said or not said in these few pages. This is the second introductory article in a series of three. It is bound in a booklet with a total of 40 pages of information.

>I also think that it may be appropriate to emphasize that Ethics,  
>Honesty, and Consistency are vital on the part of management itself. You  
>can "talk" and "negotiate" until you are "blue in the face" but if an  
>employee perceives that the management is not honest, ethical or  
>consistent the effort is wasted.

Have you taken the time to consider how the employee will perceive a manager who deals with him in the way I describe? I bet you have never experienced a manager who offers no opinion, but questions you persistently and firmly for your own benefit. Do read Freedom From Stress.

Bill, your emphasis on many traditional concepts in your post "reinforce" my expectation that you will discover plenty to reconsider as you continue to live your life. From now on, you can fit your experiences as they unfold into the PCT framework of explanations. They must unfold before you find reason to question all the things you already \*know\* and have come to believe in. That, as I understand it, is why Bill P. says he has never seen anyone internalize PCT in less than two years.

Best, Dag

Date: Wed Mar 09, 1994 6:03 am PST  
Subject: Re: Scientific facts, social phenomena

[Dan Miller (940309)] Bill Leach:

Agreed. I will back down. Dan Miller

Date: Wed Mar 09, 1994 6:41 am PST  
Subject: control theory linked to information theory

[Hans Blom, 940309]

About a link between control theory and information theory -- a hotly debated issue in these circles -- I quote from a book that I am currently studying [Intelligent Robotic Systems; ed. Spyros G. Tzafestas; Marcel Dekker, New York, 1991] This is from page 47. I skip references and leave out some sentences in an attempt to make the following text more readable:

"The approach utilizes the hierarchically intelligent control systems theory as originated by Saridis and the generalized system theory of Conant, along with the duality of the concept of entropy as defined in information theory and theoretical thermodynamics to create a mathematical formulation for the analytic design of IMs [intelligent machines] operating under the constraints of hierarchically intelligent control systems. The intuitively defined (at first) and mathematically proven principle of increasing intelligence with decreasing precision [in the layers of the hierarchy as you get closer to its top] is the basic tool or constraint upon which the design is based. Furthermore, it utilizes the most important extension of hierarchically intelligent control system theory, which is that the functions performed in the three (interactive) levels of the hierarchy of IMs [thus "a hierarchy of hierarchies"] may be described mathematically with entropy functions as analytic measures. It is based on a three-level interactive probabilistic model that utilizes the associated entropy as a measure of succes of the job and accomodates fast and reliable operation of the functions of the IM."

"Intelligent control is postulated as the mathematical problem of finding the right sequence of internal decisions and controls for a system structured according to the principle of increasing intelligence with decreasing precision such that it minimizes its total entropy."

Appetite whetted? Mine is. Sounds very much in the spirit of PCT to me, but with some of the details fitted in that PCT lacks (an implementation of imperfect knowledge; a mechanism for learning; a mechanism for planning). And not just theory: a wealth of real robots/manipulators are described in the book!

Greetings, Hans

Date: Wed Mar 09, 1994 9:17 am PST  
Subject: Meat, potatoes and jellyfish

[From Rick Marken (940309.0830)]

Hans Blom ( 940309) --

>About a link between control theory and information theory -- a hotly  
>debated issue in these circles -- I quote from a book that I am current-  
>ly studying [Intelligent Robotic Systems; ed. Spyros G. Tzafestas;  
>Marcel Dekker, New York, 1991] This is from page 47.

Ah, proof by quotation. Very elegant.

>"It is based on a three-level interactive probabilistic  
>model that utilizes the associated entropy as a measure of succes  
>of the job and accomodates fast and reliable operation of the  
>functions of the IM."

>Appetite whetted?

Not really. I'm kind of a meat and potatoes guy. I just never developed a taste for raw jellyfish.

>Sounds very much in the spirit of PCT to me,

Then I would suggest a visit to the audiologist.

>but with some of the details fitted in that PCT lacks (an implementation  
>of imperfect knowledge; a mechanism for learning; a mechanism for planning).

Hmmm. And an optometrist too. PCT has all those details: "imperfect knowledge" is represented by the nature of the perceptual functions; reorganization is the mechanism for learning, and planning is the control of temporal perceptions like sequences and contingencies (programs).

After your doctor visits you might want to sit down for some BCP meat and potatoes. And I suggest that you try to eat the whole thing this time before you have that jellyfish dessert.

Best Rick

Date: Wed Mar 09, 1994 9:37 am PST  
Subject: Subject: Re: Facts

From Tom Bourbon [940308.0918] >[Dan Miller (940307)]

>>Bill Powers:

>This discussion of "facts" is growing tedious. However, I must create  
>one last disturbance.

Tedious? Why "tedious?" I thought it was just "warming up" nicely.

>We are in agreement that facts are perceptions that can be reproduced.  
>However, they are social objects in that we use facts. Facts are  
>constructed (as facts) for a purpose. We use them to win arguments,  
>sway people to our way of seeing (and interpreting), convict people of  
>crimes, and to gain control (reduce erro).

And, perhaps, we "use" them as clues concerning "what happens" in nature, or to discover what we did not know before. A number of the facts I have found in my work with PCT were surprises to me. In several cases, people and PCT models didn't act at all the way I thought they would when I set up my experimental conditions, or my models -- most often, both the people and the models did things I thought they could not, or would not, do. I learned something I didn't expect. In other cases, I learned that some of my prior ideas were right after all.

After the fact, I have found my little bag of PCT facts singularly useless as devices to sway people to my way of seeing and interpreting nature (at this late date, your own seemingly inaccurate ideas about how and why people perform in a tracking task is a case in point), or to win arguments (most often, PCT facts only serve to stir up arguments that we do not win), or to gain control (of whom or what?). I'll admit I hadn't thought of using PCT facts to convict people of crimes -- maybe I've been overlooking something promising. To think that all this time I believed I was learning something pretty much for its own sake -- for the sense of awe and wonder I sometimes feel when I see another demonstration of the elegance and power of simple model that, even in an early stage of development, explains and predicts such a wide range of phenomena with such incredible precision. I guess I just didn't see what was really going on, Dan. ;-)

>I [Dan] said:

>>>Indeed, I have great difficulty separating the consequents from the act.

>Your [Bill P's] reply:

>>Heck, that's easy. When I open a door, I do it by pushing on the  
>>door. The opening of the door is the consequent; the act is the  
>>exertion of a force. Entirely different things. I don't understand  
>>what you mean?

>The separation of cause and effect is arbitrary.

For some reason, this line reminds me of one of my favorite Calvin and Hobbes comic strips. It begins with Calvin watching his balloon float away. In the next panel he declares, "Gravity is arbitrary." The remainder of that strip, in which a small boy escapes the "arbitrary" bonds of gravity -- but only in fantasy -- is an elegant putdown of the trendy deconstructionist movement in contemporary science, a trend that reaches its peak-nadir in the behavioral sciences.

> It is more useful to see this as a process that begins with you  
>imagining and desiring the door to be open. The process culminates  
>with the perception of the door open. In this process there is  
>applied force overcoming resistance, but I would hesitate to specify  
>this as causal.

That's OK for talking about control, but your hesitation wouldn't serve you very well were you to try your hand at a little modeling of control. Playing with even the simplest PCT models can go a long way toward easing concerns like those you express here. I'd go so far as to say modeling might produce some interesting facts that bear on your concerns. Just don't try to use them to sway people to your views!

Bill continues:

>>Another example of imaginary facts is "mass hysteria" an  
>>imagined feature of crowd behavior. I refer you to Clark McPhail on  
>>this subject.

>Why would I have to ask Clark about this?

>Does he know something about "mass hysteria" that I don't know?

Yes, he does, and that's reason enough for you to ask him. I say this in light of your remarks below, where you present the interpretation of what happens in a tracking task that I usually hear from traditional experimental psychologists. You

certainly don't want to sound like them, do you? In contrast, I have often seen and heard Clark interpret those tasks (and "mass hysteria") in ways that make sense in terms of PCT. It is that PCTish interpretation that affords Clark a nice "opening" to talk about sociologists whose work prepared the ground for using PCT in sociology -- people like Mead and Couch. Go ask the old guy; he really does have this part nailed down.

Dan:

>Regarding my contention that facts are constructed, and that the  
>tracking experiments were designed, modified, and intended to generate  
>"facts" supporting your contentions about PCT, Bill responded:

>>There was nothing inherent in my actions determining that the  
>>experimental points lay where they did. ...

And that's the truth.

>Then noting the presence of a participant in the construction of his  
>facts Bill notes:

>>If that person had moved the handle differently,  
>>the points would have been differently distributed in the plot.

>I disagree with the first contention, and wholeheartedly agree with  
>the second. First, I must insist that you designed your experiments,  
>the machinery, the computer program, and the instructions given to the  
>research participants (which include you, Rick, Tom, and other close  
>associates) solely for the purpose of generating points that lay in the  
>plots you describe.

What a shattering disappointment it was for me to read this passage. After these several years, do you actually believe such things? I don't know if you have read any of our papers on tracking tasks -- I certainly can't discern that fact from what you wrote here; but I \*know\* you have watched several of my students describe their work at meetings of CSG. You said that the equipment, the instructions and the (presumed) relationship of the subjects to Bill, Rick or me cause the agreement between behavioral data points and the points predicted by the PCT models. That is a common fallacious argument put up by experimental psychologists and I think you really should ask Clark about its adequacy.

Oh, in your list of obviously corrupted and conspiring participants in our studies you forgot to mention sons, daughters, spouses, neighbors, students in my classes, undergraduate and graduate student volunteers, people wandering past demonstration-poster sessions at meetings, students and other faculty at workshops -- that's enough for now. Get the picture? :-)

> Furthermore, you tell "naive" participants what to do, what to look  
>for, and how to do it. Second, if they do act differently (as  
>rebellious subjects), then the plots would look differently is without  
>doubt a true statement.

Why the quotes around "naive?" And, \*after you talk to Clark\*, tell me about how instructions lead to control by the one instructed. I must have a mistaken idea about what is going on and I need to be set straight before I pollute the literature any further.

As for the plots looking different if a person "does something else," which means "if the person controls a different perception than the one I expected," you're right. That was the point of my paper, "Mimicry, repetition, and perceptual control," in Closed Loop, Vol. 3, no. 4, Fall 1993. That paper followed up on Models and their worlds, which Bill and I had in CL Vol. 3, no. 1. In Worlds, we talked about the common mistake in which people believe tracking is about one thing -- keeping a cursor even with a target. (We also laid out the details of how we do the modeling -- check it out -- it isn't at all like what you described in this posting.) We talked about how a person could chose any other imaginable perception to control. In Mimicry, I demonstrated what happens if the person controls, not the seen relationship between cursor and target, but the felt movements of her or his hand, in one case making felt hand movements match seen movements of the target, in another making felt hand movements match a remembered pattern of felt hand movements.

Your guess was right; the plots did look different than when the person kept the cursor aligned with the target. However, the PCT model duplicated the two alternative kinds of tracking -- all it needed was a change in its reference signal for which perception to control. But I imagine that fact won't impress you. After all, I just made it turn out that way. ;-))

Later, Tom

Date: Wed Mar 09, 1994 9:37 am PST  
Subject: Subject: Systems Theory and PCT (long)

From tom Bourbon [940308.0920] >[Cliff Joslyn (940305, all day long!)]

Wow! When you say long, you mean \*long\*, don't you Cliff?! . . .

>This dialog, although invigorating, is also a bit wearing.

Wearing? And Dan thinks the thread he started on "facts" has grown "tedious." Don't you guys realize PCT modelers are high-gain and VERY negative feedback control systems? We don't know when to let go.

>In order to avoid some repetition in the replies, first I lay out some  
>points in labeled paragraphs, clarifying my position, so we're all  
>punching at the same targets. Then there are specific replies to Tom,  
>Bill, Rick, and Mary as appropriate, referring to those paragraphs.

A good strategy, especially in a long post directed at a swarm of PCT hornets.

On the whole, I was pleased to see that you shared many of my own impressions of past interactions with systems theorists and cyberneticists.

. . .  
>[E] I agree that the current state of ST/Cyb is generally moribund,  
>and, with exceptions, has been declining steadily since the late  
>1960's. The ISSS in particular is so weak right now that they've given  
>over things mostly to the "global managers" and spiritualists. There  
>are VERY few people doing good ST/Cyb right now. As bad as you people  
>think you have it, we're worse of substantively. All we have left is  
>really the inheritance of our old institutions: a few journals, a few

>professors, a few societies.

Institutions? Journals? Professors? Where can we get some of those?!

>[H] I do NOT defend ANY of my colleagues for either their low quality work, or for ignoring PCT. I have been highly critical of them myself, and will continue to try to educate them not only about PCT, but the continuing value of the entire so-called "first order cybernetics" approach, of which PCT is a part.

\*You\* see PCT as part of first-order cybernetics. That's fine. On this side, I'm not doing FOC. I'm doing PCT, with nothing borrowed from, or deliberately aimed at, people who do FOC, or any other order of cybernetics, or Systems Theory. This is not out of spite or malice. I'm simply doing something else.

>[J] I think that it is manifestly evident that the early (pre-1970's) ST/Cyb movement is the "intellectual heritage" (whatever that means) of PCT. Any history of ST/Cyb in this century would have to include the Powers' school as a prominent chapter. More on that below.

"Whatever that means" turns out to be a very important consideration. Frankly, I see \*no direct way\* in which the pre-1970's ST/Cyb movement is the intellectual heritage (forebear) of PCT. As for PCT being a chapter in ST/Cyb, that book and chapter must be written by someone else; we aren't even thinking about that linkage. Sorry; that's just the way it is, with absolutely no offense intended.

=====

>>From Tom Bourbon [940223.0825]

>

>>A few days ago, Rick Marken [specific references are not available to me in this text editor] stirred up a hive of killer bees.

>Buzz! But hey, what about honey bees? We're all friends here, I trust.

Hey, killer bees make honey, too. And you weren't the only person to plant a stinger in Rick back then.

>>In 1977-78, systems scientists taught me how to do modeling and simulation.

>>the SGSR provided me a well-rounded exposure to systems research and modeling.

>Well then I'm glad that we did contribute something after all.

Wait a minute! :-) Those two sentences carry very different meanings. The NDTRAN modelers got me started, and I appreciate that fact. But I didn't really get under way until I started trying to learn how Bill Powers did his programming and modeling. And the sentence fragment about the exposure through SGSR was part of my remarks about the fact that ST modeling and research seemed completely unrelated to what we do in PCT. By the way, out of curiosity, the other day I went and looked at six recent issues of Behavioral Science, the old SGSR journal. From what I saw, nothing has changed so far as the huge differences between research and modeling in PCT and in ST.

. . .

>>I joined SGSR and remained a member until 1988.

>>That makes two good ones and one  
>>bad one, during 17 years; perhaps there were others I missed.

>I'm sorry, but I don't understand why you participated in the SGSR and  
>read all that material for so long? Did you find ANYTHING of value in  
>all that investment of reading? Or perhaps you do more than just PCT  
>(could it be? :->).

I just like punishment, that's all. Actually, I kept hoping to see something --  
anything -- that would convince me there was merit in ST and that there might be  
some avenue for "building bridges" with them -- an activity at which PCTers seem  
not to excel. ;->

>>During those seven or eight years, I saw absolutely nothing that  
>>would pass as a standard or common "language," whether for talking  
>>about "general systems," or for modeling them.

>Well here I do disagree. The Systems Methodologies track, led by my  
>teacher George Klir, was very prominent in the SGSR in the early  
>1980's. You must have seen their material. I regard them at that time  
>as ST's "last gasp". It was a standard, but unfortunately not common  
>enough, language.

You disagreed with me, then made my case. By "standard or common language" I meant  
one shared by ST people. There was no such animal, as you say here. I saw Klir's  
work. As you say, it was not widely used or accepted. Instead, there were many  
seemingly unrelated methods and languages. Remember, I was a relative outsider,  
looking in. The view from my location probably was very different from the one you  
encountered and constructed (do you see, Dan?) as a student of one of the "names"  
on the inside.

>But it was MATHEMATICAL, and the "global therapists" couldn't cope.  
>That's the problem: doing GOOD ST really requires formidable formal  
>skills. That school was one of the foundations of ST, and flourished  
>for a while (Mesarovic, Odum, Zadeh, Klir, a few others). This is now  
>being passed on to computer people (Wymore, Pichler, Oren), and I hope  
>to drag myself along with them. They can barely get themselves to one  
>decent conference a year. It's very sad.

I wish I had made my point as well as you did here.

>>A few of us attended the two  
>>Gordon Research Conference on cybernetics, organized by the ASC.

>Hey, cool! Who besides Bill and Mary were at the 1990 Gordon  
>conference in Tilton? That's where I met them. I don't think that was  
>an ASC proper event, but there were plenty of ASC people there.

Different meeting. The first Gordon Conference on cybernetics was at Wolfboro, NH;  
the second was at Oxnard, CA.

I'm not commenting on all of your points. As I said before, I'm pleased to see  
that your remarks confirm so many of my impressions and ideas about the  
ST/Cybernetics movements way back then.



=====

>>[from Mary Powers 940224]

Cliff:

>I'm not sure what to say, Mary. It is certainly not my place to argue  
>the history with you. I'm just a kid, and you both were there. It just  
>strikes me that denial of the intellectual heritage of PCT in  
>cybernetics and systems theory is very sad and short sighted.

How can it be short sighted, Cliff? The heritage isn't there to deny. The situation is a lot like that when you look back at the "ingenious devices" described in the ancient Arabic text that contained a compilation of many clever and ingenious control devices, some dating back to ancient Greece. The devices demonstrate that a technology of control emerged once, long ago, but it also vanished long ago and did not influence the emergence of ST, or cybernetics, or PCT. It's nice, sometimes, to look at all of the interesting places and times where people either "got it," or came close to getting it, on the idea of control. But not all of the times and places can be strung together on the same path of descent; some are off on deadends and alternative branches. Temporal priority or contemporaneity does not necessarily imply a common line of descent.

>Sigh. Maybe I'd feel differently if I had suffered your slings and  
>arrows, if I had been spurned for decades as you have, if I was a  
>practitioner rather than a theoretician. I really hope that one day my  
>community will give yours your full due. Because it really is the  
>whole ST movement, more than just psychology, that PCT so critically  
>addresses. The problem of control is perhaps the original systems  
>problem, and not only a PSYCHOLOGY problem. The REAL problem for PCT  
>begins WELL BEFORE the evolution of neurons. EVERY organism is a  
>control system, maintaining its internal metabolic state far from  
>equilibrium.

A nice idea, that, and well put.

>I guess until that happens, y'all will have some justification in  
>resisting my (apparently bizarre) claiming of PCT by ST. But when you  
>come from my side, it's as plain as the nose on my face.

It's obvious your nose is different from the noses of most other ST/Cybernetics people! :\*) <--CJ

>I wish I knew more about the history of science. I read a book review  
>of the biography of one of the early English chemists and head of the  
>Royal Society (I WISH I could remember his name, Butler?), a teacher  
>of Maxwell. He discovered many of the elements, and was instrumental  
>in early electrochemical and electromagnetic theory. Maxwell went on  
>to surpass him, of course, and he's passed out of history. Butler (if  
>that's who it was) was wrong about a lot of things, and severely  
>limited by his time, but laid the foundation for all the others. I  
>think about Ashby and Powers that way.

Not a very good analogy, I'm afraid. Butler taught Maxwell and Maxwell knowingly "built on" and surpassed the work of Butler. Not so, in the case of Ashby and Powers. You already have a Powers comment on this point.

I've enjoyed the historical jaunt along with you. Back to work.

Later, Tom

Date: Wed Mar 09, 1994 9:39 am PST  
Subject: Just theory????

[From Rick Marken (940309.0840)] And another thing ....

Hans Blom (940309) --

>And not just theory: a wealth of real robots/manipulators are de-  
>scribed in the book!

So robots and manipulators are what's "real" to you. Well we've got 'em. Haven't you seen the "little man" demo? Bill has several real hardware demos at home -- it's hard to send them around on a disk. And, of course, we have done many demos of PCT with the "mushy-ware" robots known as people.

PCT is not "just theory"; it is a WORKING model of living systems.

Have you seen Dag's PCT demo disk? It's quite good but, given your ability to compensate for the disturbances of BCP, probably not worth the very moderate expense.

Hopelessly Rick

Date: Wed Mar 09, 1994 10:01 am PST  
Subject: 99 and 44/100% Pure PCT

[From Rick Marken (940308.1320)] Dan Miller (940308)

> If this is so [centrality of symbols in the organization of neural  
>functions], then the higher levels must be developed (through  
>social interaction?) before control is possible.

Would I be acting too much like a guardian of the purity of PCT if I pointed out that many organisms (including humans) can control many variables quite well before the ability to control "higher level" variables has developed (or evolved)? You can test this yourself; watch how well your dog or cat controls its balance, gait, etc. even though it can't even talk (well, mine can, but I'm told that that is an exception).

>Yet, the guardians of PCT use the model of simple control systems  
>when enforcing purity. Most of your examples (and interests) are  
>of complex living control systems (as with the Piaget study).  
>My sense is that complex living control systems are qualitatively  
>and quantitatively different from simple ones.

Why not turn your "sense" into tests of the "simple" control model to see where it fails with "complex living systems"?

>If so, then why are you (or anybody) defending the purity of an incomplete  
>theory.

The "if so" means "if my sense (that complex living control systems are qualitatively different from simple ones) is correct". Well, maybe it's correct and maybe it's NOT. That's why we do experiments; to find out what's SO. Then we develop the simplest model to explain as much as possible of what we know is SO. This is called "science" and it was a popular way of developing a sound understanding of the world before the dark ages of "trendy science" and deconstructionism descended on the western world. From the point of view of trendy scientists and deconstructionists, I am probably the devil incarnate -- and proud to be it. But I am not a devil who is defending the "purity" of PCT; I'm just pointing out (whenever necessary, and that is almost all the time in these dark ages) when people make factual errors about how the model works or about what we have observed in our tests of the model.

>Adding necessary complexity and modifications to a beautiful,  
>elegant model is not treason.

True. It's a normal part of science. But the "necessity" of additions to the model should be determined by comparison of model behavior to actual behavior -- ie. by TESTING the model. Are your suggested additions and modifications to the model based on such testing?

>The model is not god, and criticisms of it are not blasphemy.

Yes. But back here in the 18th century, where we still do science, criticisms take the form of experimental evidence rather than "having a sense" of what is wrong.

I think you do me an injustice if you think I am defending the "purity" of PCT. I am defending the purity of science as a whole. Science was once described by William T. Powers as "disciplined imagination". The "impurity" that I see on this net is an almost complete lack of interest in "disciplining" one's imaginations with OBSERVATION. Without this discipline, imagination becomes "belief", historically the major obstacle to gracious human interaction. Believers seem to assume that all people base their ideas on imagination alone. That's why people like Tom, Bill and I must look like stubborn lunatics for arguing the same points over and over. But we are stuck with the observations the way they are (not as they are imagined --er. constructed -- to be).

I think the problem is that some people seem to be willing to look -- and change their beliefs based on what they see -- and others are not. And some that are willing to look seem unwilling to change their beliefs based on what they see. I don't think there's much we can do about this. This difference between people probably results from strategies for controlling very high level perceptions. C'est la vie.

Best Rick

Date: Wed Mar 09, 1994 10:37 am PST

Subject: SRA vs Deconstructionism

[From Rick Marken (940308.1500)]

Hal Pepinsky (980308) --

> I'm no longer concerned with "proving" to people SRA [Satanic  
> Ritual Abuse] exists.

I am not surprised at all that SRA exists. What surprises (and terrifies) me is that deconstructionism exists. I have had first hand experience with the latter (from recent discussions on the net) but (fortunately) know of the former only by hearsay.

Best Rick

Date: Wed Mar 09, 1994 12:45 pm PST  
Subject: Sexism; role-plays; internal decisions and controls

[From Bill Powers (930409.0900 MST)] Dag Forssell (940308.2300)

Some good writing in your latest effort, but you're going to get flak for sexism, even though you're not sexist. The oh-so-useful "he" pronoun unfortunately is taken as a symbol from the use of which one can judge character. There are people who are unable to read the sense of a sentence if its form is politically incorrect.

Fortunately, there are ways of writing around this, not all of which involve completely recasting a sentence, a paragraph, or a page:

>Here, let us show how you can facilitate a simple conflict  
>resolution between two people. An associate may ask for your  
>assistance in order to resolve some problem, or you, as his  
>manager, peer or friend may approach him. You work one-on-one  
>with him alone. You help by compelling him to map the conflict  
>and draw his own conclusion.

A quick fix is to say

"Here, let us show how you can facilitate a simple conflict resolution between yourself and a male associate (the example is just as valid for a female associate). "

This shows that you are aware of the sexism issue without crippling your writing style. :\*)-

-----

On role-plays.

The main problem I have with role-plays written out is that their author is in full control of both sides of the role-play, and can therefore can too easily make the worker's responses fit the theory. BOSS: I have some observations on your work -- would you like to hear them? WORKER: Oh, yes, Boss, sure, Boss, I really would (lick, lick, suck, suck) sir or madam as the case may be.

When a boss deals with a real worker, there is no way to predict what the worker will say or do. The point of control theory is that you can understand, or learn to understand, what is going on in the worker no matter what the worker says. This takes longer than a simple role-play and involves a lot more exploration. Every

interchange involves branch-points where the worker responds one way rather than another, and so does the boss. Three exchanges into a real role-play, you would have followed one series of branches out of thousands.

If you want to construct a convincing role-play, you should try to think up uncooperative or unexpected comments from the worker, with the aim of giving you and the theory a hard time (while, of course, maintaining consistency and believability). Or you should carry along several branches based on the worker's having different problems and attitudes and discuss how the boss would deal with each of them, and why. A role play should make you think on your feet, not generate a complacent sense that everything is going just as the theory says it should go.

A real role-play, a useful one to discuss, would be one that makes everybody go up a level. In every manager-worker encounter, there are unspoken rules and understandings that structure the interaction but which, under ordinary circumstances, never come up. The basic one is the power relationship. Both the manager and the worker understand that the worker's job, income, or job satisfaction is on the line and that the manager has the upper hand right from the git-go. So the worker is wondering how to convince the boss that big changes will be made, without actually having to make them, and the boss is wondering how to give this guy the message that it's time to shape up or ship out, while putting everything in the form of a question that sounds as if more than one answer is acceptable. That kind of interaction gets nowhere because it stays at one level, and therefore it isn't honest.

I can't resist trying my hand. What would happen if a manager started an interview like this?

BOSS: You know and I know that I can fire you or keep you here. You're aware of that, aren't you?

WORKER: Yeah, well, the union would have something to say about that.

BOSS: Right, and then I would have something to say, the lawyers would have something to say, and you would have something to say, and it would get to be a big mess. That isn't what I want. Do you want it?

WORKER: I'm keeping this job no matter what you do.

BOSS: Good, that's what I want, too. You may not believe me, but I hate firing people. I'm not blind, I know what I'm doing to a person when I have to go that far, and I don't like doing it. I hope that sounds like the truth to you, because it is.

WORKER: Are we finished, then?

BOSS: Come on, aren't you going to try to meet me half way?

WORKER: Well, what do you want from me?

BOSS: I'll tell you if you'll tell me what you want.

WORKER: What I want? You know what I want.

BOSS: No, I don't. All I know is what you tell me and you haven't told me anything yet.

WORKER: I want to keep this job and be left alone.

BOSS: Why?

WORKER: Why? Why would anyone? I need the dough. I don't like to be told how to do my job. I don't like people to be looking over my shoulder all the time. So what do you want?

BOSS: Pretty much the same. I don't like the VPs telling me how to manage. I don't like you telling me how to manage. So who's telling you how to do your job?

WORKER: You are. Morrie down in small parts is.

BOSS: Bullshit. I don't tell you how to do your job. I don't even know what your job is.

WORKER: Well, Morrie does, he tells me I should sweep from the corners to the middle, and where I should hang the broom, and when I should take coffee, and anything else he can think of. I told him where he could hang the broom.

BOSS: Yeah, I heard.

WORKER: That's what this is about?

BOSS: Just one of the things. How would you do your job if nobody told you how to do it?

WORKER: I'd get a vacuum cleaner, sweeping around all those machines is crazy.

BOSS: Why crazy?

WORKER: It gets dirt in them, especially if I sweep from the corners to the middle, and it just raises dust.

BOSS: Did you tell Morrie that?

WORKER: ....

BOSS: Why not? (Or why?)

And so forth.

The best way to go up a level is to go up a level. The boss went up to the unspoken level right away, so it wouldn't be hanging in the background being avoided. But there's no single way of doing this; this role play could have involved an entirely different conversation, depending on the people involved. What matters isn't the conversation, but the principles behind it. Get the conversation up to the level where some decisions can be made, instead of just fencing back and forth at the level of the conflict or hiding threats inside of questions.

It's possible, I suppose, to construct a believable role-play, but the more believable it is, the more unexpected the twists and turns. In all the role-plays I've seen, everything works out just the way it's supposed to, as if both parties had been reading the same book.

There isn't any way of phrasing things, or not phrasing things, that will have some magical effect on the other person. That's a salesman's pipe-dream. It doesn't matter whether you communicate through declarative sentences or through questions. What does matter is where you want to go with the interaction, and what you want to be while it's going on, and how much you can learn about the other person, and how you want the whole interaction to come out. You don't have to encourage people to behave like control systems. That's what they are, and that's how they behave whether you like it or not.

-----  
Bill Leach (940309.0543 EST) --

Read the chapter on emotions in LCS 2 and see how that jibes with your ideas about feelings.

-----  
Hans Blom (940309)--

"Intelligent control is postulated as the mathematical problem of finding the right sequence of internal decisions and controls for a system structured according to the principle of increasing intelligence with decreasing precision such that it minimizes its total entropy."

>Appetite whetted? Mine is. Sounds very much in the spirit of PCT to me...

I'm afraid that my appetite takes more than that to be whetted. Somehow it seems to me that there is more to control than making the right sequence of decisions and "controls," whatever that means, and that before one speaks of intelligence one should really have some assurance that the word means something. This sounds like more of the mathematics-driven systems theory that Cliff Joslyn described so well.

Nevertheless, I'll try to get hold of a copy of the book. If you liked it there must be something good in it.

Best, Bill P.

Date: Wed Mar 09, 1994 6:18 pm PST  
Subject: Re: More musings

From Tom Bourbon [940309.1211]

Well, here's hoping the great cyber-blender in the sky does not turn this brief reply into six copies that all arrive on the net two days late. (Sorry about the many copies of my two posts on the 8th. You are seeing an example of what happens when a living high-gain negative feedback control system [that's me] tries to work in an environment [my connection to the net these days] that operates with a time constant of days, if it works at all. Instability and oscillation everywhere!)

><[Bill Leach 940308.18:56 EST(EDT)] >>NET

>

>I don't think that he was speaking from any sort of understanding of PCT  
>but I have noticed that the late Earl Nightingale must have intuitively  
>understood some of the PCT concepts.

>

>For example he said:

>

>"To let another determine whether we will be rude or gracious, elated or  
>depressed, is to relinquish control over our own personalities; which is  
>ultimately all that we possess. The only true possession is self  
>possession."

. . .

>As he implies here and states more explicitly elsewhere, if someone is  
>rude to us, IT IS STRICTLY our own decision on how to respond. That is  
>WE alone control our response to stimuli... if that is not fundamental  
>PCT then I should eat the tapes.

Wait! Don't eat those tapes -- not unless you add a little mustard. ;-)

I don't think "fundamental PCT" would say that we (alone or in the company of others) "control our response to stimuli." Instead, we set the reference signals that determine whether a particular circumstance in the world around us will be "related to" our actions, in any way that an observer can discern. A particular environmental "event" or "object" can appear to be a "stimulus" only if that environmental feature affects a perception that an individual controls. Once we set a reference level, our actions are controlled by the environment and uncontrolled by us. Specifically, given that I set and maintain a specific reference level for a designated perception, then I will necessarily "respond" to the effects of disturbances that affect the environmental variable(s) that affect the designated perception. I will seem to "respond" to a "stimulus," but in a PCT model of the events, which always takes the perspective of the person represented by the model, the necessarily variable and uncontrolled actions are means to the end of controlling the designated perception.

Whether or not our "response" to another person will be deliberately "rude" or "gracious" or whatever else is determined by our reference levels, if any, for acting rudely or graciously. Also, no one can "be rude" to me unless I have reference levels for perceptions that allow me to "see" the other person's actions as rude.

On the other hand, whether any of our own actions are seen by another person as being rude or gracious is independent of our own intentions and of our specific actions and depends instead on the other person's reference levels for seeing others (which now means us) engage in certain actions; I do not control another person's perceptions of me as being rude, or gracious, or anything else, hence I cannot control that person's reactions to me as being one who is rude or gracious. (Talk about the "loopiness" of social control systems can be a little hard to follow, sometimes.)

Earl Nightingale -- a radio voice from my childhood. I didn't know anyone else in the world remembered him.

Later, Tom



Date: Wed Mar 09, 1994 8:22 pm PST  
Subject: Roleplay, article

[From Dag Forssell (940309 1845) Bill Powers (930409.0900 MST)

Bill, I think I have a new perspective on roleplays now that both Rick and you have given me enough disturbances to get my attention and I have reconsidered. The role play you posted today was a good illustration of the pitfalls and the entertainment value of a good roleplay. I shall save it, thank you.

Rick has given me more support today, with nitpicky critique and suggestions by fax and phone, and the article is now finished -- that is, I hit the print button for 30 copies in the first batch.

Another article on applications is next, and I anticipate that writing that will be easy, now that a foundation has been laid down.

I feel good about it, and appreciate the interest and suggestions I have received from netters.

In the last few days, I have written a paragraph on organization and defined the concept of "mapping wants and perceptions." Ed's approach to teaching responsible thinking (and his roleplays) become a special case of the latter.

It occurred to me today, that what has happened to me is that I have written (in the first article and in the earlier parts of this one) as clearly about PCT and the implication of it as I could. My description of a company organization (portrayed as a control system), was simply my understanding as accurately expressed as I could back in September.

Now, all of a sudden, the PCT implications I spelled out along the way, showed me a new way to discuss an organization and show how to apply PCT. (Mapping wants and perceptions in side by side control systems).

I think that I have allowed PCT to show me the way, and take me off of preconceived dead ends.

At the moment, I feel contented--really nothing, mostly tired.

Best, Dag

Date: Wed Mar 09, 1994 8:30 pm PST  
From: Richard Robertson MBX: urrobert@uxa.ecn.bgu.edu

TO: \* Purposeful Leadership / MCI ID: 474-2580  
Subject: catching up with our phone conversation

[From Dick Robertson] (940309.2145cst)

Hi Dag, I got back from being out of town for a couple of weeks to find that my last attempt to post to you had been returned. But this time I found the cause: my own stupidity. So I will try to post again with my last message. Please let me know if you finally get it. I'm also having trouble either with my procom plus package, or the mainframe at NE Il U, because I can't see my full messages, only

the bottom line keeps scrolling up. So I upload and hope for the best. ~r  
dforssel.asc

Date: Wed Mar 09, 1994 10:35 pm PST  
Subject: Re: More musings

<[Bill Leach 940310.00:19 EST(EDT)] Tom Bourbon [940309.1211]

No, I'm not going to eat the tapes though I imagine that he likely had never even heard of PCT.

>I don't think "fundamental PCT" would say that we (alone or in the  
>company of others) "control our response to stimuli." Instead, we set  
>the reference signals that determine whether a particular circumstance  
>in the world around us will be "related to" our actions, in any way that

I don't think that that is the point at all as far as taking a PCT "view" of the assertions of such as Earl Nightingale. I realize now that "mixed in" with all of the "positive thinking", "motivational hype" and "hero examples" is an underlying theme. Dennis Waitely came close when he said "Life is a 'do it to yourself proposition.'"

Such people are asserting that the individual is "in control" of their own behaviour and that they do not "have" to respond in any particular way to stimulus.

Even Frankel (sp?) made statements based upon compelling real life experience (including his own) that were clear refutation of S-R. I think that he recognized that there was a great significance to his "discovery" but as far as I know, he never codified it and I'm quite certain that he sure did not want to repeat the "experiments."

Please understand me, I am not trying to make any claims that any of these people "understood PCT" or even recognized in any scientific way the concepts of PCT. What I am saying is that much of what they "preached" is consistent with PCT and NOT consistent with S-R.

When a Nightgale tells people that when you say that someone "made you angry", you are wrong. Essential he stated something to the effect that you made a decision to be angry (right or wrong).

As I see it, this IS consistent with PCT. When someone does something to disturb your environment, you will act to control but it is still your own decision as to what references will be used.

-bill

Date: Wed Mar 09, 1994 11:26 pm PST  
Subject: Re: Conventional wisdom, article

<[Bill Leach 940309.23:43 EST(EDT)] Dag Forssell (940309 0525)]

>When I included a roleplay that demonstrated dealing with a dork, you

>joined Rick in finding objections to it. It seems I am damned if I do,  
>and damned if I don't.

<Chuckle> I hear ya!

I think that the problem with the "role" example was its' extreme oversimplicity. As an example of the function of PCT it is no doubt fine. As an example of a realistic exchange it was far too simplistic. My opinion (and probably Rick's) is that unless it is developed with at least one set of exchanges that probe to a realistic depth it will continue to appear "to phoney".

Of course I was commenting upon only what I was reading and expect you to accept, reject or question anything that I might say as you feel appropriate to your needs in developing the article.

I personally am probably "more like" your target audience than anyone else on the NET. It is with that understanding that I hope my input will be useful.

>Have you taken the time to consider how the employee will perceive a  
>manager who deals with him in the way I describe? I bet you have never  
>experienced a manager who offers no opinion, but questions you  
>persistently and firmly for your own benefit. Do read Freedom From  
>Stress.

Yes, I have and I fully believe that I would be inclined to trust such a person.

I have also worked with one manager (a Corp Pres actually) that did behave in such a fashion and I can't wait till the next time I have an opportunity to find out if he knows anything about PCT!

Where do I get "Freedom from Stress"? It will go "on the list" along with finishing BCP, LCS, LCS-II, Mind Readings and Introduction to Modern Psychology.

-bill

Date: Thu Mar 10, 1994 3:00 am PST  
From: CHARLES W. TUCKER MBX: N050024@univscvm.csd.scarolina.edu  
Subject: Mindread program on 486DX/33

Dear Dag,

I put your new (February) Mindread program on a faster machine and it made the tables in about 3 minutes and the numbers "wander" less on the screen. Thanks.

Regards, Chuck

Date: Thu Mar 10, 1994 11:43 am PST  
From: tbourbon MBX: tbourbon@heart.med.uth.tmc.edu  
Subject: Silent partner

Dag [direct],

Just a brief note to tell you that I have been following, as best I could, your development of the article. You have probably seen some of the comments on, and

consequences of, my lack of direct access to e-mail, which has continued since mid-January. I have given up trying to read and comment on e-mail, in a timely manner, which means I have dropped out of most public and private conversations. My ineffectual attempts at trying to keep up were producing too much unresolvable error.

All of that aside, I have followed the progress of your article and it looks good. I'll be watching for, and I hope I will be able to comment on, the next one.

Best regards, Tom

Date: Thu Mar 10, 1994 12:53 pm PST  
Subject: Re: More musings

From Tom Bourbon [940310.1022]

><[Bill Leach 940310.00:19 EST(EDT)] >>From Tom Bourbon [940309.1211]

>

>No, I'm not going to eat the tapes though I imagine that he likely had  
>never even heard of PCT.

Good! I didn't like the idea of you choking down a mess of plastic.

>>I don't think "fundamental PCT" would say that we (alone or in the  
>>company of others) "control our response to stimuli." Instead, we set  
>>the reference signals that determine whether a particular circumstance  
>>in the world around us will be "related to" our actions, in any way that  
>

>I don't think that that is the point at all as far as taking a PCT "view" of  
>the assertions of such as Earl Nightingale. I realize now that "mixed  
>in" with all of the "positive thinking", "motivational hype" and "hero  
>examples" is an underlying theme. Dennis Waitely came close when he said  
>"Life is a 'do it to yourself proposition.'"

I wasn't talking about Nightingale. I was replying to \*your\* statement:

"That is WE alone control our response to stimuli... if that is not fundamental PCT then I should eat the tapes."

What you said there is not fundamental PCT. That's all I said in my reply to you. No aggression or hostility intended on my part. :-))) (A friendly face, not a hostile one.)

>Such people are asserting that the individual is "in control" of their  
>own behaviour and that they do not "have" to respond in any particular  
>way to stimulus.

I got the idea -- that's what I remember from those radio days when I heard Nightingale and others talking. And that is where their ideas differ from PCT. PCT is not about how we are "in control of our own behavior." PCT is about how our own behavior (behavior as actions that affect the environment) is \*out of our control\*. We inherit, learn, or select specific reference signals for designated perceptions, then the combination of our own actions and environmental

disturbances affects the designated perceptions. For a simple-minded modeler like me, what happens after that is still one of the remarkable facts associated with the phenomenon of control: we do not control our own actions, but our "uncontrolled" actions combine with the uncontrolled disturbances to produce our controlled perceptions.

Don't get me wrong. I think Nightengale and some of the other people like him had some important insights into behavior -- social interactions, in particular. They recognized that the environment, per se, does not "cause" you to react to it in any particular way. Compared with the unadulterated S-R behavioral and social science of their day, theirs was an important alternative position to get before the public, albeit outside the channels of "respected," "scientific" behavioral science. In the eyes of "real" behavioral scientists, those people merely dabbled in pop psychology and folk lore. But, important as it is as a general alternative to S-R, the idea that "people control their own actions" is not hardcore PCT. Heck, these days most real neuro-cognitive-behavioral scientists can tell you that people control their own behavior -- first we process information about the environment, then we make a plan for our actions in response to the information, then we send the commands to our muscles, and "abracadabra" out pops controlled behavior.

>Even Frankel (sp?) made statements based upon compelling real life  
>experience (including his own) that were clear refutation of S-R. I  
>think that he recognized that there was a great significance to his  
>"discovery" but as far as I know, he never codified it and I'm quite  
>certain that he sure did not want to repeat the "experiements."

Certainly his insights were compelling, and certainly he did not do PCT experiments. That's not the point. The point is that PCT is not about how we control our behavior -- our actions. We \*use\* our actions, which we produce but do not control, as the means to add our influences to those of the environment in order to produce our own controlled perceptions.

>Please understand me, I am not trying to make any claims that any of  
>these people "understood PCT" or even recognized in any scientific way  
>the concepts of PCT.

I didn't read that idea into your posts. :-)

> What I am saying is that much of what they  
>"preached" is consistent with PCT and NOT consistent with S-R.

I saw you saying that, from the start. And what I'm saying is the idea, that object of control in PCT is a person's own actions, is not consistent with PCT. Instead, it is an example of contemporary neuro-cognitive-behavioral science, whose adherents are also opposed to, and see their ideas as inconsistent with, S-R psychology. Just casting S-R psychology into hell fire and damnation is not enough -- we must make the next step carefully. Most neuro-cognitive-behavioral scientists made a lousy next step.

>When a Nightgale tells people that when you say that someone "made you  
>angry", you are wrong.

Right!

> Essential he stated something to the effect that  
>you made a decision to be angry (right or wrong).

Not necessarily right. You made a decision to control certain perceptions at reference levels such that environmental disturbances to those perceptions might very naturally lead to you feeling anger. The presence of anger does not mean people set their reference signals directly for "feel anger."

>As I see it, this IS consistent with PCT. When someone does something to  
>disturb your environment, you will act to control but it is still your  
>own decision as to what references will be used.

And that's my point. But it is not the same as saying people control their own behavior. :-))

Later, Tom

Date: Thu Mar 10, 1994 1:08 pm PST  
Subject: Derivatives

From Tom Bourbon [940310.0840]

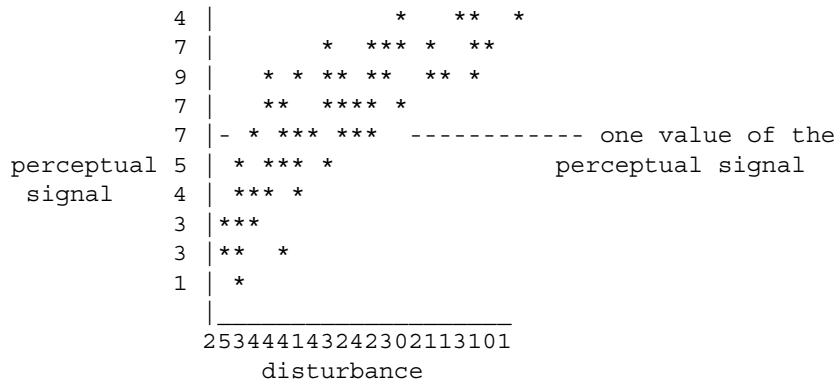
Recently, Martin Taylor posted several messages in which he described, in detail, how he would calculate the information in an environmental disturbance and a perceptual signal [Martin Taylor: 940301.1200]. My chronic network problems prevented me from taking part in the ensuing discussion of your post, Martin. (Who knows when, or if, \*this\* message will make its way to csg-1?) In part, my comments here are intended as a "reality check" on my understanding of the exchange that followed your post, but they go further than that. In particular, they demonstrate that I believe the computational procedures you described are exactly those Rick Marken, Bill Powers and I have assumed you would describe -- the classical computations, after Shannon, found in many undergraduate textbooks on perception and experimental psychology, during the late 50s, the 60s and the early 70s, before quantitative information theory fell from favor in psychology. Further, I believe your conclusions and predictions concerning the correlations between disturbances and perceptual signals, or their derivatives, did not depend on, or emerge of necessity from, information theory in the style of Shannon. I \*do\* think your predictions and the ensuing discussion mark an important step in two long-running themes on this net -- "information in perception" and the role(s) of information theory in PCT. Knowing your aversion for tar babies, I am especially pleased that you ignored that aversion and put your predictions on the net. :-))

Martin, you said you were replying to a request by Rick Marken [940228.2200] and that you would have provided this "easy" material much earlier, if only Rick had asked a sufficiently direct question. That remark from you genuinely surprised me. This was exactly the kind of material I had been requesting from you for over two years. I believe Rick had also made similar requests, starting long ago. Often, when you had made bold assertions about the role(s) of information theory in PCT, we asked that you provide one simple and direct computational example, in which you used information theoretic terms and measures. In this case, you did. Why you had not done so long ago still puzzles me; an example like this could have prevented, or better informed, many prolonged exchanges on the themes of "information in perception" and "information theory in PCT."

You said (necessarily; as was expected):

>You take the values of the perceptual signal and the values of the  
>disturbance signal and plot them against one another. If those values  
>are sampled, you have a scattergram, but if they are continuous, you  
>get a kind of scribble.

Then you included this illustration:



>You get the uncertainties of the two independent signals by summing  
>the p log p values.

You had unambiguously illustrated a strong positive correlation between the two variables. Next you described the well-known IT calculations of uncertainty in the two variables, taken alone, and together. The procedures are well known, but you had previously failed to demonstrate their application to PCT or to a behavioral task (tracking) as it is modeled by PCTers. That is what I have requested of you from the start.

In a post later that day [Martin Taylor: 1 Mar 1994 18:53:37], you said the following:

<Martin Taylor 940301 12:00> >Rick Marken (940228.2200)

>In my response to Rick's direct question:

>>How does one measure the uncertainty of a disturbance given the  
>>perceptual signal??

>I gave a direct answer. But I think I neglected to point out that the  
>question itself was inappropriate for a control system. It makes  
>sense in the absence of control, but a more useful question when there  
>is control is

>How does one measure the uncertainty of changes in the disturbance  
>given changes in the perceptual signal.

>The methods are the same, but the value of the result is quite  
>different. Over any reasonable period of time, the VALUE of the  
>disturbance is more or less independent of the VALUE of the perceptual  
>signal, as we all know. But the derivatives are not independent, at

>least not for a correct value of the time delay associated with the
>perceptual input function. So, when there is no control, the value
>question makes sense, but not when there is good control. For poor
>control, both questions are probably useful.

You had clarified your earlier post. The figure you had posted earlier showed a
strong positive correlation between the disturbance (d) and the perceptual signal
(p). As you say, such a correlation would be found only in the absence of control.
Next, you said that, in control, the value of the disturbance is more or less
independent of the value of the perceptual signal. As you say here, we all know,
or agree on, that point. Up to now, everything you had said was precisely what I
had expected, "way back when" I first asked you if IT could provide any thing more
than another way to describe the process of control.

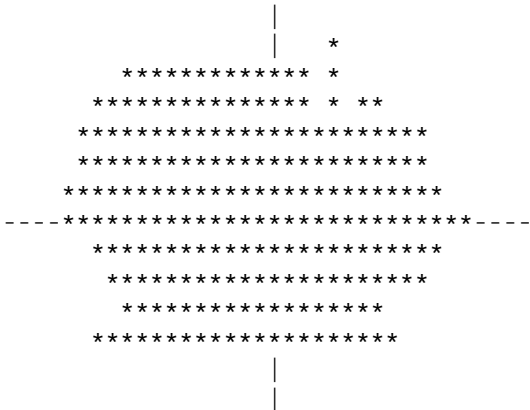
I did \*not\* expect your statement that the \*derivatives\* of d and p would not be
independent. You did not specify the nature of their dependency. From the events
that followed your correction, I believe Bill Powers, Rick Marken, and I all drew
the same inference from your remarks; the scatterplot for the two derivatives
would take the form of the one shown in your earlier post -- a strong positive
correlation. You were talking about an instance of control. It was clear to me
that you did \*not\* expect the scatterplot of the two derivatives to show no
correlation (near-zero correlation); you had carefully drawn a distinction between
the results when there is no control, compared to control. All that saved you from
a \*triple\* avalanche of replies was my infernal inability to access the e-mail.

Within a short time, you received the following reply:

[From Bill Powers (940302.1210 MST)]

>Preliminary results in developing computation of conditional
>probability:

>When I plot the first derivative of the disturbance against the first
>derivative of the optimally-delayed perceptual signal (delayed value
>of cursor position) for experiment 2 (compensatory tracking) and
>disturbance UDIS4B.BIN, I get a plot like the following:



>The plot is roughly circular and is centered on 0,0.



Bill included lists of the derivatives of d and p, from actual data, shown in the scatterplot, then said:

>I think these numbers should be sufficient to allow you to compute the  
>conditional probability of disturbance given the perceptual signal, in  
>terms of derivatives. While I don't yet follow the details of the  
>calculation, I would guess that it will come out pretty close to zero.

Rick Marken also weighed in [From Rick Marken (940302.1300)], pointing out the discrepancy between your (Martin's) "prediction" \*as it was interpreted by Bill and Rick (and me, in frustrated silence)\*. What happened next was disappointing and confusing to me. You (Martin) seemed to express dismay (surprise? chagrin? X?) that Bill had performed his analysis, and you described Rick's (typically spirited) reply as a "diatribe."

From my position of undesired and strictly enforced nonparticipation, I believe I saw:

1. a description of a specific application of standard IT computations to data from an instance of human control behavior (tracking);
2. a "prediction," offered and carefully amended by Martin, of the results of the computations in 1.
3. a presentation by Bill of the results when he performed the calculations described by Martin.
4. a radical discrepancy between Bill's results of the computations and Martin's prediction of the results (as Bill understood that prediction).
5. a high-spirited discussion by Rick of the prediction (as Rick understood it) and the actual results, and the discrepancy between them.
6. a reply in which Martin seemed to question the appropriateness of Bill's computations and of Rick's discussion, but in which Martin did not say, specifically, how the computations should be amended so that they might more adequately test his predictions. Neither did Martin clarify his prediction, if he believed Bill and Rick had misinterpreted him.

This exchange marked the first direct attempt, on csg-1, to apply IT computations, after Shannon, to data on human control behavior. If Bill and Rick (and I) correctly understood Martin's prediction, then the prediction failed. However, if we misunderstood your prediction, Martin, could you state it more clearly so that this important quantitative testing of predictions against data can go forward? This is the proper way to address the questions of "information in perception" and of the role(s), if any, of information theory in PCT.

Later, Tom

Date: Thu Mar 10, 1994 1:10 pm PST

Subject: Wisdom, Freedom, MINDREAD

[From Dag Forssell (940310 1045)] Bill Leach 940309.23:43

>I personally am probably "more like" your target audience than anyone  
>else on the NET. It is with that understanding that I hope my input will  
>be useful.

That is how I took it. I appreciate that you were only slightly offended when I thanked you a few posts back.

>Where do I get "Freedom from Stress"? It will go "on the list" along  
>with finishing BCP, LCS, LCS-II, Mind Readings and Introduction to  
>Modern Psychology.

This net has a very short memory--as Greg Williams once comforted me, when I had said something foolish. Does anyone ever read the CSGintro document which Gary posts once a month? It has references!

Here is a repeat of a recent post, updated for correct pricing:

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Subject: Hal's understanding

[From Dag Forssell (940214 1200)] 940214.1030 Hal

Hal,

You have taken some of the words used by PCT and developed your own understanding of what they mean, in the context of your lifelong experience and uniquely personal interpretation of the english language. I am not picking on you. We all have a uniquely personal interpretation.

PCT is an engineering science, not just any collection of loose word pictures. PCTers use the PCT terms with precise meanings, and do not substitute terms that happen to sound similar but may mean anything at all. Only those who study the PCT texts and computer demonstrations with care and precision have ever grasped the full message of PCT.

There is one book available I would recommend to you, which introduces PCT in as lay a language as possible, but even that book must be read with care, in sequence, with attention to the very few graphs in order to for the message to be understood.

\_Freedom From Stress\_ by Ed Ford, our resident PCT counselor and teacher, is easy reading (despite my caveats above), starting out as a counseling session about a job related problem and branching into marital problems, child rearing problems, and ways to manage your boss. The newest edition includes a report on how Ed teaches his parenting program to teachers and parents together in several school districts. Ed is a very non-technical fellow, but due to his early experience that PCT works, he has struggled diligently to learn enough technical specifics to be able to teach the essence of PCT correctly. He has leaned on other PCTers to make sure his understanding and manuscript are technically correct, while the principles are expressed in lay language and illustrated in the problems of daily life.

Now that I think of it, Ed's previous book, \_Love Guaranteed\_, is even easier to follow. It is shorter and less involved. They cost \$10 and \$9 respectively. I don't think you will be sorry if you send \$22 (10+ 9 + 3 for shipment) to:

Ed Ford  
Brandt Publishing  
10209 North 56th Street  
Scottsdale, AZ 85253 (602) 991-4860

With your request.

If you find PCT valuable and important to you, you will make the effort to learn it correctly. If you don't, you won't. (Says PCT).

-----  
Bill Leach 940310.00:19

>Please understand me, I am not trying to make any claims that any of  
>these people "understood PCT" or even recognized in any scientific way  
>the concepts of PCT. What I am saying is that much of what they  
>"preached" is consistent with PCT and NOT consistent with S-R.

The following post may be of interest to you. Send the message

get CSG-L LOG9401E to LISTSERV@VMD.CSO.UIUC.EDU to get it

Subject: PCT, behavioral theories, and ordinary experience  
[From Bill Powers (940129.0940) MST] Clark McPhail (940128)

-----  
Chuck Tucker (direct)

Good that MINDREAD worked well. That is the final version, then.

-----  
Best, Dag

Date: Thu Mar 10, 1994 2:13 pm PST  
Subject: PCT Police Action

[From Rick Marken (940310.0930)] Tom Bourbon (940309.1211) --

>I don't think "fundamental PCT" would say that we (alone or in the company  
>of others) "control our response to stimuli."

Careful, Tom. People might think that you're in cahoots with me on the PCT purity police force.

Bill Leach (940310.00:19)--

>Please understand me, I am not trying to make any claims that any of  
>these people "understood PCT" or even recognized in any scientific way  
>the concepts of PCT. What I am saying is that much of what they  
>"preached" is consistent with PCT and NOT consistent with S-R.

Apparent consistency with PCT is not good enough for the PCT police. There are many points of view that can be seen, in one way or another, as consistent with

PCT. This is the basis for the dreaded "nothing but" syndrome. For example, scientific psychology is now dominated by one form or another of "cognitive" theories which say that "mental events cause behavior". Cognitive psychologists reject S-R causality so, when they run into PCT they say "yeah, PCT is JUST LIKE (or NOTHING BUT) 'goal theory' or 'mental model' theory or whatever". After all, PCT does say that mental events (reference signals) determine what we "do". But what reference signals really specify is what level of a perceptual variable we are to perceive. Every cognitive model I know of says that behavior is CAUSED OUTPUT; PCT says that behavior is CONTROLLED INPUT. In his marvelous post, PCT police lieutenant Tom Bourbon (940309.1211) explained why this apparently small verbal difference makes ALL the difference when it come to understanding human behavior.

Best from police headquarters

Rick

Date: Thu Mar 10, 1994 2:23 pm PST  
Subject: PCT and representational momentum

[From Richard Thurman (940310.1130)]

Bill, Rick, Tom and other PCT researchers.

I have been very hesitant to ask a question for the past few weeks for fear of it looking like just another 'how does PCT relate to....' question. The reason it bugs me is that I feel I should know the answer, and indeed I keep having one of those 'feeling of knowing' experiences every time I try to think it through. Alas I am unable to answer it for my benefit and wonder if you all would indulge me just this once.

The question I have is on 'representational momentum.' That is, figures displayed on a computer screen that have undergone a rotation or translation are remembered as being slightly beyond their final position. For example, in a standard representational momentum study, subjects view successively (say on a computer screen) three rectangles of identical dimensions that are oriented (say) 15 degrees apart. The shift in orientation suggests a clockwise (or counterclockwise) rotation of a single rectangle. Kind of a crude animation effect. After the third figure is removed, a fourth rectangle is shown that may be in the same orientation as the third or it may be slightly forward or backward relative to the implied rotation. A subject is supposed to judge whether the fourth rectangle is in the same orientation as the third.

When these studies are run, most subjects (yes... not all) will answer that the fourth rectangle matches the third only if it is rotated slightly further than the third actually was. Now I know that the group based statistics are bogus measures (translation... yes I have read BCP, Mind Readings, Casting Nets, LCS 1 & 2). I am more interested in the individuals who do consistently display this pattern.

What is the PCT explanation for this pattern of behavior? What perception(s) can we postulate the individual is controlling? How would one go about explaining this phenomenon? How would one test for the controlled variables guessed at above? How would one perform the kind of research that PCTers would like to see published.

The closest I can come is that it has something to do with control of configurations and transitions. Yet what is it about the dynamics of a control loop that make this happen. Is it the same phenomenon that happens when you ask someone to track your finger as you make a circle in the air and then stop moving your finger. (One can see slight overshoots there as well.) Does it just take a while for the loop to 'wind down?' And if so ..... well you get the idea.

Any help on thinking this through would be greatly appreciated!

Thanks Rich Richard Thurman Air Force Armstrong Lab

Date: Thu Mar 10, 1994 5:07 pm PST  
Subject: Re: Representational momentum

[From Rick Marken (940310.1430)] Richard Thurman (940310.1130)

>The question I have is on 'representational momentum.' That is,  
>figures displayed on a computer screen that have undergone a  
>rotation or translation are remembered as being slightly beyond  
>their final position.

>What is the PCT explanation for this pattern of behavior? What  
>perception(s) can we postulate the individual is controlling? How  
>would one go about explaining this phenomenon?

It looks like a very interesting way to study memory -- especially if it were set up as a continuous control experiment (rather than the way it seems to be typically done, where the subject must choose from a distinct set of orientations).

I would set it up like this. Show an object rotating through a partial turn (this probably works with any object, so a line, rather than a rectangle, should work). Then turn off the object (the "off time" would be a variable of the experiment) and then turn on the comparison object and have the subject rotate it to match the final (remembered) orientation of the first object. This procedure (or something like it) should let you quantify the degree of "representational momentum".

My first shot at a model would be one where the perception controlled (when adjusting the comparison) is the relationship between the perceived orientation of the comparison object,  $p$ , and the remembered orientation of the original rotating object,  $p'$ . So the subject is trying to keep  $p-p'$  equal to 0.  $p'$  (the remembered perception of the final position of the original rotating object) is the interesting part of the model, of course. There are many ways to get  $p'$  to show apparent "momentum". Until I knew a lot more about the phenomenon I would probably make  $p'$  have the temporal properties it needs so that the model's control behavior (keeping  $p-p'=0$ ) matches the subject's.

I bet we could test and model this phenomenon using HyperCard; what say?

Best Rick

Date: Thu Mar 10, 1994 6:11 pm PST  
Subject: Missed Posts

From Bill Powers:

I lost all posts today, the 10th, before about 3 PM, due to a full disk. I can recover the CSG-L posts Tuesday (when I get back from Boulder) from the archives, but direct posts (Rick and Tom, as I remember from the screen directory) are gone.

See you tuesday bill

Date: Thu Mar 10, 1994 11:17 pm PST

Subject: Derivatives and Clarifications

[From Rick Marken (940310.2130)] Tom Bourbon (940310.0840)

Excellent post Tom. But I have one little concern about it. You quoted Martin Talyor 's (940301 12:00) fateful claim that:

>the VALUE of the  
>disturbance is more or less independent of the VALUE of the perceptual  
>signal, as we all know. But the derivatives are not independent, at  
>least not for a correct value of the time delay associated with the  
>perceptual input function.

and then correctly describe the subsequent events:

>a presentation by Bill of the results when he performed the  
>calculations described by Martin.

>a radical discrepancy between Bill's results of the computations  
>and Martin's prediction of the results

You then conclude:

>This exchange marked the first direct attempt, on csg-l, to apply IT  
>computations, after Shannon, to data on human control behavior. If  
>Bill and Rick (and I) correctly understood Martin's prediction, then  
>he prediction failed. However, if we misunderstood your prediction,  
>Martin, could you state it more clearly so that this important  
>quantitative testing of predictions against data can go forward?  
>This is the proper way to address the questions of "information in  
>perception" and of the role(s), if any, of information theory in PCT.

I completely agree with your last sentence. But I am worried by the request that Martin clarify his prediction if we misunderstood it. I can't imagine a clearer claim (or prediction) than the one that Martin made above ("But the derivatives [of perceptual signal and disturbance] are not independent"). When we showed a plot indicating that these derivatives ARE independent, Martin said that this result was EXPECTED. So I can only presume that Martin's "clarification" of the prediction of non-independence of the derivatives is that they ARE independent. Is that clear?

I am afraid that this "clarification" process could go on forever; Martin makes a prediction, we ask if that's what he really means, he says "yes" (usually in a private post, as he did to me when I asked if he really meant to say that the derivatives are not independent), we collect the data, it doesn't fit the agreed on prediction, we think we have falsified the prediction but then Martin wants to

"clarify" what he really meant to say. This could become silly; we'd have to get people signing sworn avadavits and whatnot.

Anyway, it's up to you (and Bill) if you want to pursue this: if you do, I'll make one prediction (that won't need clarification) -- I predict that every time you get a result that is inconsistent with one of Martin's predictions you will find Martin saying that you had not been "clear" about what he really meant.

Good luck Rick

Date: Fri Mar 11, 1994 5:18 am PST  
Subject: Re: More musings

<[Bill Leach 940310.15:39 EST(EDT)] >Tom Bourbon [940310.1022]

Actually, its that stringy oxide coated mylar that would be really tough to handle (even with mustard).

Tom, I really do appreciate the efforts of you and others here in helping my understanding. Maybe I am "out of line" even pursuing this until after I have at least digested some more of the fundamental texts.

I accept that we act to control our perception. I accept that because, that is the way control systems operate and I refuse to accept the idea that the humans are not control systems. Also, while there may really be many phenomenon about human experience that we do not understand that is often classified as "mystical" by some observers I fundamentally believe that all physical phenomenon IS rationally explainable even if WE can't do it.

I can envision how S-R psychology must have seemed "like the answer" and can even see why many might "want to hold on." After all, it certainly would not be at all difficult to present millions or even billions of examples of human behaviour that "appear to validate" S-R.

Of course the problem that S-R faces is that you only have to present ONE valid example that contradicts the "model" and the "model" is in trouble. That does not mean necessarily that the model has to be discarded completely. A partial model can be and often is actually useful particularly when the limitation(s) are well understood. And no, I am not trying to "make a case" for retaining the S-R "model", that is just a general statement. In the S-R situation, it appears to me that the "model" is so fundamentally flawed to the point where it is hampering research and treatment that it should be discarded completely.

I no doubt, am pushing for understanding of "practical consequence" of PCT in areas where I feel some comfort in discussion. Like many people with a grey hair or two, I have experience a lot. I know that for my own life and "happiness", the vague understanding that "I did not have to be a 'victim' of circumstances." is probably the single most important realization I have ever had.

It is my "very real to me" perceptions that we really can "reprogram" ourselves to achieve what we want in life. We really can choose to be basically happy or unhappy.

That is, while we can not always choose what happens to us, we can usually choose weather it is "bad" or "good." We are in control (in the pop-psychology sense as well as PCT).

I am not trying to call any of this PCT. What I am try to do is to understand it in terms of PCT and see if what I believe about the appearent success of this "stuff" makes logical sense in the PCT model.

I fully recognize that PCT no more trys to make "value judgements" than a pressure regulator. I consider that using "positive thinking" modes IS a good thing but PCT has to be able to explain Charles Manson just as easily as Saint Francis (ultimately).

>Frankel

No, the point is, that he "kicked" the supports out from under S-R in an irrefutable, undeniable way (well not really, some people can ignore any facts).

If I don't take Rick as hostile, I certainly won't see you that way :-)

-bill

Date: Fri Mar 11, 1994 5:18 am PST  
Subject: Re: Wisdom, Freedom, MINDREAD

<[Bill Leach 940310.16:42 EST(EDT)] >[From Dag Forssell (940310 1045)]

>That is how I took it. I appreciate that you were only slightly  
>offended when I thanked you a few posts back.

You really had me smiling over that one. Of course I suspect that there is a great deal of grinning going on when PCTers read some of my comments concerning my own believed level of PCT understanding too.

With the work load here, the progress is slow but I still am amazed at the depth of thought on this subject. I just read Bill's four page preface to Living Control Systems (I had a meeting yesterday and took that book with me in case I had some time). Like literally everything else of his that I have read so far, I am overwhelmed by how profound, how clear and how concise his writing comes across.

I probably should post this private since I'm sure that it gets a little embarrassing for Bill after awhile (I'm quite sure that I'm not the first to talk like this about his work)... but I won't.

I would liken the experience with Bill's work to my astonishment when after spending many hundreds of hours studying various works in my attempt to gain some meger understanding of Relativity, I stumbled across a book written by Albert Einsein himself on the subject. I was a child at the time, but I still remember laughing and even crying as I began seeing the stunning clarity of his presentation.

I really believe that there is more in this comparison than one might think. My earlier experience taught me that in a fundamental shift in thinking it is quite likely that in the works of the originator of the shift may still be the only



source that really presents the "path to discovery". Indeed, that was what was "wrong" with everything else that I had read on Relativity. Einstein was writing to a world that did not "know" that he was "right", but the other authors that I had been reading were writing about an accepted theory.

The "upshot" of all of this is that right or wrong, I am convinced that I must 'absorb' Bill's work first. I must let him "lead me through the discovery" or I will never fully appreciate the work of any that build upon his work. I recognize that Bill was not alone in this but his physics and engineering background probably makes his 'style' best for me.

>when I had said something foolish. Does anyone ever read the CSGintro  
>document which Gary posts once a month? It has references!

The answer is yes but one does not necessarily always remember the details.

-bill

Date: Fri Mar 11, 1994 5:54 am PST  
Subject: Re: Conventional wisdom, article

From Tom Bourbon [940310.1242] Bill Leach 940309.23:43

Just a brief suggestion, Bill. It concerns the following:

>Where do I get "Freedom from Stress"? It will go "on the list" along  
>with finishing BCP, LCS, LCS-II, Mind Readings and Introduction to Modern  
>Psychology.

Drop a private post to Ed Ford at 0005913466@MCIMAIL.COM

I am sure he will be DELIGHTED to tell you how to get a copy.

Also, you might want to expand your reading list just a little. ;-) Easy for me to say. You should include a couple of sources that will let you see work by a few of us who don't have our own books, yet.

1. An edited book:

Wayne A. Hershberger (Ed.), 1989. Volitional Action: Conation and Control.  
NY: Elsevier/North-Holland.

Wayne's book contains a mix of PCT chapters and chapters more or less related to PCT in that the authors write about some aspect or another of "volitional action," rather broadly defined.

2. A special issue of a journal:

American Behavioral Scientist, Sept/Oct 1990, vol, 34, no 1. A special issue edited by none other than Richard S. Marken, containing articles by twelve different PCT authors, or sets of authors. In this case, all of the material is on PCT.

With these additions, the list should keep you busy for a while. :-))

Later,

Tom Bourbon

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University of Texas Medical School-Houston

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6431 Fannin, Suite 7.138

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Houston, TX 77030 USA

tbourbon@heart.med.uth.tmc.edu

Date: Fri Mar 11, 1994 7:01 am PST

Subject: Re: PCT Police Action

<[Bill Leach 940311.08:08 EST(EDT)] >[Rick Marken (940310.0930)]

Dear Mr. Police Officer;

I didn't do it, and if I did, I didn't mean and won't do it again :-)

I suspect that a lot of my postings are "so much noise" to others on the net (though I really do hope that I am not upsetting people too much).

Read my lips: I ACCEPT "CONTROL OF INPUT"! :-)

I have done a bit of design work occasionally over the last 30+ years and some has been with control systems in the classic mechanical sense, so again -- I BELIEVE IN CONTROL OF INPUT.

<as an aside>BTW, I have noticed that often when one encounters a person that has trouble with that idea, it is useful to use the specific term "drive" in an explanation. That is, many people that seem to have trouble with the control theory concept that a controlling system is controlling input will almost magically understand if you just say: "A control system controls its input by driving its output." and then go on from there.

I want to accept and internalize that humans are control systems. The concept is so elegant, so ..... CLEAN! I don't, for a moment, doubt that my years of experience as and with humans has "filled my brain" with "truths" that just aren't! I am not bothered by this, I have had to go through such things before though probably nothing even close to being as "ingrained" as this sort of "knowledge and understanding".

I am trying to understand how control theory works to explain human behaviour at the purpose level. That is, I am absolutely comfortable with PCT concepts of the control systems explanation for the physical operation of limbs, voice, etc. I don't however, think that I have the "Ah Ha, this is obviously true!" feeling for how PCT explains "higher level" behaviour -- yet.

A part of my search for understanding is to deal with "other discoveries" that seem to be true (TO ME). I believe that when one learns to employ the teachings of such as Dale Carnegie a person will almost always experience stunning "success" in their life and be a "fundamentally happy" person. I KNOW that I do not have proof in the scientific sense that such is true but I do have rather compelling evidence from experience that indicates that it is true.

In trying to relate what little I understand about the implications of PCT for human behaviour, I personally have to come to terms with such beliefs.

I recognize that in the first place, my conclusions about such behavioural modification techniques could just plain be wrong. It may well be that Carnegie (and others) have not discovered any generalized principles about human interaction. Though I really have a VERY long way to go before I will believe that.

It could be that even if such discoveries are true that they are irrelevant to PCT. I have even more trouble with that thought. In my concept of what PCT is, at some point in the future, given enough information PCT should be able to explain the success or failure of any human interaction process.

At my present stage of understanding I am not so concerned with whether the theories or ideas of these others are "good PCT" but rather whether PCT explains their successes and failures. As I see it, at least in a general way, it should.

Now then, that is one point. Another, equally important is that when I am "assaulted by the PCT thought police" and "beat over the head" about some "minor nit", I do recognize that such (possibly) seemingly minor detail can be vital to understanding.

For example you can explain the operation of a controlling control loop by starting at ANY point in the loop and going full circle. Such a method may even be work for complex control systems but "understanding" can not be achieved unless one recognizes that the whole process does have a "starting point" and that the starting point is always purpose (which at least for an existing design, is usually assumed rather than explicitly addressed). [I have to chuckle a little at that last parenthetical comment as the thought occurred to me that as far as control system design efforts are concerned, there just aren't many designs that fit the description of "already existing" as profoundly as we the people! ] Obviously if you start with purpose then there is only one view that is correct and that is that everything that happens in a functioning control system is a result of trying to match input to reference.

I don't doubt any of that but I know I am not used to thinking in terms of it applying to "even" my own behaviour. The implications are staggering.

-bill

Date: Fri Mar 11, 1994 11:57 am PST  
Subject: Re: PCT Police Action

[From Rick Marken (940311.1030)] Bill Leach (940311.08:08 EST)

>Read my lips: I ACCEPT "CONTROL OF INPUT"! :-)

Not to worry. I'm not a "belief" policeman; I'm an "understanding" policeman. I don't much care whether people "accept" PCT or not. So you won't get a ticket from me for rejecting PCT. But, nor will you be able to get out of a ticket by simply saying that you "accept PCT". I'm not interested in getting "converts"; I'm interested in getting people who understand 1) what PCT is about (control) 2) how it applies to behavior and 3) how it works (control of input).

I call myself a "policeman" , by the way, because I think I am perceived as being one by many people on the net -- as Dan said, I seem to be trying to maintain "PCT purity". But I just think of it as trying to be accurate about the three points I mentioned above. Sometimes my interest in accuracy may seem like a personal attack -- but it's not (though I admit that I may have a communication style problem -- I like my style, though, so I guess we're all stuck with it). My "PCT Police Action" was satirizing the perception that I think people have of me as the Ayatolla of the net; I was not really policing your "beliefs"; I was policing statements that you made about PCT that (I thought) were wrong. I would rather be thought of as the strict, inflexible math teacher who will only accept the EXACT right answers; still an annoying person but, at least, not a belief policeman.

Again, I am just not interested in whether people "believe" in PCT or not. What I care about is that they know how the model works, how it applies to behavior and (given those two) HOW TO TEST IT. The only reason anyone should believe in PCT (or anything else, for that matter) is because they have TESTED IT -- and tested it PROPERLY. People who have not tested PCT are not of interest to me; if they believe in it, that is no more important to me than that they believe in christianity or whatever; if they don't believe in it then BIG DEAL; they probably don't like it because they don't like the word "control" or something like that.

It is very difficult to correct a misunderstanding without implying that the person who produced it is "dumb". That's the spot that I'm in; if I say that some statement is wrong, the person who made the statement is likely to feel bad and angry at me. I don't know how to avoid this problem.

All I can say is that I LOVE EVERYONE WHO PARTICIPATES ON THIS NET even though I think that some of you (like Martin, for example) say things that are demonstrably wrong about PCT. I care about getting the PCT ideas right; I am not interested in "putting people down" or "proving how smart I am". But I do have strong feelings about getting the PCT message across honestly and ACCURATELY. I know that I seem like a policeman but please try to remember that, whenever I "attack" something that seems incorrect about PCT, I am attacking the idea -- NOT the person who proposed it.

And, by the way, when people come back and tell me (often quite rightly) that I am wrong, I assume it's because what I said was wrong (or because it was thought to be wrong), not because I am "dumb" (at least, I IMAGINE that that's why people are doing it).

Best, Rick

Date: Fri Mar 11, 1994 1:22 pm PST  
TO: BILL SILVERT MBX: bill@biome.bio.ns.ca  
Subject: Server information for CSG

[From Dag Forssell (940311 1320) - direct]

Bill, I am wondering if the part of CSGintro document that deals with your BIOME server has become obsolete. You must have been traveling or something, since I have not seen any comment from you on the DEMODISK I sent, or on the index structure (which I found you have changed??!).

I will certainly appreciate a report or some comment from you. Best on the net, of course.

Thanks, Dag

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Document files and uuencoded versions of program files can also be obtained via e-mail. Here are some basic commands for obtaining files and information:

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 modeling 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.

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Date: Fri Mar 11, 1994 2:16 pm PST

Subject: Embarrassment, Testimonial

[From Dag Forssell (940311 1200)]

Bill Leach 940310.16:42

>I probably should post this private since I'm sure that it gets a little  
>embarrassing for Bill after awhile (I'm quite sure that I'm not the  
>first to talk like this about his work)... but I won't.

I am glad you don't post things privately. This net is for PCT discussion and yours qualifies. Your reference to Einstein sounds interesting; something I would like to look up some day. Do you have a title?

Anyhow, it is impossible to embarrass, insult or flatter Bill Powers:

>[From Bill Powers (940115.0930 MST)] RE: insults

>

>The dictionary has a great deal of trouble with the word "insult." Most  
>of the definitional loops are small and tight (see "insolent"). The  
>larger loops have to do with being attacked or treated with contempt.  
>I deduce that the main effect is an injury to one's self-image. In  
>George Herbert Mead's world, where the self is defined by society,  
>everyone else in the world controls your self-image, so insults must be  
>common. If, on the other hand, your evaluation of yourself is not  
>strictly a function of other people's opinions, you are much less  
>vulnerable to insult -- you might even be impervious to insult (which  
>implies that you are also impervious to flattery). In that case, the  
>occurrence of an insult says more about the source than the destination.

>So far I have not felt insulted by anything I have read on this net. I  
>have occasionally been embarrassed by reading certain posts, written by me.

-----  
Dag Forssell  
Purposeful Leadership  
23903 Via Flamenco  
Valencia, CA 91355-2808

March 4, 1994

Dear Dag,

It has been nearly 18 months since you presented Perceptual Control Theory to a small group at Radionics. I am writing to report on results from applying PCT to New Product Development, so you can share them with other students. Learning PCT has made a difference in my life, both professionally and personally, and I will be happy to share more of my experiences and answer any questions.

In the daily management and coaching of our product development engineers, PCT has helped me to explain to them why marketing, purchasing and manufacturing personnel routinely "see things differently"--resulting in the predictable mix of confused communications, misaligned expectations, and missed opportunities to "do it right the first time." I have been able to help the engineers in such a way that the new product introduction team could refocus on the tasks to be achieved--effectively overcoming individual perception barriers to full cooperation.

Radionics manufactures computerized security systems. My role is to support the development of new products. I have studied and taught TQM and communications techniques to the engineers to help them become more effective contributors to their teams. My experience is that engineers can be skeptical of training programs. I was very pleased to see how easily our engineers accepted your logical

presentation. The product development engineers, project managers, and marketing product planning managers who attended your seminar are working together more effectively today.

In the development of new products, most projects begin with a relatively high degree of "uncertainty" (features, costs, intended customers, technical questions, etc.). The project team's goal is to deliver tangible product, at a specific selling price, on a specific date. The team's charter is to achieve these goals by interacting with all internal and external parties to dramatically reduce the uncertainty to a predefined level. The primary medium of exchange in new product development is information. The timely, complete, accurate, and detailed exchange of product related information is the key determinant of how effectively a project will achieve its goals. Individual differences in perception frequently disrupt the free flow of information in "real world" product development.

Let me tell you about one project that was initiated a few months after we learned about PCT. In the early stages of this new project, clear interdepartmental "battle lines" were drawn early and emotionally. The old timers in marketing wanted to use an outside consultant to develop a new product, while the engineers wanted to develop the product in house to take advantage of many reusable aspects of their software libraries, test plans, and circuit designs. Emotions were running high when I interviewed the marketing product manager to evaluate his "world" of wants and perceptions. Marketing's concerns were all "emotional" issues. They expressed discomfort at throwing yet another project on the pile of things the engineers were already working on. There was a clear concern that the engineering department was taking too long. The marketers felt a strong distaste to go through a lengthy discussion of product requirements. In short, they thought that working with engineering was going to be a long painful process while working with the consultant (a well liked former engineer) was going to be a breeze.

Our in-house engineers' perceptions were that maintaining the consultant's code would be difficult. His previous products were judged to be difficult to maintain or upgrade with new features, since nobody could follow the documentation he had provided for them.

Based on this understanding of the concerns of both parties, I helped the engineers prepare a presentation that addressed a far wider range of issues than they originally felt comfortable dealing with. They presented detailed cost analysis data, project scope estimates and an executive overview. At the end of the day-long series of presentations, a previously unrealized alternative was selected by the group as it reached consensus to develop the product in house. This consensus would not have been reached without mutual understanding of the other person's perceptions.

In another, more recent project, the software engineer had a very detailed "architectural vision" of how to deliver a requested new feature to our market. His vision delivered the feature in such a way that it would also open up numerous additional possibilities that actually exceeded the scope of what his marketing partner (a company VP) had requested. The engineering project scope necessary to do it this way was larger than required for the initial request, and the benefits weren't immediately visible. Here's what we did:

- 1) I spoke with the software engineer and got a clear understanding of his "world." His vision saw our security systems gaining new capabilities. Many future potential requirements could be satisfied by a few weeks of additional

requirements definition and a week or two of additional software development. Overall, the additional project risks due to the expanded scope were not large.

- 2) Then I spoke with the VP to understand his perceptions that centered on the reluctance to deal with the unknown and the fear of wasting time gathering data in which he had low confidence.

Armed with a map of where the perceptions were, I set out to resolve the situation.

- 3) I instructed the engineer to gather as much data as he could. Using some on-line services he had reams of data available in two days. Sifting through it, he was able to gather enough relevant and recent data to demonstrate that the market potential was very exciting and well worth pursuing. On the basis of what he was able to dig up so quickly, we were able to schedule a quick presentation, convince our VP of the market potential, and get him excited about it.

- 4) The final step was to schedule two days of conceptual design reviews to ensure that the dozens of scenarios for potential requirements were well thought out from our customers' point of view. At the end of two days of meetings, the engineer had experienced the "thrill of victory" and the marketing VP was energized by the prospects of dramatically beating our competitors into new market segments. The software requirements are all documented and coding will begin in April.

It will be very lucrative for us that we got these key players to interact more effectively.

The new product development teams have been favorably impacted by the exposure to the PCT model and your specific role playing scenarios and the discussions we had. Our engineers have a more comfortable willingness to truly get into their "customers world" and view the problem from the other persons perspective. Interactions that used to be strained and difficult are no longer that way.

I want to thank you and Christine for introducing us to a powerful new concept.

Matt Gibbons  
Engineering Administration Manager

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I have just received this testimonial letter from Matt. He got excited about PCT, and we have stayed in touch. When I finally got this report, I was in the middle of struggling with roleplays. It seems obvious to me now that roleplays have only a limited place in the teaching sequence with the audience we address. I believe it is fine for me and Christine to present a roleplay as an illustration. When it comes to participants, it is probably best to discuss our way through the process of "mapping wants and perceptions." That appears to be a good way to label what Matt has used his insights to do.

Best to all, Dag

Date: Fri Mar 11, 1994 4:38 pm PST  
Subject: Listserver performance



<[Bill Leach 940311.09:58 EST(EDT)] >NET

I suspect that this will not be particularly comforting to Tom, but I notice that the turnaround time that I experience with CSG-L is so short that quite often the server will have received, processed and returned the first posting of mine in a session before I can finish posting the other messages in a session.

Such is not always the case of course, but it is very rare that if I log in a couple of hours after posting that all of the messages from the previous session will have been processed.

-bill

Date: Fri Mar 11, 1994 7:01 pm PST  
Subject: Re: Conventional wisdom, article

<[Bill Leach 940311.17:37 EST(EDT)] >Tom Bourbon [940310.1242]

EXPAND MY READING LIST!?! If I expand my reading list any more, I'm going to have to buy a warehouse for just the pending works.

Seriously though, I will save your message but on this system with some 200,000 files I may not be able to find it when it comes time to consider additional PCT material, thus don't be shy when I ask in the future.

-bill

Date: Fri Mar 11, 1994 8:23 pm PST  
Subject: Re: Embarrassment, Testimonial

<[Bill Leach 940311.22:17 EST(EDT)] >[Dag Forssell (940311 1200)]

He might say that he is immune but I am quite certain that the opinions of those of you for which he has developed a deep trust and respect through many years of interaction are important (as they should be).

That letter from Matt is outstanding. Can you "use" it?

-bill

Date: Fri Mar 11, 1994 8:23 pm PST  
Subject: Re: PCT Police Action

<[Bill Leach 940311.18:03 EST(EDT)] >[Rick Marken (940311.1030)]

Rick; let make sure that the are NO errors in perception here on this matter. I said "PCT Policeman" (or however I phrased it) because I thought it was "cute". You originally proposed the idea (I think) and I got quite a chuckle out of it.

"Yes, indeedy, no bout a doubt it!" You are a purist, sometimes a vague purist but a purist just the same.

All that might bother me but you are also persistent, so if someone continues, you will keep trying to present your point in different ways and eventually either because of something you say or a comment from someone else will "clear things up."

While I'll admit that I have "cringed" a couple of times when reading your postings, usually to someone else (fortunately for me), I don't take your methodology as demeaning. Besides I actually enjoy your approach most of the time (even when I have been the 'target').

As I tried to say before... If there is any one thing that attracts me to PCT, it is precisely the refreshing devotion to correspondance with reality that is present in PCT research.

And this (roughly):

"What a novel concept, these nuts actually believe that if the model and reality don't match that it is the model that need work!" <dripping with sarcasm>

-bill

Date: Sat Mar 12, 1994 1:21 pm PST  
From: Bill Silvert MBX: bill@biome.bio.ns.ca  
TO: \* Purposeful Leadership / MCI ID: 474-2580  
Subject: Re: Server information for CSG

I haven't had a chance to do anything with the disk you sent, and probably won't for a week or two. I've been in Europe and just got back. If you want to modify the document, that is fine.

In general it is much easier for me to install software that is uploaded to biome. With the disk I have to do all the necessary transfers and don't have much spare time for that. But I'll get to it when I can.

Bill

Bill Silvert at the Bedford Institute of Oceanography  
P. O. Box 1006, Dartmouth, Nova Scotia, CANADA B2Y 4A2  
InterNet Address: silvert@biome.bio.ns.ca  
(the address bill@biome.bio.ns.ca is only for mailing lists)

Date: Sun Mar 13, 1994 5:23 am PST  
Subject: More Nightingale...

<[Bill Leach 940312.19:08 EST(EDT)] >NET

Interesting how close:

"Focus on one objective at a time. Like a servo-mechanism, the brain set on a target, will call into play those mental processes that will bring your efforts to

fruition; your action will conform to your expectations thereby bring about the event."

-bill

Date: Sun Mar 13, 1994 3:46 pm PST

Subject: Owed to a Nightengale

[From Rick Marken (940313.1530)]

Bill Leach (940312.19:08 EST) --

>Interesting how close [to PCT?]:

>"Focus on one objective at a time. Like a servo-mechanism,  
>the brain set on a target, will call into play those mental  
> processes that will bring your efforts to fruition; your  
> action will conform to your expectations thereby bring  
> about the event."

I believe that Maxwell Maltz said the same kind of thing in "Psychocybernetics". I have tried to see Nightengale's comments as reflecting a perspective on behavior that is close to PCT. But I run into problems all over the place. The idea of "focusing" on one objective seems nice, for example. But what is being "focussed"? I see two possibilities -- consciousness or control.

If consciousness is what Nightengale wants focused, then the rest of the paragraph is misleading because it suggests that consciousness is necessary for control (ie. for "calling into play" the outputs that produce the intended result). In fact, consciousness of the objective is not necessary in order for the objective to be achieved (indeed, consciousness can get in the way; just ask any zen master).

If, on the other hand, it is not control that is somehow being focussed then the statement is wrong for another reason -- it implies that you should only control one perception (achieve one objective) at a time. In fact, we **MUST ALWAYS** be achieving hundreds of objectives simultaneously; objectives about muscle tensions, body position, movements, sequences of results, programs of action, principles, systems concepts, etc. Control systems **MUST** work together to achieve all their objectives at the same time. If one tries to achieve one objective "no matter what" then there will almost certainly be massive conflict -- mainly with the operation of other control systems inside oneself.

For example, Nightingale implies that if I focus on one objective, like making tons of money, I will end up doing (or thinking) all the right things to achieve this objective. But suppose that one of those things involves producing a level of a perception that conflicts with something else I am controlling. For example, I might focus and find that I can make tons of money by selling real estate in Orange County, IF I sell to everyone except blacks, jews and gays. I want lots of money but I want fairness too. If I do what my "focus" says will work to achieve the "lots of money" objective I end up "un-achieving" my objective of treating people fairly (a principle level perception). My money and fairness goals are in conflict.

What Nightingale does get right is the "automaticity" of control. That is, he is right to say that a properly functioning control system, "will call into play those mental processes that will bring your efforts to fruition; your action ["result" in PCT] will conform to your expectations [reference] and thereby bring about the event." But this only works if you already have built-up (learned) a control system that controls for the perception that the objective (reference signal) specifies. When I have learned how to control the cursor in a tracking task, a higher order system in me can specify a particular reference distance between target and cursor and expect the "tracking" control system to generate exactly the outputs needed to continuously achieve that objective. However, if I have not learned how to control a perception (like amount of money at my disposal) then I can "focus" on the objective of being rich all I want (set a reference for perceiving lots of money) and, Nightingale notwithstanding, my actions will NOT produce the intended result, except by luck. (Indeed, my objective may concern perceptions that are uncontrollable IN PRINCIPLE, such as the perception of the way another person behaves).

The most "PCT-like" interpretation I can give to Nightengale ends up with him telling us what we don't need to know, namely, that if we know how to achieve our objective (we have an achievable [controllable] objective, we know how to achieve [control] it and we have no internal conflicts about it) then we can achieve it. Thank you, Mr. Nightengale.

I have a feeling that you are hoping to find in the "sayings" of pre- or non- PCT students of human nature something that smacks of an inkling of the wisdom that comes out of the functioning PCT model. I think it's a fools errand -- and I know that you're no fool. It seems to me to be like looking in the Bible for precursors to scientific understanding of relativity physics or evolutionary biology.

Trust me (even though Martin says not to); nobody got it before William T. Powers did; not even close.

Best Rick

Date: Sun Mar 13, 1994 4:47 pm PST  
Subject: Re: Owed to a Nightengale

<[Bill Leach 940313.18:50 EST(EDT)] >[Rick Marken (940313.1530)]

>fool's errand

No, I agree, that searching for some evidence of a "real" understanding of PCT on the part of such persons is futile and indeed that is not my "take" at all.

What I believe is that there is a great deal of real wisdom out there, mixed in with all the garbage, half-truths, un-truths and the like.

It is my personal perception that Nightingale "had it together" quite well (maybe not as well as Carnegie but well).

I also think that it is invalid to just use excerpts such I have used to conclude "the right approach" and that your objections as to "correctness" are just as wrong for the same reason (though I specifically DO NOT object to your analysis).

No, I believe that Nightingale fully believed that the most important things that a person can do is to try to; determine just what IS meaningful to themselves, relate that to obligations and the like, form consistant goals, and then go out and LIVE.

I think that he is like many others in the motivational and other fields in that he fully recognized that you first have to "get someone's attention" before you will have much chance to get them to think.

Quite obviously, I have my own perceptions of what he was trying to say and recognize that I could well be a long way "off base" as to his intentions (or any of the others for that matter).

It seems to me that most of these people seemed to recognize that one had to "be at peace" with oneself to be "successful." Indeed, more than one motivational speaker has maintained that this vague condition of "being at peace" IS success. Though not often delinated specifically, they also seem to recognize that there is a vast difference between caused by "internal conflict" and that caused by "external conflict."

Again, it is not my contention that these people "knew" (know) anything about PCT or even necessarily have any understanding of the concepts at all but rather that they seem to be successful at helping people and that I believe that the reason for this success, at least in part, is that they are (consciously or otherwise):

helping people to become consistant in their beliefs (lower magnitude and frequency of internal conflict).

helping people to process their high level perceptions concerning other people in a more realistic and rational fashion (lower the magnitude and frequence of externally induced conflicts).

and though related to the previous, help people process their own perceptions of themselves in a fashion more realistic with both reality and their potential ability.

I think that Earl Nightingale's favorite expression was: "You become what you think about most." Now this is both ridiculous and profound.

>The most "PCT-like" interpretation I can give to Nightengale  
>ends up with him telling us what we don't need to know, namely,  
>that if we know how to achieve our objective (we have an achievable  
>[controllable] objective, we know how to achieve [control] it and  
>we have no internal conflicts about it) then we can achieve  
>it. Thank you, Mr. Nightengale.

If one trys (as I no doubt was trying a month ago or so to do) to make the work of such people "PCT" then yes, his advice is "goofy" at best. No, I think that I fully recognize that the only PCT is taking place fully within the confines of those that fully recognize PCT.

My interest in such things is that I fully expect PCT to explain what is and is not right about the approach taken by these folks. It is one think to even convince someone that "internal conflict" causes a person to be less effective than they could be and somewhat unhappy... at best and possibly serious

psychological problems at worst, and an altogether different matter to work them through the changes necessary to a "healthy" mental state.

As far as people that desire to help someone else it is, I think, necessary to have a diverse repitore of anecdotes, ideas, methods, analogies and the like to bring to bare upon the problems. However, it also seems to me that knowledge of PCT IS what can help to make the "right" choices in guiding the "someone else."

<chuckle> Yes Rick, I trust you <grin>.

-bill

Date: Mon Mar 14, 1994 10:47 am PST  
Subject: Re: Representational momentum

[From Richard Thurman (940314.0830)]

Rick Marken (940310.1430) Rick --

Thanks for the ideas on representational momentum. Although its out of my field and I know very little about the phenomenon, I keep having recurring thoughts about how it may be a useful way explore the dynamics of control systems within an established field of study. Also I keep having the feeling that I should understand what is going on here. But for some reason the PCT-based explanation is just out of reach -- mentally. Its like one of those tip-of-the-tongue experiences.

>It looks like a very interesting way to study memory -- especially if it  
>were set up as a continuous control experiment (rather than the way it  
>seems to be typically done, where the subject must chose from a distinct  
>set of orientations).

I hadn't been thinking of it as a memory task. At least not in its classical sense. It seems to me that it has more to do with the dynamics of lower level control systems that take a while for a reference signal to get to them.

For example, suppose you have a tracking task where the you are chasing a moving target around the (computer) screen with the mouse. At some point the target stops moving, but for a brief instant you keep the mouse moving in the same direction. At some point (10, 20, 100 milliseconds?) you stop moving the mouse. But it will be further down some path than the target.

This 'momentum' resides entirely within the lower levels of the hierarchy doesn't it. That is, its not just a matter of the momentum of a mass (the hand and arm moving and then trying to come to rest.) It seems to be a matter of perceptual 'momentum.' The higher levels in the hierarchy need to supply a new reference signal to the lower ones. This takes time. Thus we see the mouse still moving while the target has stopped.

Could it be that the old reference signals are somehow stored and compared with the new ones? Or could it be that when the computer animation is 'turned off' it simply leaves the old reference signals in at the same level they were before. (Actually, they would be at a level that was slightly beyond where they should have ended -- because of the dynamics of hierarchal control.) Then when a subject

is asked to place the figure at its last known position he places it slightly beyond where he should because the last reference signals are still stored and it just 'feels right' to do so.

If the latter idea were true, it would mean that 'memory' can reside below the brain stem!

I will conclude this speculation with some quotes from Kelly & Freyd (1987) Explanations of Representational Momentum. Cognitive Psychology 19, 369-401.

>Figures that have undergone an implied rotation are remembered as being >slightly beyond their final position. This phenomenon has been termed >'representational momentum' because of the possibility that it reflects the >internalization in perceptual systems of the principles of physical >momentum. . . . The demand characteristic is such that subjects in imagery >experiments often believe they are to create a mental image that duplicates >laws of motion found in the environment. The subjects in such tasks are >instructed to transform a mental image, and perhaps take this to mean a >'natural' transformation. As a result, imagery processes appear to mirror >physical processes, but not because of any intrinsic coupling between the >two. However, in momentum experiments, the subject's task is to resist >transformations of the mental image. In particular, this would require a >decoupling of imagery processes with physical processes. Yet subjects >simply are unable to implement this decoupling. That is, representational >momentum appears to be mandatory and inaccessible to beliefs, desires, >and expectations. . . . In addition, representational momentum occurs very >rapidly. Representational distortions are observed within 20 ms after the >third figure is removed. it is hard to imagine that such rapid distortions >are influenced by beliefs or expectations. the distortions appear to be >more reflexive than thoughtful. . . . If momentum is indeed part of a >modular system, and if its shared aspects with imagery phenomena reflect >more than a spurious correlation, then at least one aspect of mental imagery >would have been shown to be of an intrinsically analog nature.

Richard Thurman

Date: Mon Mar 14, 1994 5:27 pm PST  
Subject: Second tries

From Tom Bourbon [940313.1309]

Sometimes you win (the mail goes through), sometimes you don't (it doesn't). Here are two second tries on some things that didn't get out of the que on our local server on Friday.

Replies to Richard Thurman; Rick Marken & Martin Taylor

=====

Subject: RE: PCT and representational momentum

From Tom Bourbon [940310.1512]

>[From Richard Thurman (940310.1130)]

>

>Bill, Rick, Tom and other PCT researchers.

>

. . .

>The question I have is on 'representational momentum.' That is,  
>figures displayed on a computer screen that have undergone a  
>rotation or translation are remembered as being slightly beyond  
>their final position. For example, in a standard representational  
>momentum study, subjects view successively (say on a computer  
>screen) three rectangles of identical dimensions that are oriented  
>(say) 15 degrees apart. The shift in orientation suggests a clockwise  
>(or counterclockwise) rotation of a single rectangle. Kind of a  
>crude animation effect. After the third figure is removed, a fourth  
>rectangle is shown that may be in the same orientation as the third  
>or it may be slightly forward or backward relative to the implied  
>rotation. A subject is supposed to judge whether the fourth rectangle  
>is in the same orientation as the third.

. . .

>What is the PCT explanation for this pattern of behavior? What  
>perception(s) can we postulate the individual is controlling? How  
>would one go about explaining this phenomenon? How would one  
>test for the controlled variables guessed at above? How would one  
>perform the kind of research that PCTers would like to see published.

I only have a minute, right now, so this is not a full reply. My first comment is that you have described what seems to be a perceptual effect similar in certain ways to many other "aftereffects" of perceptions -- reversed rotations of the visual field after you stop spinning, color and brightness afterimages, and so on. These probably reflect activity within the perceptual functions (input functions) for the different perceptions, rather than controlled perceptions. PCT tests, then, would not necessarily be aimed at the phenomenon itself as a controlled perception (although it might be one).

It looks to me, after your brief description, as though the experimenters were already doing something somewhat close to what we call "the test for a controlled variable." They put the fourth rectangle at various positions and ask the person whether it is in the same position as the third one. A more direct test might be to put the fourth rectangle at various positions and ask the person to put it, and keep it, where it belongs by moving a control manipulandum. Certain positions for the fourth rectangle should not disturb the person's reference for position, others would. I would expect the aftereffect to "fade" or diminish in magnitude across time, so the person's placement of the fourth rectangle ought to change across time as the aftereffect changes.

>Is it the same phenomenon that  
>happens when you ask someone to track your finger as you make a circle  
>in the air and then stop moving your finger. (One can see slight  
>overshoots there as well.) Does it just take a while for the loop to  
>'wind down?' And if so ..... well you get the idea.

In this example, the person is actively tracking and higher levels in his or her system have longer time constants than lower ones --- you've seen that line of reasoning before, as your passage reveals.



I must run. I'll try to post more thoughts on this later.

Until then

Tom

=====

Subject: RE: Derivatives and Clarifications

From Tom Bourbon [940311.0915]

>[From Rick Marken (940310.2130)]

>

>Tom Bourbon (940310.0840) --

>

>Excellent post Tom. But I have one little concern about it. You  
>quoted Martin Talyor 's (940301 12:00) fateful claim that:

>

>>the VALUE of the

>>disturbance is more or less independent of the VALUE of the perceptual

>>signal, as we all know. But the derivatives are not independent, at

>>least not for a correct value of the time delay associated with the

>>perceptual input function.

>

>and then correctly describe the subsequent events:

>

>>a presentation by Bill of the results when he performed the

>>calculations described by Martin.

>

>>a radical discrepancy between Bill's results of the computations

>>and Martin's prediction of the results

>

>You then conclude:

>

>>This exchange marked the first direct attempt, on csg-1, to apply IT

>>computations, after Shannon, to data on human control behavior. If

>>Bill and Rick (and I) correctly understood Martin's prediction, then

>>he prediction failed. However, if we misunderstood your prediction,

>>Martin, could you state it more clearly so that this important

>>quantitative testing of predictions against data can go forward?

>>This is the proper way to address the questions of "information in

>>perception" and of the role(s), if any, of information theory in PCT.

>

>I completely agree with your last sentence. But I am worried by the

>request that Martin clarify his prediction if we misunderstood it. I can't

>imagine a clearer claim (or prediction) than the one that Martin made

>above ("But the derivatives [of perceptual signal and disturbance] are

>not independent"). When we showed a plot indicating that these

>derivatives ARE independent, Martin said that this result was

>EXPECTED. So I can only presume that Martin's "clarification" of

>the prediction of non-independence of the derivatives is that they ARE

>independent. Is that clear?

Like you, Rick, I have a hard time imagining what else Martin might have meant in his original post. Martin, I wasn't asking for a lengthy or elaborate discussion

when I said: " if we misunderstood your prediction, Martin, could you state it more clearly so that this important quantitative testing of predictions against data can go forward?"

It looked to Bill, Rick and me (reading and acting independently of one another) as though you were saying the derivatives would be dependent. When Bill showed, on a first analysis, that they were independent, you said you had predicted (expected) independence, not dependence.

All I asked, Martin, was that you clarify the situation for us. Did you expect independence and if so, where did we go wrong in our readings of your seemingly clear prediction-expectation? If we missed the point entirely, can you tell us, briefly and concisely, the general error of our ideas? That's all I'm asking now.

>I am afraid that this "clarification" process could go on forever; Martin  
>makes a prediction, we ask if that's what he really means, he says "yes"  
>(usually in a private post, as he did to me when I asked if he really  
>meant to say that the derivatives are not independent), we collect the  
>data, it doesn't fit the agreed on prediction, we think we have falsified  
>the prediction but then Martin wants to "clarify" what he really meant to say.

This is certainly the way things look, Martin. And, like Rick, I am disappointed that you chose to reply to me in private, rather than on the net where I believe these discussions should be conducted. The discussion of IT-PCT had reached a point where specific predictions-expectations were subjected to quantitative testing. The results of the first attempt at a test looked unambiguous. We all should be able to share in a discussion of what happened, or did not happen, and move on from there.

If, instead, each of us goes private whenever we make a mistake, or whenever someone else thinks we made a mistake, this net will become an unexciting corner of cyberspace.

Later, Tom  
=====

Maybe this time. Tom

Date: Mon Mar 14, 1994 5:50 pm PST  
Subject: Musings and questions

<[Bill Leach 940314.18:34 EST(EDT)] >NET

A computer engineer friend and I were talking over lunch today and mentioned something that he experienced while working on a "computer vision" system for a quality control application (this was quite a number of years ago).

He mentioned that originally a laser range-finder was used to "map" the "contour" of the foil side of a stuffed but unsoldered printed circuit board with the objective of finding improper "part stuffing" prior to wave soldering. This is definately a worthwhile project as it is much more difficult and costly to attempt to correct an error after the board has been soldered.

For his own purposes, he rendered the resulting map as a plane surface using pixel intensity to indicate displacement from the reference (a level at or slightly below the surface of the board substrate). Thus, the "traces" would be a very dim gray and leads would range from black (inside a hole in the board) to white when at the maximum extension.

He noted that he personally had difficulty when examining the image to determine the existence of problems (basically missing or too short a lead).

Out of curiosity, he rendered the image as a histogram in three dimensions and tilted the image 45 degrees. Upon examining the new image, he noted that finding "problems" was even more difficult. However, he tried one more "trick" and that was to rotate the image and to his surprise, defects "seemed to jump out at him."

He concluded that although there was no additional information present in the rotation than existed in the "still" view, the "mind" is able to detect relationships in moving objects faster than still images (roughly stated).

I am sure that there must be actual research work in this area but I sure have not kept up myself (other than to pick up on some of the other "neat" computer graphics tricks like letting the mind "fill-in" missing information to reduce data bandwidth).

-bill

Date: Mon Mar 14, 1994 7:47 pm PST  
Subject: PCT-ish ideas

[From Bill Powers (940314.1800 MST)] Back from Boulder, 19 messages behind.

RE: PCT-ish ideas.

The concept of other approaches which seem to have anticipated or at least to have used PCT comes up frequently. It shows that the actual nature of PCT has not yet been appreciated.

PCT is a theory of behavior, an explanation of how behavior works at many levels from motor behavior to cognitive behavior. It proposes that the focus of behavior is on controlling perceptions; making the perceived world match what is desired, with higher levels telling lower ones what states of their perceptions to desire. It shows how the properties of a negative feedback loop can be used to explain behavior.

The visible actions involved in this process are mostly misleading, because the point is not to control actions, but to control effects that the actions produce in the world. Behind the theory is a fairly solid science of control systems, so we can, or can hope to, model specific behaviors using control theory.

Out of this theory come many predictions and expectations concerning behavior. The test of the theory, of course, is whether these predictions and expectations fit what we actually observe, or predict how behavior will actually change when we change conditions. At one level of testing, we use informal observations of ordinary behavior, both our own and that of others. PCT helps us to notice things about behavior that need explaining, for example the ability of a person to choose

a goal and then produce actions on the world that will alter the world of perception until it matches the goal. That is one way to talk about how negative feedback control systems work.

So we are concerned with two things: the kinds of behavior we actually see people creating, and the model that explains what we see. The model not only explains what we see, but often tells us that we need to look at behavior in a different way. For instance, when we see the driver of a car turn the wheel to the right when a gust of wind arises from the right, we might see a cause-effect relationship, as if the wind is causing the driver to turn the wheel to the right. This might lead us into a fruitless search for the driver's wind-sensors, or for effects produced by the wind that provide "subtle cues" to the driver. We might start keeping records of wind velocities and directions, and the driver's efforts applied to the steering wheel in different amounts and directions, and conduct statistical studies to try to isolate the factors that have the most influence on the driver's responses to the wind.

While PCT doesn't say that one shouldn't take such an approach, it does provide a much simpler explanation for what we see. It says, in effect, that we should look for some variable that the driver can perceive which is affected in one direction by the wind, and in the opposite direction by the driver's "responses." This variable turns out to be the car's lateral position on the road. So given the same observations, we come up with a different conception of what is happening: it is the driver's intention to keep the car in the certain position, and given the PCT model, this gives us a complete explanation of the relationship between the driver's actions and the effects of the wind. We can represent the driver as a negative feedback control system, identifying the parts of the system with observable aspects of the situation, and apply control theory to show that the modeled system behaves like the driver.

But PCT doesn't predict that drivers will keep cars on the road, or that drivers have intentions regarding the position of the car. It doesn't predict that winds (or collisions with ostriches) will disturb the path of the car, or that the driver's actions (or another collision with a emu) will keep the car on the road. It doesn't explain things that are well-known and easily observable by anyone whether or not they know PCT.

Anyone could say that drivers know where they want the car to be and act so as to keep it there. Anyone could say, even, that the driver's behavior is goal-directed, and that the driver has to have an image of where the car should be in order to steer it. Anyone could observe that the driver is acting purposively. All those observations could be made without knowing anything about PCT. All that making such observations shows is that the person making them is not in the grip of a theory that declares such phenomena to be nonexistent or meaningless. It does not show that the person understands anything about PCT.

People have noticed throughout history that behavior is purposive. They have noticed that people have goals and control things, and through introspection have noticed that controlling things or pursuing goals involves mental images of how things are to be, and that actions must be directed to make the way things are change toward the internally-specified goal state. These are not theories, but observations. Simply making similar observations doesn't require PCT.

Where you need PCT is to explain how such things could possibly occur. What is a mental image or a goal, such that it could direct physical actions on a physical

world to create its own fulfillment? How can a goal that does not yet exist in reality be brought into existence by any action? Why does this not require the future to affect the present? How can an action be said to be intentional when the intended result doesn't actually occur? What directs actions so they produce intended results even when the environment changes?

These are the kinds of questions that PCT answers. They are not questions about what happens. They are questions about how what happens could possibly happen in a physical or neural system. PCT is about HOW IT WORKS.

Tom Bourbon (940310.1022) said

>PCT is about how our own behavior (behavior as actions that  
>affect the environment) is \*out of our control\*.

That's not quite right. PCT is a theory from which we can deduce that actions are not controlled. The statement that actions are not controlled is a prediction of the theory, not the essence of the theory. We can easily verify this prediction by looking at real examples of control behavior, and showing that if the action were controlled to follow any preselected course through time, the variable it is supposed to control couldn't be controlled. If the driver selected a preferred way of moving the steering wheel and then succeeded in moving it in that pattern, the car would soon go off the road. So we can show that the prediction holds up in real experience.

But the theory doesn't say that: the theory says that the output quantity is a function of the difference between a reference signal and a perceptual signal, and so forth, which reduces to a set of system equations that we can solve for the various variables. When we do so, we find that the action depends primarily on disturbances, not on the intended state of the controlled variable. Indeed, if the action is selected in advance, it becomes an independent variable, and solving the equations under that condition shows that no control will happen. From that result, we can generate a verbal statement to the effect that action is not controlled by a control system, and find the required mapping of the model onto specific behaviors that will show that this is, indeed, the result.

So when a sports psychologist tells an athlete to form a mental image of a particular aspect of performance and try to make it as clear as possible, is this psychologist "doing PCT?" Not at all. The psychologist is just speaking from experience: if you generate a clear mental image, you will get better results. You can easily find out if the psychologist understands anything about PCT: just say "Oh, is that so? Why does that give better results? How does that work?"

If you read something that sounds "like PCT," you can be almost certain that it sounds like PCT only because it describes the sort of phenomenon that PCT explains. People have always noticed this sort of phenomenon; it's only science that has ignored it. PCT allows us to talk about behavior in ordinary colloquial terms, because it explains behavior as it actually occurs in nature. Talking in those terms shows that the speaker has not been brainwashed by scientific theories which claim that these phenomena don't exist, or which convert them into some other sort of phenomenon that fits the theory better. It shows, perhaps, that the speaker might be able to grasp PCT more easily than someone who doesn't think those things happen. But just talking about goals, perceptions, actions, intentions, desires, and so forth is no sign that the speaker understands HOW

these things work as they do. Being able to explain HOW is the critical test for understanding PCT.

Best, Bill P.

Date: Mon Mar 14, 1994 8:37 pm PST  
Subject: Re: PCT-ish ideas

<[Bill Leach 940314.22:43 EST(EDT)] >[Bill Powers (940314.1800 MST)]

Thanks for yet another nice clear presentation to clear up misunderstandings. Also, quite timely and I have just started discussing PCT with an engineer friend that will probably be interested and that last message looks like a real good start.

-bill

Date: Tue Mar 15, 1994 9:47 am PST  
Subject: Improved fault detection with rotation

[From Rick Marken (940315.0815)] Bill Leach (940314.18:34 EST)

>he tried one more "trick" and that was to rotate the image and  
>to his surprise, defects "seemed to jump out at him."

>He concluded that although there was no additional information present in  
>the rotation than existed in the "still" view, the "mind" is able to  
>detect relationships in moving objects faster than still images (roughly  
>stated).

>I am sure that there must be actual research work in this area but I sure  
>have not kept up myself (other than to pick up on some of the other  
>"neat" computer graphics tricks like letting the mind "fill-in" missing  
>information to reduce data bandwidth).

Great post Bill!! This is directly relevant to some of the work being done here at Aerospace on ways to represent satellite state of health telemetry so that anomolous conditions can be more easily detected by human operators. I know that there is research on dynamic data display and I should know about it (after all, I am trained as a perceptual psychologist) but I'm getting old and slow (though I'm still very cute) and, I'm afraid, I'm unable to keep up with everything that I should. But your anecdote is an EXCELLENT start; it's a nice, concrete example of how a particular kind of anomaly, which is invisible in a static data display, becomes obvious in a dynamic display of the SAME data. Maybe you could give me some more details (off line) of exactly what your friend was looking for and how the dynamic display was implemented.

[Though it only contains a paragraph or two on dynamic data displays, here is a book on graphic data perception that might be of interest:

Cleveland, W. S. (1985) The elements of graphing data, (Monterey, CA: Wadsworth)

Interestingly, it was a division of Wadsworth that published my textbook "Methods in experimental psychology" (1981), my last contribution to conventional psychology.]

From a PCT perspective, the dynamic display might "work" better than the static display because your friend had a reference for the "right" state of a particular transition perception but not for the "right" state of a particular static configuration perception (perhaps there is no particular reference for the "right" state of a static configuration; your friend must have known how some aspect of the dynamic display SHOULD look if there were no "part stuffing"). What "jumped out" at your friend was a perception of discrepancy between the self generated IMAGINED reference perception and the actual perception of the dynamic display (this discrepancy being perceived by a system at a higher level in your friend's brain that which perceives the actual dynamic transition).

As Tom Bourbon (940313.1309) said in his reply to Richard Thurman about "representational momentum" the important aspect of the explanation of the dynamic data display advantage is in terms of what is perceived; the most difficult aspect of modelling this phenomenon (as with the "representational momentum" phenomenon) would be modelling the perceptual input function that produces the transition perception signal that is compared to the imagined reference perception.

Indeed, I would say that, in general, PCT moves the emphasis of explanations of behavior from figuring out schemes for generating actions to figuring out schemes for generating perceptual signals. PCT shows that there is no need (and no feasible way) to devise action schemes that will produce consistent results in an inconsistent environment; the problem of understanding (and modelling) behavior is to figure out how organisms represent (perceive) those aspects of its own sensory input that are influenced by its actions.

Best Rick

Date: Tue Mar 15, 1994 11:39 am PST  
Subject: Re: representational momentum

[From Richard Thurman (940315.1030)] Tom Bourbon(940313.1309)

Thanks for the input on representational momentum:

> My first

>comment is that you have described what seems to be a perceptual effect  
>similar in certain ways to many other "aftereffects" of perceptions --  
>reversed rotations of the visual field after you stop spinning, color and  
>brightness afterimages, and so on. These probably reflect activity within  
>the perceptual functions (input functions) for the different perceptions,  
>rather than controlled perceptions. PCT tests, then, would not necessarily  
>be aimed at the phenomenon itself as a controlled perception (although it  
>might be one).

I had been wondering the same thing. Except that I don't think that it reflects a perceptual aftereffect. It seems to me that this may be an aftereffect due to the dynamics of closed loop control systems. For example, this could be a simple case of what happens when a hierarchy of control loops no longer receive updated reference signals (or something like that).

The thing that's bugging me is that the current explanation for this phenomenon is so bizarre. Basically the best researchers in the field in this area are saying that this effect happens because the human brain has evolved in such a way that the most enduring characteristics in the environment (laws of motion, momentum, etc..) have been internalized in perceptual systems during the course of evolution. And as a consequence perceptual processes would be expected to resemble corresponding physical processes. For example here is a quote. 'Given the pervasive tendency for a moving object to continue along an established direction, the visual system may simulate momentum in its operations. On detecting a moving object, the visual system might automatically calculate future positions of the object based on a perceived trajectory. This ability might play an important role in a number of activities, such as anticipatory reaching and avoidance of projectiles.'

Instead of trying such speculations, wouldn't it be better to create a model based upon PCT and show that the phenomenon is nothing more than the side effects of a set of control systems that were abruptly 'turned off.'

>It looks to me, after your brief description, as though the experimenters  
>were already doing something somewhat close to what we call "the test for a  
>controlled variable." They put the fourth rectangle at various positions  
>and ask the person whether it is in the same position as the third one.

Yes, they are doing about 1/4 of the test for the controlled variable. I guess I am interested in seeing if they can be induced to apply the rest of the test and get to work on the real phenomenon. For example, if I create a hierarchal model that shows the same effects (say above .98 correlations) as human subjects display. And then show that the result is simply a side effect of hierarchal perceptual control. I would then want to induce them to perform a line of research more like what you describe below.

>A more direct test might be to put the fourth rectangle at various positions  
>and ask the person to put it, and keep it, where it belongs by moving a  
>control manipulandum. Certain positions for the fourth rectangle should not  
>disturb the person's reference for position, others would. I would expect  
>the aftereffect to "fade" or diminish in magnitude across time, so the  
>person's placement of the fourth rectangle ought to change across time as  
>the aftereffect changes.

I agree that this would be a more appropriate way to research representational momentum. But first I need to get a grip on how and why there is such an 'aftereffect' and the mechanism underlying its fading.

Thanks for the response.

Richard Thurman

Date: Tue Mar 15, 1994 3:06 pm PST  
Subject: reorganization cannot be random

[Hans Blom, 940315]

Title: Why reorganization cannot be random in complex organisms



A popular notion in PCT is that learning (reorganization) is random. A popular demonstration of "random learning" is shown in what is called "E. coli"-behavior, where random changes of direction, if applied at appropriate times, can bring a simulated coli bacterium to its goal.

This short note is an attempt to show that in more complex organisms randomness is not a sufficient (i.e. good-enough) mechanism for learning.

Assume the following (in PCT circles well-known) demonstration. A "coli", a spot on the computer's screen, follows a trajectory of short steps that form a straight path until a keyboard key is pressed. Then a random change of direction takes place, after which another straight path is followed in small steps until the next key press. Ad infinitum. If the key is pressed at the appropriate moments, a demonstration shows that coli will approach and remain in the vicinity of its goal, another spot on the computer's screen.

Note, that each time a step in a new direction is chosen, the probability that it takes coli AWAY FROM its goal is slightly larger than the chance that it gets closer to its goal. In order to see this, assume that the goal's position is at coordinates (0, 0) of the x-y plane. Call the size of the steps  $r$ . When coli is at a position at a distance  $R$  from its goal, a step in a new random direction takes it to a new position that is at least  $R-r$  and at most  $R+r$  from its goal. In fact, the new position will be somewhere on a (small) circle with radius  $r$  around its previous position. The length of the (small) circle segment OUTSIDE the circle with radius  $R$  is always larger than the length of the (small) circle segment INSIDE the circle with radius  $R$ . The new position after the first step in the new random direction is either nearer to the goal, that is within a circular segment bounded by circles at  $R-r$  and  $R$  with (0, 0) as a center, or farther from the goal, that is within a circular segment bounded by circles at  $R$  and  $R+r$  with again (0, 0) as a center. The chance to find coli farther away (in the outer segment) compared to the chance to find it nearer to the goal (in the inner segment) is equal to the ratio between the surfaces of those segments, which is  $((R+r)^2 - R^2) / (R^2 - (R-r)^2)$ , i.e. again (usually slightly) larger than 1. It is assumed that the organism's sensory capabilities are limited, and that a finite  $r$  ( $r > 0$ ) is in fact required before it can discover whether the new direction improves matters (leads towards the goal) or not.

Now thus far our simulated coli lived in a two-dimensional world, in which only two directional parameters play a role. In a higher-dimensional world the situation changes: if the dimension is  $N$ , the ratio between the  $N$ -dimensional volumes of the segments becomes  $((R+r)^N - R^N) / (R^N - (R-r)^N)$ , a number which tends to infinity for any finite  $R$  and  $r$  and increasing  $N$ . In mathematics, a similar result is known: the volume of an  $N$ -dimensional sphere is, for increasing  $N$ , ever more concentrated in its surface layer.

This means, in the practice of a complex organism whose behavior depends on a correct-enough setting of  $N$  "learning parameters", that any random change of its parameters will have a very high likelihood of taking the organism AWAY FROM its goal, i.e. making its behavior WORSE. Discovery of this fact by the organism after travelling a finite distance into this new (worse) direction does not help: a new random parameter change will again have a very high likelihood of taking the organism ever farther away from its goal.

From this short discussion follows a prediction: the more learning parameters need to be tuned for correct-enough behavior, the longer the learning time will take. That is, the more complex the organism, the less appropriate is "random learning" as a mechanism for reorganization. Another, more easily testable prediction: a simulated coli that "lives" in a high dimensional space will not be able to approach its goal in a reasonable time.

Some time ago (I cannot find the reference in my files), Bill Powers mentioned very long learning times for higher dimensional simulations of random reorganization. This note may provide the reason.

Greetings,

Hans

Date: Tue Mar 15, 1994 3:49 pm PST  
Subject: Re: Server information for CSG

The disk received from Dag Forssell is now available as a ZIP archive on the biome server under programs/msdos/forssell.zip -- sorry for the delay.

Bill

--

Bill Silvert at the Bedford Institute of Oceanography  
P. O. Box 1006, Dartmouth, Nova Scotia, CANADA B2Y 4A2  
InterNet Address: silvert@biome.bio.ns.ca  
(the address bill@biome.bio.ns.ca is only for mailing lists)

Date: Tue Mar 15, 1994 9:02 pm PST  
Subject: Re: Improved fault detection with rotation

<[Bill Leach 940315.21:18 EST(EDT)] >[Rick Marken (940315.0815)]

I'm going to be a bit "unscientific" here propose three suggested reasons that I have for why this behaviour occurs (I say occurs plural because I have had similar experiences but do not recall enough detail to make them useful):

The first idea is that the construction of the human "vision system" is such that it is indeed more sensitive to detecting effects due to motion or indeed change of any kind than to just intensity. I believe that the same sort of behaviour exists for sound.

The second idea (which may at least in part be a reason for the first) is that there is a visual "smear" that occurs with motion because of persistence effects of vision.

The third is that while it is true that there actually is no new information provided by the rotation, the same information is presented with a continuously shifting reference. I surmise that this results in a "defect standing out" because a defect will appear to have less relative motion with respect to all adjacent non-defective locations.

>From a PCT perspective, the dynamic display might "work" better than the

>static display because your friend had a reference for the "right" state  
>of a particular transition perception but not for the "right" state of a  
>particular static configuration perception (perhaps there is no ...

I am unable to accept this explanation (as I understand it). In so far as I have personally experienced such things, I rather vividly remember that a significant part of the experience is the "shock" of discovery.

What I can agree with is that possibly the mind is quickly able to both recognize differences in the rotating image and then also recognize that these differences are representative of the subject inquiry.

Of course this is all conjecture on my part and is worth about as much as any other psychological research... except one. :-)

-bill

Date: Tue Mar 15, 1994 9:03 pm PST  
Subject: Re: representational momentum

<[Bill Leach 940315.21:43 EST(EDT)] >[Richard Thurman (940315.1030)]

Your quote starting with: >+Given the pervasive tendency for...

There were some five specific disclaimers to any certainty in that quote.

A thought that I had concerning this that I did not express before, is that it seems to me that "visual persistence" would act in exactly the opposite direction. That is, when the image "disappeared", there is an actual retinal signal that persists for at least several hundredths of a second after the image is gone.

There would be a real "beaut" for PCT to successfully model.

Another "interesting" experiment would be to repeat known PCT experiments using Video Monitors that have been "turned upside down". Basically, I wonder if anyone has questioned the monitor's own effects on motion display? Of course, it might be simpler (or already have been done) if there are display units available that have significantly different screen write speeds.

-bill

Date: Tue Mar 15, 1994 9:13 pm PST  
Subject: Re: reorganization cannot be random

<[Bill Leach 940315.21:57 EST(EDT)] >[Hans Blom, 940315]

Am I misunderstanding something here?

When the organism has no basis for achieving a "goal" it "strikes at random." As I understand the concept, if it continues to "strike at random" it perishes but that THAT is not the way that an organism functions.

In some random attempts, it perceives no improvement in the error condition and will choose another "transfer function" essentially at random (I can envision that this might not be fully random in that the organism may "avoid" transfer functions that are perceived to be similar to the failed one).

Eventually, if the perception is controllable, and the organism survives long enough, one or more "transfer functions" will be found that reduce error.

My take here is that always moving in a random direction is not an attempt at control at all unless there is a comparator function operating that is able to deal with something other than error=zero / error~zero (though I admit that in the special case where a particular type of behaviour will always ultimately result in success a binary comparator would be suitable).

-bill

Date: Tue Mar 15, 1994 9:38 pm PST

Subject: Multidimensional reorganization; representational momentum

[From Bill Powers (940315.2130 MST)] Hans Blom (940315)

Thanks for the interesting post on multidimensional reorganization. Yes, I did find that the convergence times for more than about 20 dimensions became very long -- so long that I didn't wait around to witness the final convergence. Your analysis is an explanation of why.

There's another way to approximate the same prediction without taking into account the curvature properties of hyperspheres. Ignore the curvature, and assume that for each dimension the probability of choosing a better direction is just 50% (the choice in each dimension is simply to increase or decrease the parameter). For 20 dimensions, the random changes have one chance in  $2^{20}$  (about one in a million) of achieving a better direction in all dimensions simultaneously. Of course that is not required; only a little more than half of the changes have to be favorable, reducing the chances to 1 in 1000+. In a parallel system this really isn't so bad, but on a sequential computer it is SLOW.

Random reorganization, however, is very powerful for simultaneously adjusting control in "a few" dimensions, whatever that number is, maybe 5 to 10. This can become an argument in favor of a modular and hierarchical structure of control systems, where the number of dimensions involved in any one control loop, at a given level, is kept small. It is far more likely that such a structure would evolve than a structure in which the entire multidimensional system transfer function had to become organized for internal consistency over the whole system -- and its environment.

It is also possible, or even likely, that through the basic process of random reorganization, there would evolve systematic reorganizing processes. I'm working on one now, an "artificial cerebellum" that uses a very simple and neurologically plausible deconvolution method for stabilizing control systems. I'll present it at the Aberystwyth meeting, with a demo program. I hope to install it in the Little Man in time for the special issue of IJMMS that Martin is editing. This is one case in which a reorganizing system can be given specialized and systematic properties, because it deals only with internal processes and does not have to anticipate any details of the environment in which an organism finds itself.

I am shy of any proposal that involves a great deal of systematic reorganization, first because "systematic" tends to mean "higher-level," and higher-level systems must themselves be constructed, and second because it is too easy to give such systems knowledge about the environment that is not likely to arise through the slow processes of evolution. My rule of thumb is that an evolved higher-level systematic process of reorganization can't take advantage of environmental regularities that last less than 100,000 years.

I think your Subject heading, "reorganization cannot be random," overstates the case considerably. IF the entire system must be reorganized as a single multidimensional unit, then of course you are right. But if reorganization occurs level by level, and in separate contexts involving only a few dimensions at a time, then reorganization at random becomes more feasible. I do not, however, rule out some carefully-selected instances of systematic reorganization.

-----  
Richard Thurman (940315.1030) --

I concur completely with your thoughts on "representational momentum," including your description of the cited explanation as "bizarre." This is most likely a phenomenon of perception, to be explained by the dynamical properties of perceptual functions. If my idea about "transition" perceptions is right, that they are derived from successive configuration signals, then it is impossible for a transition perception to appear and disappear instantaneously. In particular, the instantaneous cessation of physical rotation (itself a rare phenomenon in nature) could not be represented by a neural signal derived as I propose; the neural signal would take time to build up to an asymptotic value, and after the cessation would take time to decline to zero again -- leading to a continued impression of motion for a short time after the actual motion had stopped.

According to that idea, there should be a converse phenomenon: a motion that persists for only a very short time (how short to be determined) should lead to an impression of less motion than actually occurred. This should be testable by experiments similar to those in which representational momentum was found, but using rotations that begin instantaneously and persist only for a few tenths of a second or so before stopping.

This is all worth experimental investigation -- by someone who isn't up to the ears in other experimental investigations.

Best, Bill P.

Date: Tue Mar 15, 1994 11:58 pm PST  
Subject: Finally, a reply on Systems Theory and PCT (winding down?)

[Cliff Joslyn, 940315]

Here are replies to the posts from last week on ST and PCT from Mary, Rick, Bill Leach, and Tom. More apologies for the long delay. No good excuse this time. Hopefully we're drawing this to a close.

>[Mary Powers 940306]  
>  
>I thought your post was terrific.

Thank you! I feel better already. No toes stepped on too heavily, I hope.

>I do wonder, however, whether your colleagues aren't going  
>to view you as some kind of aberrant freak for being interested in PCT -

Our numbers would make it hard to pick me out of the crowd. ST is famous for courting the lunatic fringe. It's great for inclusion, new ideas, and "frame-challenging", but bad for rigor and the signal-noise ratio. This has been noted by many, including Tom. In fact, within ST/Cyb, proponents of traditional entity-relation, state-machine based ST (what I described earlier as the Mesarovic/Klir approach) and first-order cybernetics (causal loop modeling) are viewed as archaic relics. If you can't spin Maturanesque dripping prose, you're passe.

>You may well be the only ST person in the world who sees any value in PCT

I KNOW that's not true, if only because I've convinced some of my colleagues of PCT's value. However, this is still just a few of us.

>what did 19th century  
>medical researchers like Pasteur and Koch and Lister owe to an  
>intellectual heritage that was convinced that disease resulted  
>from an imbalance in the four humors?

Well, PCT may be to PSYCHOLOGY as chemistry is to alchemy, but do you really want to claim that of ST?

>PCTers are like Calvin - they are having a wonderful time with  
>this big tiger, which the rest of the world thinks is a stuffed toy.

Well put. Good luck. Don't get bit.

>[From Rick Marken (940306.1230)]

>

>But if you perceived it as  
>scurrilous, ignorant, and gratuitous then it was (for you).

Hmm, I sense a radical constructivist. Do you admit no objective meaning of "ignorant" (I'll give you "scurrilous")?

>Your heart's in the right place, Cliff. Keep up the good work.

Thank you. It's been, well, exciting.

><[Bill Leach 940306.10:43 EST(EDT)]

>A "scientist" that is unwilling to submit  
>his work to "acid test" of reality is no scientist.

Generally I agree. I'll just remind you, however, that ST has many "aspects", and one is the search for a universal modeling language. To that extent, it is similar to mathematics, and then is, as Bill Powers once suggested, merely an internally consistent theory. If we called "mathematics" by "mathematics theory" or "mathematics science", would the same issues be raised for it? Correspondingly, we

could call ST by "systematics" (already taken), "systemology" (which a colleague of mine is using), or "systemics".

But, to some extent ST does aspire to be an empirical science, and then your criticism holds.

>My own impression of ST is that it generally has less substance than even  
>"the laying on of hands". Naturally this opinion is based largely upon  
>the media popularization of Climate Models, Ozone Models, Population  
>Models and the like.

Now this is a bit involved. You probably know the following completely, but I'm not sure.

First, I am a systems theorist, and not a meteorologist, climatologist, or population biologist. ST is concerned with models IN GENERAL, for example, dynamical models based on sets of interacting differential or difference equations (these are the kind you refer to). But these modelers you cite are essentially doing APPLIED ST. So just as the mathematician is not responsible for a bad bridge design, so ST cannot be held responsible for bad modeling. In particular, every model is highly incomplete, with many sources of error, including bad structure, bad transfer functions, bad input data, high imprecision, high innacuracy, and low tractability.

It is the responsibility of the specialist modeler to fess up to these domain-specific facts, and deal with them as honestly as physical scientists do with uncertainty and error of experimental results, or pollsters with poll margins of error. I have no comment on whether any particular modelers (e.g. NASA climatologists) have or have not adhered to these standards.

That being said, scientific corroboration of models of complex systems is inherently different from traditional scientific reasoning. This is because the pillar of the scientific method, repeatability, does not generally hold with them. We cannot run the US economy back to 1932 and consider what would have happened without the New Deal; or try cutting CO2 emmisions and seeing if it really would help. With such systems, models are the ONLY source of predictive power we can have, however uncertain and innacurate they may be.

Finally on this point, risk analysis is dependent on the DUAL factors of likelihood and cost. One should give as much credence to a high likelihood (highly certain), low cost outcome (it will rain tomorrow) as a low likelihood (highly uncertain), high cost outcome (global warming will flood Manhattan).

>I could easily go on with my "tirade" against ST by adding my assertion  
>that "they" have not only "sold out science" but humanity itself.

Well, then I guess your knowledge of ST is indeed as weak as Rick's. My gosh, we're now not only a Threat to PCT but also an Enemy of the People.

>That many of the Systems Theorists actually  
>believe that "the cause of 'saving the world'" is more important than  
>truth is only an excuse for them as individuals.  
>people with virtually no ethics and morality that became the leading figures of  
>their respective fields by being "Politically Correct" rather than honest.

And exactly who would these people be, now?

>I'll take a Rick Marken anyday.

Take my Rick Marken. . .please! ;->

>he has convinced me that he is dedicated to objective science

Have I of myself, as well?

>Until ST can become

>"respectable" with rational people instead of "popular" with politician,

>PCT would be well advised to maintain its distance.

Hey, someone phone my agent! ST is popular with politicians! And after all these years I thought the term "unintended consequences" had barely penetrated political language. . .

Honestly, Bill, your perception of what ST is is so far removed from my own that I find it difficult to respond. I would be VERY interesting to hear from you a concise description of exactly what you perceive ST to be.

>From tom Bourbon [940308.0920] via Rick Marken (Second try)

>

>>As bad as you people

>>think you have it, we're worse of substantively. All we have left is

>>really the inheritance of our old institutions: a few journals, a few

>>professors, a few societies.

>

>Institutions? Journals? Professors? Where can we get some of those?!

I'm sorry, I wasn't clear. The key word is "substantively". In other words, you have better "substance" (you're doing better work), while we continue to live off the institutional capital acquired in the "golden age" of ST (1940-1970).

>\*You\* see PCT as part of first-order cybernetics. That's fine. On

>this side, I'm not doing FOC. I'm doing PCT,

That's because you've got a GOOD MODEL. Remember, ST/Cyb is concerned with models of ALL KINDS. But once you get a good model to describe a particular phenomenon, then of course you settle on it, and mine it for all it's worth. You no longer have need for OTHER KINDS of models, and thus for ST/Cyb in general. You're no longer doing the theory of systems in general, you're doing the theory of CONTROL SYSTEMS in particular. You only venture back "up" into the world of FOC when you call your basic architecture into question, or try to explain it to people or relate it to OTHER kinds of systems. Thus my suggestion that the real interface between ST and PCT is with the interaction of control and non-control systems, e.g. populations and organisms in their environments.

I wish I had a good analogy. I think Martin compared PCT to Fourier transforms. Is someone dedicated to exploring the world of Fourier transforms still doing mathematics?

>Frankly, I see \*no direct way\* in which the pre-1970's ST/Cyb movement

>is the intellectual heritage (forebear) of PCT.



Well, I guess we'll just disagree. I've explained this about as well as I can. But see below.

>As for PCT being a chapter in ST/Cyb, that book and chapter must be written  
>by someone else; we aren't even thinking about that linkage.

Certainly the book as a whole would be written by ST people. But if I actually INVITED a paper for PUBLICATION (now think carefully!) in such a book, would you really turn me down? Nahh, PCT people ENJOY not being published! ;->

>at six recent issues of \_Behavioral Science\_, the old SGSR journal.

The SGSR (now ISSS) has for a long time been unhappy with \_Behavioral Science\_ as their flagship journal. De facto that is now \_Systems Research\_. However, it will likely not please you either, if you could even get your hands on a copy.

>You disagreed with me, then made my case. By "standard or common  
>language" I meant one shared by ST people. There was no such animal,  
>as you say here.

More lack of clarity on my part. The Klirian language is standard in the sense that it is universal, i.e. can be applied to model anything (I'm not saying it SUCCEEDED, but that it made progress). It is not standard in the sense that it is an "accepted" standard. It is standard like Esperanto, not English; like Ada, not MS-DOS (yikes, I HATE ADA!). And of course, it is regrettably not common in any sense.

>The view from my location probably was very different from the one you  
>encountered and constructed (do you see, Dan?) as a student of one of  
>the "names" on the inside.

I actually quit my job and moved hundreds of miles to study with Klir BECAUSE I appreciated the language. But the point is taken.

>Different meeting. The first Gordon Conference on cybernetics was at  
>Wolfboro, NH; the second was at Oxnard, CA.

OK. BILL: who else was at Tilton?

>Temporal priority or contemporaneity does not necessarily  
>imply a common line of descent.

All right, let's argue the semantics of "heritage". To my mind, this does not require lineal descent. First, I have argued, and Bill Leach agreed, that in its SUBSTANCE PCT is a KIND of ST, as I've defined ST.

But even historically, there has clearly been SOME relation between PCT and ST/Cyb (especially it's Cybernetic component) over the years. Mostly, PCT has evolved to SOME extent in the general CONTEXT of ST/Cyb. The participation of PCT people in ST/Cyb newsletters and conferences, if even to only reject ST/Cyb, is at least evidence of this. As noted, this relationship has not necessarily been cooperative or even tolerant, let alone reciprocal, but nevertheless PRESENT. HAD I NOT BEEN A CYBERNETICIAN, I WOULD NEVER HAVE HEARD OF PCT OR MET THE POWERS! OK?

>It's obvious your nose is different from the noses of most other  
>ST/Cybernetics people! :\*) <--CJ

Hmm. :ST))

Cliff Joslyn

Date: Wed Mar 16, 1994 7:41 am PST  
Subject: Re: Finally, a reply on Systems Theory and PCT (winding down?)

<[Bill Leach 940316.09:25 EST(EDT)] >[Cliff Joslyn, 940315]

>... So just as the mathematician is not responsible for a bad bridge  
>design, so ST cannot be held responsible for bad modeling. In particular,  
>every model is highly incomplete, with ...

I will accept this in part. ST is not responsible for errors in application except that ST scientists should speak out against obvious missapplication and should present a united front against their own that abuse the principles of ST and science in general.

More to the point... we are not here talking about something so minor as a bridge. We are talking about a particular field of science that is being used as the justification for fundamental alterations of human society. Alterations that will involve trillions of dollars and billions of lives.

The problem is not "at the feet" of ST people exclusively by an means. The real problem is that as far as the interaction with the general public, science has sold itself out. It seems that research dollars and fame have become much more important than truth itself.

My perception of Rick and others here is that while not necessarily high on the diplomacy scale, they are VERY high on the integrity scale. I can not imagine Rick selling out PCT for either fame or money.

>It is the responsibility of the specialist modeler to fess up to ...

Again, yes this is true but how many times have the members of a fundamental science tried to correct errors in the application of "their" knowledge? My perception from my own study of the history of science is that such effort has always been common. The "pure science" individual that totally ignores all applied science is rare not common.

>...inherently different from traditional scientific reasoning. This is  
>because the pillar of the scientific method, repeatability, does not  
>generally hold with them. We cannot run the US economy back to 1932 and  
>consider what would have happened without the New Deal; or try cutting  
>CO2 emmissions and ...

You are begging the issue with this one. Yes, as far as we know, you can not rerun the economy from 1932. This only means that a particular method of validation is not available not that there is NO rigorous validation possible. In my mind a "true Systems Theorist" would not consider a model reliable until validated and even then would want to recognize the limits of the model's predictive power.

>Risk analysis

Your statement on this is, in my opinion, quite correct. However, it is typically used in a highly misleading fashion. "I mean, like after all, there is a finite probability that the SUN will stop fusing tomorrow at noon. We must act NOW! Never mind that we don't know what is going on. Never mind that we have NO theories for either the problem or the 'solutions'... but only hypothesis that do NOT stand close examination."

>And exactly who would these people be, now?

I know that several of the names have appeared here in the postings of others but I would have to do a bit of research myself to find them (your name was not in the group :- ) ).

>>he has convinced me that he is dedicated to objective science

>Have I of myself, as well?

No, not yet, but you show promise (besides, you obviously have earned the respect of at least several others here).

>Honestly, Bill, your perception of what ST is is so far removed from my own that I find it difficult to respond. I would be VERY interesting to hear from you a concise description of exactly what you perceive ST to be.

Actually, I think it would be more interesting to hear what you consider ST to be... more useful too.

-bill

Date: Wed Mar 16, 1994 9:24 am PST  
Subject: fault detection; random reorganization

[From Bill Powers (940316.0820 MST)] Bill Leach (940315.2118 EST)

RE: improved fault detection through rotation.

One feature of human vision that hasn't been considered so far is the way it constructs 3-d images out of shading. Renderings in graphics displays and visual depth perception in pictures of the moon show z-axis variations through creating a first spatial derivative. Lighting from one side makes upslopes brighter and downslopes dimmer than the average illumination. While this kind of perception is ambiguous (I have seen the Moon looking as if it's covered with domes instead of craters), the depth effect is strong.

As I understand the situation, the high spots on the circuit board are shown bright and the low spots or holes dark. This is like viewing under flat (full-moon) illumination. There is a possible way in which rotation of the image could create apparent Lambert-law shading in the eye. A moving spot will create an edge-enhancement on the leading edge of the spot, and an edge-suppression on the trailing edge. This will convert a small flat dot into a hillock (or a dimple) through the brain's interpretation of shading as depth information.

Can you confirm that this effect is seen -- that the defects seem literally to "stand out" in depth?

I should mention that this isn't really a "PCT" phenomenon: it's a phenomenon of passive perception, but not of control. We need, of course, information about the properties of perception in modeling a whole control system so this kind of information is nice to have around.

RE: random reorganization

>In some random attempts, it perceives no improvement in the  
>error condition and will choose another "transfer function"  
>essentially at random (I can envision that this might not be  
>fully random in that the organism may "avoid" transfer  
>functions that are perceived to be similar to the failed one).

That's the idea, but a little too elaborate. Reorganization as I conceive it is not "choosing among" transfer functions, but modifying parameters on which the transfer function depends. Suppose you have a parameter  $p$ . Associated with this is a  $\delta p$  (which is very small). In computer simulations,  $\delta p$  is added to  $p$  on every iteration. As long as the control-system error is decreasing,  $\delta p$  remains unchanged and the parameter continues to change by the same amount  $\delta p$  on every iteration. When error increases, however, a new value of  $\delta p$  is chosen at random, scaled to the absolute value of error. This scaling assures that the magnitude of  $\delta p$  will decrease as error approaches zero, and the rate of change of the parameter will slow down. As  $\delta p$  varies at random over its range from some positive limit to some negative limit, the parameter  $p$  is made to increase and decrease at various rates. Provided there are no strong local minima in the relation between  $p$  and error, the result will be adjustment of  $p$  to the value that minimizes error.

The transfer function can depend on more than one parameter -- for example, proportional gain, first derivative, and integral components, each associated with a proportionality factor. The interesting property of random reorganization is that all three parameters can be adjusted simultaneously simply by monitoring the square of the error signal and computing its rate of change. If the rate of change is positive, the  $\delta p$ 's associated with all three parameters are reselected at random, at the same time. If the rate of change is negative, the  $\delta p$ 's are left alone. Again assuming that no strong local minima are encountered, the result will be a strongly biased random walk toward the values of all three parameters that will minimize the error. The reorganizing process can be made more selective by setting the threshold for random change to cause reorganizing only for significantly negative rates of change of error-squared.

When error approaches zero, the random walk will cause continuing small fluctuations in the parameters, but they will stay in the vicinity of the optimum values for minimum error. This kind of reorganizing process can be left turned on all the time, always ready to bias the random walk toward a new optimum when external conditions change.

In my tests of this idea, which have only been preliminary, I started with 10 control systems controlling an environment consisting of 10 variables. Each control system perceived a function defined as a weighted sum of all 10 variables. The outputs of each of the the 10 control systems fanned out to affect all 10 variables, the sign of each effect being selected at random and then being left

fixed. Any one external variable was thus affected by the outputs of all 10 systems. The criterion for reorganization was that the sum of the squares of all 10 error signals became positive (there was no attempt to assign the cause of error to any one control system -- only the total summed square of error signals was used).

There was a delta chosen at random corresponding to each of the 10 perceptual weights in each of the 10 control systems. When a reorganization occurred, all 100 deltas were reselected at random and scaled to the total absolute error signal. At all times each delta was added to its corresponding perceptual weighting on every iteration.

Oh, yes: the 10 reference signals were also selected at random prior to a run, and left fixed. All the output functions were leaky integrators with a static gain of about 20.

The result was that each control system acquired a set of perceptual weightings and brought the resulting perceptual signal to a match with its corresponding reference signal -- after 500 to 5000 reorganizations. Each control system was then perceiving and controlling a different aspect of the 10 external variables, by acting on all 10 variables. Each "aspect," in Martin Taylor's terms, would be a CEV. The set of 10 systems reorganized its perceptual functions to define 10 CEVs. None of the CEVs had any single external counterpart; they were just different functions of the same 10 variables.

This is entirely equivalent to solving 10 simultaneous nonlinear equations in 10 unknowns by a method of steep descent. I first used a similar method in an entirely different context in the early 1960s, while developing methods first for sharpening spectra, and then for sharpening astronomical television images.

Any criterion at all can be used as the basis for triggering reorganizations, as long as it pertains to an effect of changing the system parameters. We could make the 10 external variables have side-effects on simulated hunger, thirst, horniness, pain, and so forth, with each side-effect being compared with a reference state and the summed squares of error being used to cause a reorganization when the total rate of change became positive. If there is any solution, the reorganizing system will eventually find it, or die. We must never forget that organisms do die, and quite frequently. There is no ultimate optimal solution in nature -- so far.

Best, Bill P.

Date: Wed Mar 16, 1994 9:33 am PST

Subject: In defense of "randomness"

[from Gary Cziko 940316.1648 GMT] Hans Blom, 940315, argued:

>Why reorganization cannot be random in complex organisms

If you'd like some good evidence of how "random reorganization" (I'd rather call it blind variation and selective retention) can result in the emergence of complex adaptive systems, then just look in the mirror or observe any other living organism. This is how evolution works.

If you'd like faster and more high-tech examples, then I suggest that you take a look at John Koza's (1993?) book GENETIC PROGRAMMING and accompanying videotape (I don't have the full reference here but can provide it if needed).

Or for many more examples, you can wait for the (hopeful) publication of my book WITHOUT MIRACLES: UNIVERSAL SELECTION THEORY AND THE EVOLUTION OF KNOWLEDGE.

You point out that "random reorganization" (my "blind variation") is more likely to result in less adaptive (fit, useful) systems. This is the case on any one trial. Mutations are also almost guaranteed to be maladaptive. The trick is to eliminate the less fit solutions and save and cumulatively build on the rare advantageous ones. This is how evolution works and I would argue (and do in my book) that ALL knowledge growth is based on this process. Pretty wasteful, I agree. But that seems the price one must pay for a naturalistic explanation of knowledge. --Gary

Date: Wed Mar 16, 1994 11:51 am PST  
Subject: Playing Consiousness

[From Rick Marken (940316.1000)]

Hans Blom (940315) makes a good point about random reorganization but, as Bill Powers (940315.2130 MST) says, probably overstates the case considerably when he says that "reorganization cannot be random".

Hans' analysis led me to think of a new demo -- and extension of the E. coli demo -- that 1) illustrates Hans' point and 2) shows how consciousness might "guide" reorganization.

In one version of the E. coli demo (the one I used in the "Selection of consequences" paper, an IBM edition of which is on Dag's disk) a person presses the space bar to change the direction of movement of a dot in the two-dimensional space of the display screen. The new direction is randomly selected; nevertheless, people have no problems moving the dot to any target position on the display. The person does this by pressing quickly after a random change that results in a "worse" direction and at some delay after a random change that results in a "better" direction. The result is a "biased random walk" toward the intended destination -- the "bias" being the result of the controlling done by the person at the keyboard.

Another version of this E. coli demo puts the person in something more like the position of the reorganizing system. This time the subject sees a short line that only moves in one dimension -- say vertically. The vertical position of this line is a measure of the distance of E. coli from a target point in two-dimensional space; this measure of distance can be thought of as a measure of "error" in the E. coli distance control system -- assuming that E. coli wants to be at the target -- zero error. The closer the "error line" to a mark at the top of the screen (the "zero error" line), the smaller the error. What the subject sees looks something like this:

\_\_\_ (zero error line -- fixed)

\_\_\_ (current position of "error line" -- always changing)

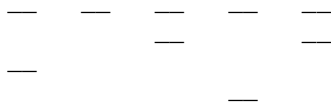
The experiment proceeds just like the original E. coli demo -- the subject presses the space bar to change (randomly) the direction of E. coli in 2-D space. But now all the subject sees is the result of this change in terms of vertical position of the "error line". The subject's goal is to move the "error line" to the "zero error" line and keep it there. The subject can do this easily -- using the same strategy as in the 2-D version of the E. coli demo.

This " error line" version of the E. coli demo puts the subject in a position like that of the reorganizing system. All the subject can see is the "error" in a control system; the subject (like the reorganizing system) can't see what the control system is controlling (2- D position in space in this case) or how it's controlling it (by moving in a direction in that space); all the subject (reorganizing system) sees is how well the control system is doing in terms of current level of error. When the error is not close to zero error, the subject (like the reorganizing system) hits the "change" bar. The result may be that the error start to get bigger or smaller; if it's the former than the subject hits the change bar again immediately; if it's the later, the subject waits to see how things go. This is how the reorganizing system is thought to work in PCT; the subject in this version of the E. coli demo is a "model" of reorganization.

Now to Hans' point. Hans argues (rightly) that if there are many error lines on the screen SIMULTANEOUSLY then it would be very tough to get then all to zero at the same time by just saying "change". Each error line represents error in a dimension of E. coli space -- which is now hyper-dimensional, as Hans suggested. Long ago I set up a verion of this demo (I'll have to do it again; it's pretty easy) and, sure enough, once the number of error lines to be controlled becomes greater than about 5 it becomes very difficult to keep all lines simulataneously at their zero error lines. This becomes difficult for precisely the reason Hans gave -- when you press the "change" bar (because one or another od the erroo lines is moving away from the error line) you MIGHT make things better for one or two error lines but, at the same time, the change makes things worse for the others.

It IS possible to control five error lines simulataneously using one change bar (I've done it), but the control is VERY poor -- and things become progressively worse as you add more error lines. This demo shows that Hans point is correct -- application of the random change strategy as a means of SIMULTANEOUSLY controlling error in multiple dimensions WILL NOT WORK once the number of dimensions becomes large (and not very large at that).

But I don't think that Powers ever proposed a "one change bar for many control systems" strategy of reorganization. I believe that Bill understood the problem right off the bat and suggested that one role of CONSCIOUSNESS is to DIRECT reorganization to the control systems that NEED IT; and the control systems that need to be reorganized are the one's experiencing chronic or increasing levels of error. To simulate the PCT view of the role of consciousness in reorganization, all we have to do is change the "multiple error line" demo so that each of the error lines is influenced by it's own private "change" bar -- say the keys that are in a row on the keyboard. The situation might look like this -- with 5 error lines:



—  
A S D F G

This display shows the "zero error" and "error lines" for five control systems (each experiencing a different level of error at the instant of the display) . The letters at the bottom are the "change" keys those associated with the error line above it; the "A" key influences the leftmost error line, the "S" key the second from left, and so on. The subject in this experiment plays CONSCIOUSNESS; when the error in any of these five control systems becomes "too big" from the perspective of consciousness (the big control system in the sky, so to speak) the subject (standing in as consciousness) can "reorganize" the system by hitting the appropriate "change" key. This way, reorganization can be applied only to those systems that need it and we don't run into Hans' problem of randomly reorganizing systems that don't need to be reorganized.

I'll try to work this up as a HyperCard stack or C program if anyone is interested.

Best Rick

Date: Wed Mar 16, 1994 3:01 pm PST  
Subject: Re: representational momentum

From Tom Bourbon [940315.1239]

>[From Richard Thurman (940315.1030)] >Tom Bourbon(940313.1309)  
>  
>> My first  
>>comment is that you have described what seems to be a perceptual effect  
>>similar in certain ways to many other "aftereffects" of perceptions --  
>>reversed rotations of the visual field after you stop spinning, color and  
>>brightness afterimages, and so on. These probably reflect activity within  
>>the perceptual functions (input functions) for the different perceptions,  
>>rather than controlled perceptions. PCT tests, then, would not necessarily  
>>be aimed at the phenomenon itself as a controlled perception (although it  
>>might be one).  
>  
>I had been wondering the same thing. Except that I don't think that it  
>reflects a perceptual aftereffect. It seems to me that this may be an  
>aftereffect due to the dynamics of closed loop control systems. For  
>example, this could be a simple case of what happens when a hierarchy of  
>control loops no longer receive updated reference signals (or something  
>like that).

I sent a very quick reply, although you wouldn't know that from the fact that it appeared on the net days later. When I mentioned "perceptual aftereffects" I wasn't ruling out a PCT explanation. As you say later in your reply, many current explanations of aftereffects border on, or cross into, the outlandish.

I was emphasizing the idea that the "momentum" might arise from a perceptual function of a control loop. In the example you described, it seems that the reference signals would remain "in effect" while the input, hence the perceptual



signal, changes (or, in another way of describing the "rotation" that stops, the input "does not change"). What could serve as a source of "updated reference signals" that are "received" by the hierarchy?

. . .

>Instead of trying such speculations [TB: such as evolutionary mumbo jumbo],  
>wouldn't it be better to create a model based upon PCT and show that the  
>phenomenon is nothing more than the side effects of a set of control  
>systems that were abruptly +turned off.+

Side effects. That looks like an interesting idea to explore.

But isn't it the change in environmental events that is "turned off" abruptly, not the control system? Which control system? Is the participant sitting and looking at the sequence of displaced boxes? Which perceptions might the person be controlling in that experiment? It seems that we would need to identify a candidate for the role of "controlled environmental variable," and then postulate a controlled perception, before we could begin to model anything -- first the phenomenon of control, then the modeling. My questions are not intended as criticisms; I'm just having some difficulty figuring out what you think the controlled perception might be.

>>It looks to me, after your brief description, as though the experimenters  
>>were already doing something somewhat close to what we call "the test for a  
>>controlled variable." They put the fourth rectangle at various positions  
>>and ask the person whether it is in the same position as the third one.  
>

>Yes, they are doing about 1/4 of the test for the controlled variable.

I'm not sure I would say they do even 1/4. They may be doing something a little bit (< 1/4 ?) like disturbing a controlled perception, but I doubt they think of their procedure in those terms.

> I guess I am interested in seeing if they can be induced to apply the rest  
>of the test and get to work on the real phenomenon.

That's always been the difficult part!

> For example, if I  
>create a hierarchal model that shows the same effects (say above .98  
>correlations) as human subjects display. And then show that the result is  
>simply a side effect of hierarchal perceptual control. I would then want  
>to induce them to perform a line of research more like what you describe below.

That you might build a model, and that you might show it to them I do not question or doubt. That, you having done so, they might be "induced" to do \*anything\* like change their current line of research, I doubt. Let's just say, I'm (sadly) willing to place a little side bet on the outcome.

Also, before you build the model, be sure there is a strong candidate for a controlled perception. Without that, there is nothing for the model to control, and with nothing to control, there is no side effect of control.

>>A more direct test might be to put the fourth rectangle at various positions

>>and ask the person to put it, and keep it, where it belongs by moving a  
>>control manipulandum. Certain positions for the fourth rectangle should not  
>>disturb the person's reference for position, others would. I would expect  
>>the aftereffect to "fade" or diminish in magnitude across time, so the  
>>person's placement of the fourth rectangle ought to change across time as  
>>the aftereffect changes.

>

>I agree that this would be a more appropriate way to research representational  
>momentum. But first I need to get a grip on how and why there is such an  
>+aftereffect+ and the mechanism underlying its fading.

Wouldn't you first want to see if there might be a controlled perception lurking  
somewhere in these data? You already know there is an effect of some kind -- it  
even has a proper name; but is there a controlled perception? Or is the effect  
itself only a side effect of something else? I'd want to start by answering that  
question.

>Thanks for the response.

It's an interesting problem to play with.

Later, Tom

Date: Wed Mar 16, 1994 5:20 pm PST  
Subject: Re: fault detection; random reorganization

<[Bill Leach 940316.18:29 EST(EDT)] >[Bill Powers (940316.0820 MST)]

>Can you confirm that this effect is seen -- that the defects seem  
>literally to "stand out" in depth?

Bill; I'll try to do that and get back to you.

-bill

Date: Wed Mar 16, 1994 6:52 pm PST  
Subject: More ideas on Representational Momentum

<[Bill Leach 940316.20:07 EST(EDT)] >NET

It just occurred to me that the very reason that I gave as a reason why  
"Representational Momentum" did not make any sense from a physical point of view  
may indeed actually be the major reason why the phenomenon does exist!

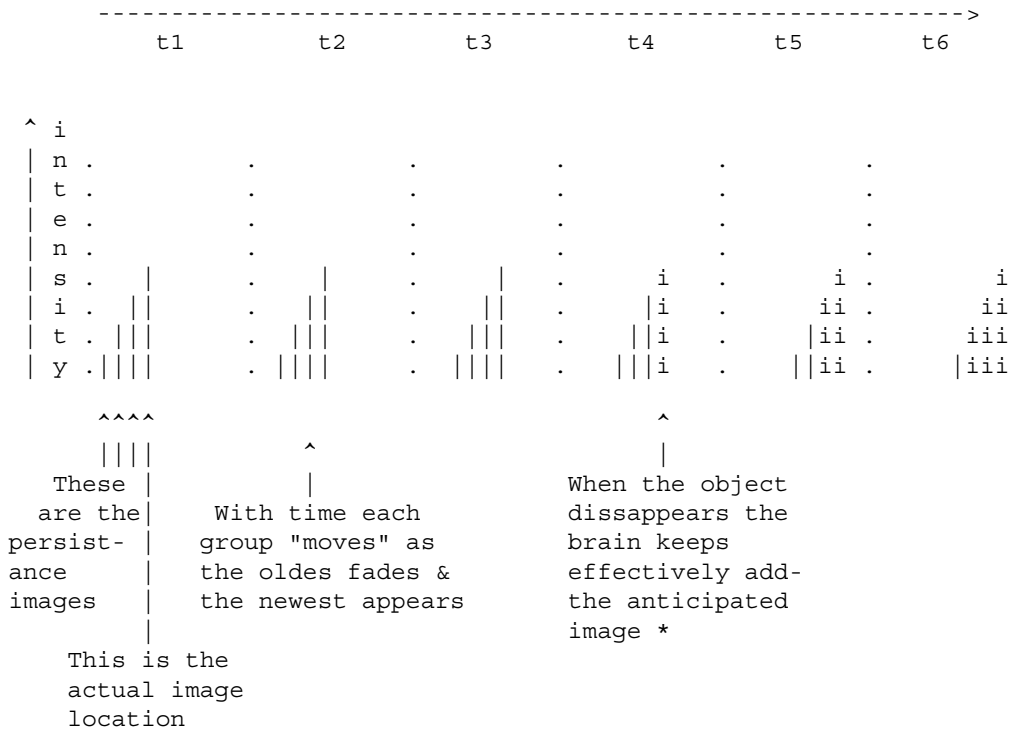
"Persistence of vision" could explain "Representational Momentum" in a fashion  
that is even more obvious than I had imagined.

Graphically:

If an object is moving from left to right, and disappears at t4.  
.=some fixed reference point

|=the visual input signal from a point on the retina (stacked for

intensity indication).



\* I don't actually believe that is what happens (ie: that the false image is actually created but instead what might be happening is that the perceptual image is created from the matrix of visual impulses compared to appropriate references chosen from some sort of image memory. The brain then rotates this perceptual image based upon BOTH the newest image input impulse AND the series of fading image impulses.

The idea here being that the brain has learned something about motion both by the "displacement" of "new" images and by such things as the amount of "smearing".

I would suggest that some people may be able to "control" which perception is used.

I realize that this might not be new (or may have been considered and rejected) by others here but I would be interested in comments. If I am correct on the ability of some people to intentionally reject the smearing then it should be possible to "train" people to perceive the actual position when the object stopped.

-bill

Date: Wed Mar 16, 1994 6:58 pm PST  
 Subject: Re: fault detection; random reorganization

<[Bill Leach 940316.20:50 EST(EDT)] >[Bill Powers (940316.0820 MST)]

Mark has agreed to write something up on his work and testing. I can't really estimate when he will do this but I'll try to keep "after him".

-bill

Date: Thu Mar 17, 1994 6:35 am PST  
Subject: Demos--Dag

[FROM: Dennis Delprato]

Can details on the demos that have been referred to as associated with Dag be posted on CSG-L (again) or sent directly to me?

Thanks, Dennis Delprato psy\_delprato@emunix.emich.edu

Date: Thu Mar 17, 1994 7:15 am PST  
Subject: Re: Demos--Dag

From Bill Silvert [940317]

>Can details on the demos that have been referred to as  
>associated with Dag be posted on CSG-L (again) or  
>sent directly to me?

These are now installed on biome. I posted a notice to this effect several days ago, but it doesn't seem to have appeared (maybe I don't have the server set to send me my own messages?).

Bill

Date: Thu Mar 17, 1994 8:52 am PST  
Subject: reorganization random?

[Hans Blom, 940317] (Bill Powers (940315.2130 MST))

>There's another way to approximate the same prediction without  
>taking into account the curvature properties of hyperspheres.

I do not think that the following train of thought applies:

>Ignore the curvature, and assume that for each dimension the  
>probability of choosing a better direction is just 50% (the  
>choice in each dimension is simply to increase or decrease the  
>parameter). For 20 dimensions, the random changes have one chance  
>in  $2^{20}$  (about one in a million) of achieving a better direction  
>in all dimensions simultaneously.

It is not necessary to achieve a better direction in all dimensions simultaneously. If the origin is the optimal combination of parameters, going from (1000, 0) to (10, 10) is quite an improvement, assuming that all parameters contribute approximately equally to the error. Ignoring the curvature results in 50% of the new parameter settings being better and the other 50% worse -- again

with appropriate scaling. Moving TOWARD the origin is better, moving AWAY makes things worse. Or don't I understand you?

> Random reorganization, however, is very powerful for  
>simultaneously adjusting control in "a few" dimensions, whatever  
>that number is, maybe 5 to 10.

That is due, I think, to the fact that in a space of low dimensionality the volume is not yet heavily concentrated in the hypersphere's surface layer. For a dimensionality of 5 to 10, maybe only 10 to 40%, say, of the new settings improve things, but not an almost infinitely small percentage. Trial and error still works as a strategy, but ever more badly as dimensionality increases.

> This can become an argument in  
>favor of a modular and hierarchical structure of control systems,  
>where the number of dimensions involved in any one control loop,  
>at a given level, is kept small.

This might work if it is not the full set of parameters that needs to be adjusted, but only a much lower number of combinations of them. That is, there are a great number of possible solutions to the adjustment problem. That seems to be the case in practice. The evolutionary problem -- gene transmission -- is solved by so many organisms in so many different ways. The human problem -- how to adjust to your niche in society -- is solved by every person by arriving at a unique character structure. Every person develops a unique repertoire of skills. Etcetera.

>It is also possible, or even likely, that through the basic  
>process of random reorganization, there would evolve systematic  
>reorganizing processes. I'm working on one now, an "artificial  
>cerebellum" that uses a very simple and neurologically plausible  
>deconvolution method for stabilizing control systems.

That is exactly the line along which my thoughts go. A number of such hill-climbing-like techniques are well known in fields such as adaptive control theory, neural networks, etc. These techniques arrive at optimal solutions in a much more efficient way (a much shorter time) than plain random search. The problem is that these techniques only work if the error surface (the mountainous landscape) is both smooth (differentiable) enough and constant enough over time; staying at a mountain top during an earthquake is not an easy thing to do. So sometimes these techniques work, and sometimes they don't. My guess is that they only work reliably enough for those aspects of the "world" that are sufficiently smooth and predictable -- those aspects, in other words, where an "internal map" can be constructed and "feedforward" c.q. map-guided behavior works. But let's not get that can of worms out of the cupboard again :-).

>I am shy of any proposal that involves a great deal of systematic  
>reorganization, first because "systematic" tends to mean "higher-  
>level," and higher-level systems must themselves be constructed,  
>and second because it is too easy to give such systems knowledge  
>about the environment that is not likely to arise through the  
>slow processes of evolution. My rule of thumb is that an evolved  
>higher-level systematic process of reorganization can't take  
>advantage of environmental regularities that last less than 100,000 years.

Maybe. From systems theory we know, that we cannot specify a good test procedure to identify a "black box", i.e. fully unknown object, of which only input and output ports are defined; it is impossible to prescribe a priori a sequence of test signals such that some sort of correlation between outputs and inputs will fully characterize the object. This is particularly difficult when the object has memory. On the other hand, when we know the unknown object's internal STRUCTURE, we only need to establish a set of parameters. The latter can be done in an efficient and rigorous fashion.

So I think that in general reorganization/learning has two phases. In the first phase, the STRUCTURE of the "black box" must be (approximately) established. Compare this with the FORM of a formula in physics: does the distance term go to the denominator or the numerator in the law of gravitation? Does it get a square, a third power or a square root? This is the hard, unsystematic work. The next phase is establishing the parameters. That is easy now.

I think the brain has/had the same problem. Finding a structure that gears well with the tasks to be solved by the organism must have taken evolutionary time. With an appropriate structure in place, finding an appropriate combination of parameters can be done within an organism's lifetime.

>I think your Subject heading, "reorganization cannot be random,"  
>overstates the case considerably. IF the entire system must be  
>reorganized as a single multidimensional unit, then of course you  
>are right. But if reorganization occurs level by level, and in  
>separate contexts involving only a few dimensions at a time, then  
>reorganization at random becomes more feasible.

What you propose here sounds similar to allowing a two-dimensional coli approach to its target along certain paths only and forbidding others. The problem now arises as to how to select helpful paths rather than an unpassable maze. We seem to have those helpful paths in abundant quantity: innate perceptual processing functions, elementary reflexes, emotions, and other mechanisms like that.

> I do not, however, rule out some carefully-selected instances of systematic  
>reorganization.

What do you mean by reorganization here? Is it more like building up the structure or is it more like establishing the parameters of an a priori given structure? It seems to me that the former is not well-defined in PCT, whereas the latter is unnecessarily often done through methods that are so inefficient (for a complex organism) that they cannot possibly work.

(Gary Cziko 940316.1648 GMT)

>You point out that "random reorganization" (my "blind variation") is more  
>likely to result in less adaptive (fit, useful) systems. This is the  
>case on any one trial. Mutations are also almost guaranteed to be  
>maladaptive. The trick is to eliminate the less fit solutions and save  
>and cumulatively build on the rare advantageous ones. This is how  
>evolution works and I would argue (and do in my book) that ALL knowledge  
>growth is based on this process. Pretty wasteful, I agree. But that  
>seems the price one must pay for a naturalistic explanation of knowledge.

I agree with you where your argument concerns the evolution of structures, but not where you seem to think that biological organisms cannot possibly have "discovered" hill-climbing-like parameter estimation techniques. Such a technique (the construction of "super-perceptions" of global, organism-wide concepts of "better" and "worse") IS one of the structures, I think, that was "invented" in the long evolutionary struggle for life. But once it was there, efficient "learning" made all the difference in higher organisms, where you may even encounter "one shot learning".

(Rick Marken (940316.1000))

>Hans' analysis led me to think of a new demo -- and extension of  
>the E. coli demo -- that 1) illustrates Hans' point and 2) shows how  
>consciousness might "guide" reorganization.

Great idea for a new demo! But why necessarily consciousness? Thus far, I have found consciousness to be a non-explanatory term. Might there not be more likely built-in mechanisms that can do this job? In me, your concept of "error lines" triggers the association of "emotions". I have heard of emotions being defined as "action tendencies", meaning that emotions select between different behavioral ways to reach the same goal. In that view, the emotions are perceptual processors that integrate all lower level perceptions into a "mood" or "feeling state", that in turn determines which of the actions in our full repertoire of potential actions are to be centered upon. A kind of attentional mechanism, therefore. Might there be more of such mechanisms?

> -- application of the random change strategy as a means of  
>SIMULTANEOUSLY controlling error in multiple dimensions WILL NOT WORK  
>once the number of dimensions becomes large (and not very large at that).

Did you really do those experiments before? Or are you so proficient a programmer that you set up this thing in a lost half hour? Great!

Greetings,

Date: Thu Mar 17, 1994 9:03 am PST  
Subject: Re: representational momentum

[From Richard Thurman (940317.0920)] Bill Leach 940315.21:43 EST(EDT)

>A thought that I had concerning this that I did not express before, is  
>that it seems to me that "visual persistence" would act in exactly the  
>opposite direction. That is, when the image "disappeared", there is an  
>actual retinal signal that persists for at least several hundredths of a  
>second after the image is gone.

Yes, that occurred to me also. Research on 'iconic storage' in the 1960's showed that the 'visual sensory register' can hold the visual scene for about 250 - 300 milliseconds. That would mean that the last visual image (the third display) would probably be accessible by the time the test display was given and may still be available for comparison. It is why I don't think it's a perception (meaning input) phenomenon. Instead I feel it may be a side effect due to the dynamics of hierarchical control itself. (But see Bill P.'s response below for a plausible input explanation.)

I was really hoping one of the old hands at this would recognize it immediately and explain it all to every ones satisfaction, along with some rather simple experiments that would 'prove' it to the world. Alas, instead I may have to actually do some modeling and run some subjects myself. I'm sufficiently curious to give it a try, but its out of my field of research and I feel a little like a fish out of water. Perhaps thats the nature of PCT... forcing one to look across disciplines so that we can get a fuller -- richer-- picture of how we (living things) operate.

>The would be a real "beaut" for PCT to successfully model.

Agreed.....

>Another "interesting" experiment would be to repeat known PCT experiments  
>using Video Monitors that have been "turned upside down". Basically, I  
>wonder if anyone has questioned the monitor's own effects on motion  
>display? Of course, it might be simpler (or already have been done) if  
>there are display units available that have significantly different  
>screen write speeds.

Sorry, you lost me there. What did you mean by "turned upside down?" In terms of display speeds its fairly routine to vary the speed of display of pictures on the computer screen. Its been done even longer with tachistoscopes. Can you explain a little more of what you meant here?

Bill Powers (940315.2130 MST)

>This is most likely a phenomenon of perception, to be  
>explained by the dynamical properties of perceptual functions.

>the  
>neural signal would take time to build up to an asymptotic value,  
>and after the cessation would take time to decline to zero again  
>-- leading to a continued impression of motion for a short time  
>after the actual motion had stopped.

Ahhh!!! This just provided one of those moments of insight. I had been looking at this as a top-down kind of thing -- Higher levels of the hierarchy doing their reference setting thing. I see now that you (and Tom, and Rick) are looking at this from the bottom up. Perceptions percolating up from intensities to and through transitions.

I had also not taken into account the idea of neural firing rates needing to rise and decline. How does one model this with a digital computer? I don't see this being taken into account in any code you or anyone else has distributed (e.g. the Byte articles, the Primer Series).

>According to that idea, there should be a converse phenomenon: a  
>motion that persists for only a very short time (how short to be  
>determined) should lead to an impression of less motion than  
>actually occurred. This should be testable by experiments similar  
>to those in which representational momentum was found, but using  
>rotations that begin instantaneously and persist only for a few  
>tenths of a second or so before stopping.



Unfortunately, the visual system registers and stores this 'iconic' information for longer than the brief presentation period. It would tend to wash out the effect one would be looking for. (I think.)

>This is all worth experimental investigation -- by someone who  
>isn't up to the ears in other experimental investigations.

Understood. Actually I'm amazed that you, Tom, Rick, Martin, Bill L. and others can get anything done at all with all the network traffic that this list seems to generate. Add that to trying to actually get 'real' experimental investigations done and I'm sure it creates quite a strain on time. I appreciate everyone's willingness to support my attempt to understand and apply perceptual control theory.

Richard Thurman

Date: Thu Mar 17, 1994 12:10 pm PST  
Subject: Reorganization can(not) be random

[Hank Folsom (half Irish & proud of it, except when I read about the IRA)  
940317]

(Hans Blom, 940315)

>A popular notion in PCT is that learning (reorganization) is random. A  
>popular demonstration of "random learning" is shown in what is called  
>"E. coli"-behavior, where random changes of direction, if applied at  
>appropriate times, can bring a simulated coli bacterium to its goal.

>... in more complex organisms randomness is not a sufficient (i.e. good-  
>enough) mechanism for >learning.

I, too, started out with this view, but the E. coli example has led me to believe/agree that reorganization does have a significant random factor.

Your geometry observations are correct geometrically, but our friend E. coli does not have sensors for points and radii. His reality is determined by his sensors. PCT unaware researchers found that he senses changes in the food concentration gradients. The food, I assume, is an irregular localized volume in which the food density decreases away from the center. What the bacteria senses is very dependent on direction.

You are quite correct that the odds are less than 1 in 2 of perceiving food. Of course, if there is more than one food source within range, the odds improve. But the odds could be maybe 1 in 10, if his sensors are not very sensitive.

The reason the bacteria can survive is because: he is a control system before he enters reorganization; he is a control system while in reorganization (I assume so as some mechanism is needed to to stop the tumbling); and he is a control system after the random tumbling.

While E. coli is moving into a food source, he has no error signal. When he passes through, he now has an error signal. He has no sophisticated directional

propulsion system to go back 180!, so he has only one option: Reorganize. In this case, reorganization is a momentary tumbling followed by taking off in some random direction with low odds of going towards a food source.

But if his sensors indicate there is no food this way, he again has the error signal and reorganizes again. E. coli wastes little time going along an unproductive path. When he does after many tries find his error signal is dropping, he continues to go in that direction. The way E. coli beats the odds is by spending much more time going along productive paths than in going along unproductive paths. Thank goodness for control systems!

I think what your analysis did not consider was that the random part of the reorganization process is a relatively small part (In this case, just long enough to change to another random direction), followed by actions determined by his control systems. It is through the actions of his control systems that he beats the bad odds.

Thus, for E. coli, at least, reorganization can be random.

Hank Folsom

Date: Thu Mar 17, 1994 12:15 pm PST  
Subject: Re: representational momentum

[From Richard Thurman (940317.1145)] Tom Bourbon (940315.1239)

Tom:

Thanks for the comments -- its helping me see and think this through.

>Except that I don't think that it  
>>reflects a perceptual aftereffect. It seems to me that this may be an  
>>aftereffect due to the dynamics of closed loop control systems. For  
>>example, this could be a simple case of what happens when a hierarchy of  
>>control loops no longer receive updated reference signals (or something  
>>like that).

..

>I was emphasizing the idea that the "momentum" might arise from a  
>perceptual function of a control loop. In the example you described, it  
>seems that the reference signals would remain "in effect" while the  
>input, hence the perceptual signal, changes (or, in another way of describing  
>the "rotation" that stops, the input "does not change"). What could serve  
>as a source of "updated reference signals" that are "received" by the  
>hierarchy?

I guess my "For example" was not very clear. I meant to speculate that the effects obtained in these studies could be the result of certain local control structures being sent reference signals to allow the overall 'tracking' of a transition. The perceptual input stops, but it takes a while for the perceptions to travel up the hierarchy. Higher levels are still sending down reference signals to 'perceive x amount of transition.' At some point in time the higher levels will perceive a 'transition gone' event and will send down reference signals to 'perceive 0 amount of transition' to the local transition control level of the hierarchy. But by that time the last display is long gone and the test figure may be appearing (or may have already appeared). At this point the subject is supposed to determine whether

the test figure's orientation matches the orientation of the figure displayed just before. This should be easy to do, except that the all these dynamic delays have been introduced.... producing a 'feeling' in the subject that the final orientation of the figure is further than it really was.

This is of course wild speculation but for me it represents a starting point to try to understand what is going on. I claim no special allegiance to this idea. Its simply the one that I came up with in the absence of any thing else. (What's that about random search being inappropriate for complex hierarchal systems?)

>>Instead of trying such speculations [TB: such as evolutionary mumbo jumbo],  
>>wouldn't it be better to create a model based upon PCT and show that the  
>>phenomenon is nothing more than the side effects of a set of control  
>>systems that were abruptly +turned off.+

>Side effects. That looks like an interesting idea to explore.

>But isn't it the change in environmental events that is "turned off"  
>abruptly, not the control system? Which control system? Is the participant  
>sitting and looking at the sequence of displaced boxes?

"Turned off" was an unfortunate choice of words. By "turned off" I meant both :  
'Were sent a reference signal to receive 0 amount of perception' and/or 'Received  
0 amount of perception.'

>Which perceptions  
>might the person be controlling in that experiment? It seems that we would  
>need to identify a candidate for the role of "controlled environmental  
>variable," and then postulate a controlled perception, before we could  
>begin to model anything -- first the phenomenon of control, then the  
>modeling. My questions are not intended as criticisms; I'm just having some  
>difficulty figuring out what you think the controlled perception might be.

But this is the point! Representational momentum may occur because of the dynamics of hierarchal perceptual control. It does not matter what perception is being controlled. Its simply a side effect of hierarchal control. If what I am saying is accurate then we should be able to look at any hierarchy of control loops and observe some sort of 'momentum' taking place. It simply takes time for a hierarchy of control loops to operate. It takes time for them (control hierarchies) to 'wind up' -- a kind of representational inertia (Bill P's [940315.2130 MST] converse example) and it takes time for them to 'wind down' giving us the phenomenon of representational momentum.

>> For example, if I  
>>create a hierarchal model that shows the same effects (say above .98  
>>correlations) as human subjects display. And then show that the result is  
>>simply a side effect of hierarchal perceptual control. I would then want  
>>to induce them to perform a line of research more like what you describe  
>>below.

>That you might build a model, and that you might show it to them I do not  
>question or doubt. That, you having done so, they might be "induced" to do  
>\*anything\* like change their current line of research, I doubt. Let's just  
>say, I'm (sadly) willing to place a little side bet on the outcome.

One must keep trying though! I have experienced a little of this in trying to explain PCT to others. At this point, I think it has more to do with my inability to describe PCT and less on others protecting their turf.

>Also, before you build the model, be sure there is a strong candidate for a  
>controlled perception. Without that, there is nothing for the model to  
>control, and with nothing to control, there is no side effect of control.

.  
>Wouldn't you first want to see if there might be a controlled perception  
>lurking somewhere in these data? You already know there is an effect of  
>some kind -- it even has a proper name; but is there a controlled  
>perception? Or is the effect itself only a side effect of something else?  
>I'd want to start by answering that question.

I'm speculating that the effect is "a side effect of something else." Simply the side effect induced by the dynamics of a hierarchy of control systems. Perhaps representational momentum simply belongs to a whole group of physiological and psychological effects that have been described in different terms and in different ways.

Richard Thurman

Date: Thu Mar 17, 1994 2:28 pm PST  
Subject: Blom on hill climbing & one-shot learning

[from Gary Cziko 930317.2140 GMT; not Irish at all and proud of it, especially when I consider the IRA]

Hans Blom, 940317 said:

>I agree with you where your argument concerns the evolution of structures,  
>but not where you seem to think that biological organisms cannot possibly  
>have "discovered" hill-climbing-like parameter estimation techniques. Such  
>a technique (the construction of "super-perceptions" of global, organism-  
>wide concepts of "better" and "worse") IS one of the structures, I think,  
>that was "invented" in the long evolutionary struggle for life. But once  
>it was there, efficient "learning" made all the difference in higher orga-  
>nisms, where you may even encounter "one shot learning".

Could you provide some examples of "hill-climbing-like parameter estimation techniques" and "one-shot learning"? I would like to try to argue that blind variation is an essential component of these processes, but I need to first have a better idea of what you have in mind.--Gary

Date: Thu Mar 17, 1994 3:11 pm PST  
Subject: Demodisk, BIOME server, Booklet

[From Dag Forssell (940317 1345)] Dennis Delprato (940317)

>Can details on the demos that have been referred to as associated with  
>Dag be posted on CSG-L (again) or sent directly to me?

Bill Silvert [940317]

>These are now installed on biome. I posted a notice to this effect  
>several days ago, but it doesn't seem to have appeared (maybe I don't  
>have the server set to send me my own messages?).

Let me begin with frustrated comments on Bill Silvert's note:

-----  
Anticipating that the files on my demodisk would be placed on the BIOME server, I downloaded the csg/Index (in January, as I recall) and found that it was broken down into several indexes, with many files gone since CSGintro was created in November 1992.

I have asked Bill Silvert to report on the new arrangements in a post to the net, in a direct post, and by snail mail when I sent the February demodisk. I have also asked Gary to check out the Index and talk to Bill as appropriate. (Gary is on the Internet; I am only on E-mail).

Just several days ago, Bill sent me a note, quoting my last request without comment, saying that he had just returned from Europe, and would attend to the disk in a week or two. Two days later, he posted that the February demodisk is available as a single zip file.

I deduce that Bill is busy, and suspect that my pleas for clarification of the Index will never be given time. Therefore this attempt to disturb Bill enough to attract attention.

On March 15 (obviously, it has not arrived yet), I mailed Bill the March demodisk as an update, along with a second disk, where I had taken the content of each directory on the demodisk and made it into an individual zip file. (With another direct plea for clarification, of course).

The file structure of the demodisk is as follows:

```
A:\          Floppy disk drive.   (This READ_1ST.TXT).
|
|---ARM1\    Little Man version 1.  With explanations.
|---ARM2\    Little Man version 2.  With instructions.
|  `SOURCE\  Source code for Little Man version 2.
|---CROWD\   Crowd program.  With instructions.
|---DEMO1\   Self-guided control theory tutorial #1.
|---DEMO2\   Self-guided control theory tutorial #2.
|---ECOLI\   Simulation of bacterial chemotaxis.
|---MINDREAD\ The computer reads your mind.  Test for control.
|---PCTDOCS\ Explanations and discussion from the CSGnet.
|---SIMCON\  Simulating analog control.  With tutorial text.
|---SPRDSHT\ Hierarchical control simulation.  With explanation.
|  `ASEASY\  A shareware spreadsheet.
```

In each directory are three files, for example:

```
ARM1.RD      1,897      (READ_1ST.TXT  5,735)
LMAN1.EXE    97,284
INSTALL1.BAT 507
```

The zip files I sent Bill typically include the three files, plus the basic READ\_1ST.TXT file. In this case: ARM1.ZIP = 90,049 bytes. (Not much compression here, because LMAN1.exe is a self-extracting, compressed archive file in the first place).

The files in ARM2\SOURCE and SPRDSHT\ASEASY are included in ARM2 and SPRDSHT, respectively. These are the files I sent Bill on the second disk:

ARM1.ZIP	90,049
ARM2.ZIP	140,570
CROWD.ZIP	166,278
DEMO1.ZIP	90,049
DEMO2.ZIP	95,015
ECOLI.ZIP	83,029
MINDREAD.ZIP	71,443
PCTDOCS.ZIP	219,666
SIMCON.ZIP	115,248
SPRDSHT.ZIP	371,783

I anticipate that netters will find it much easier to download one of these zip files than a 1.4? Megabyte zip file of the whole disk.

These files update and replace some of the files that are (used to be?) on the BIOME server.

When I attempted to take another look at the BIOME server Index structure a few days ago, I failed. The command get csg/Index did not work at all.

I conclude that the CSGintro document Gary sends out every month is obsolete and misleading when it comes to the BIOME server. It should be reconfirmed, (if I am mistaken), revised or deleted from CSGintro.

Anyone with access to Internet (most netters) is welcome to test my impression and report on the net.

-----  
Dennis, the above tells you what is on the disk, and how you can get it for free. If you want your very own disk to stick into your floppy drive, send me \$10, and I'll mail you a copy. DEMODISK is available as one 1.44MB 3 1/5" or two 1.2MB 5 1/4" disks, your choice. Address below.  
-----

Now that I have completed the third article and bound them all in a booklet along with my other supporting documents, I think it is fair to start charging \$10 for the booklet as well (postpaid). It does amount to a book on PCT. Perhaps this will make it easier for people to request a copy, since anyone will know that I get my expenses covered.  
-----

Best, Dag

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Internet: dforssell@mcimail.com                  MCI mail: 474-2580  
Date: Thu Mar 17, 1994 4:15 pm PST  
Subject: Reorganizatyion/tuning; modeling neural signals

[From Bill Powers (940317.1430 MST)] Hans Blom (940317)

>If the origin is the optimal combination of parameters, going  
>from (1000, 0) to (10, 10) is quite an improvement, assuming  
>that all parameters contribute approximately equally to the  
>error. Ignoring the curvature results in 50% of the new  
>parameter settings being better and the other 50% worse -- again  
>with appropriate scaling. Moving TOWARD the origin is better,  
>moving AWAY makes things worse. Or don't I understand you?

Sometimes I have trouble understanding myself, so don't worry.

The reason that we can't just head for the origin is that changing parameters doesn't in general have any known relationship to a change in the measure we're using as a criterion for reorganization. In my scheme, the parameters themselves aren't changed at random; what is changed is a set of deltas, one for each parameter. Each delta remains constant until a reorganization occurs. The deltas are added to the parameters over and over, so the parameters are always slowly changing (the deltas are small). The effects of these slow parameter changes show up as slow changes in the summed square of error. But as time goes by, the parameters individually pass their optimum values and start making the error greater again, and when enough of them do this the squared error starts increasing and another reorganization is required.

The analogy in the 2-D E. coli case is changing  $dx/dt$  and  $dy/dt$  at random. When the motion results in decreasing the squared distance to the center of a radial gradient, the bug proceeds in a straight line, which (since it is not likely to be aiming directly at the center) brings it eventually to move at right angles to the direction to the center, and then to start moving away from the center, at which point another reorganization is called for.

In the multidimensional case, particularly for nonlinear systems like the one I experimented with (where the parameters are input weights), the effect of changing a parameter depends on how all the other parameters are changing; what is initially a favorable direction of change (say, reducing the parameter) turns into an unfavorable direction (so the parameter might need to start increasing again).

Since parameters can only increase or decrease, the effect of either choice has a 50% probability of improving the approach to zero error (ignoring the `_amount_` of improvement obtainable, which is what I meant by ignoring the curvature). In general, however, the correct choice can't be made just once; what is correct will change as the approach to the final state proceeds. The nice thing about random reorganization is that you don't have to figure out what the best direction of change is for all the parameters at each moment (which can be a formidable computing task): you just try new sets of deltas until the error starts decreasing again. That is a stupid enough approach to be inheritable, one of the prime requirements for a reorganizing system.

>This might work if it is not the full set of parameters that  
>needs to be adjusted, but only a much lower number of  
>combinations of them. That is, there are a great number of  
>possible solutions to the adjustment problem. That seems to be  
>the case in practice.

Yes, I think you have a crucial point there. Another way to say this is that the environment must have far more degrees of freedom than the variables the organism learns to perceive and control. That is, I think, the case, even for a fully-developed human being.

>A number of such hill-climbing-like techniques are well known in  
>fields such as adaptive control theory, neural networks, etc.  
>These techniques arrive at optimal solutions in a much more  
>efficient way (a much shorter time) than plain random search.  
>The problem is that these techniques only work if the error  
>surface (the mountainous landscape) is both smooth  
>(differentiable) enough and constant enough over time ...

I suggest that we use the word "tuning" (which seems to be already in use in this context) to refer to parameter-adjustment methods that rely on some extensive amount of organization being already in existence (like a whole control system), and to reserve "reorganization" to mean the case of building a system from scratch or modifying its functions in some fundamental way. If you already have a basic control system and only need to tune it for optimum performance, that can be done by algorithm, but when you're still trying to decide what to control or what output effects to employ, that sounds more like reorganization. But I suppose the one will shade into the other in some circumstances.

This seems in line with your idea that random reorganization would apply more to building structures. Tuning would apply to adjusting parameters in existing structures. Actually, I've been using random reorganization to adjust parameters in existing structures, and it seems to work quite well, so maybe we shouldn't draw any firm lines here.

Your comment about the inefficiency of the random approach is well taken, but when we're talking about organisms we also have to consider the plausibility of an algorithmic approach. There might well be some complex and sophisticated algorithm that would work beautifully, but if it requires a complex program to implement it (and hence a highly-organized brain), it's not likely that an organism would somehow be born ready to execute that algorithm (and obtain all the input data it requires). If we propose algorithmic methods, they should be simple enough to make their operation in a 2-month-old baby (or a dog) seem plausible.

RE: Rick's beautiful new variant on reorganization experiments:

>Great idea for a new demo! But why necessarily consciousness?  
>Thus far, I have found consciousness to be a non-explanatory term.

All it describes is consciousness, unfortunately. Perhaps we should talk about "attention" instead. That isn't much better defined, but at least the term stays a little farther away from metaphysics. Rick is talking about an experiment that requires dividing attention among variables: handling more than one channel at a time, consciously. I guess we really can't get away from that word entirely. Somehow we have to distinguish between control processes going on in the foreground, which the subject could tell you about, and those occurring automatically in the background, unbeknownst to the subject.

-----  
Richard Thurman (940317.0920) --

>I had also not taken into account the idea of neural firing



>rates needing to rise and decline. How does one model this  
>with a digital computer?

While we define a neural signal in terms of firing frequencies, we model it just as a variable magnitude of something. Our models are basically analog models, even though they're implemented on a digital computer. To model the kind of perceptual function I described, you'd just use a leaky integrator.

>Unfortunately, the visual system registers and stores this  
>'iconic' information for longer than the brief presentation  
>period. It would tend to wash out the effect one would be  
>looking for. (I think.)

Maybe at one level it would, but at a lower level it wouldn't. In PCT, you have to get used to thinking in terms of levels, not "systems." There are many levels in the visual and other systems. The visual system isn't just one big lump. After you get past a certain level in the brain, furthermore, it doesn't matter whether the signals originated in one sensory modality or another; they're treated the same. You can get a perception of how many coins there are on a table by feeling them, or by looking. The sense of number is the same.

>I'm amazed that you, Tom, Rick, Martin, Bill L. and others can  
>get anything done at all with all the network traffic that this  
>list seems to generate.

I'm retired, so in my case it's not as much of a burden as it may seem, although I seem to manage to get in over my head frequently. I guess the other guys just give up free-time activities, like sleeping.

Best, Bill P.

Date: Thu Mar 17, 1994 5:30 pm PST  
Subject: Re: Demodisk, BIOME server, Booklet

I'm sorry that Dag isn't happy with the way I manage the server, and I would be delighted if anyone else would take over this job.

The files called Index are now called 00index since some packages come with Index files. This is common usage on servers.

I unzipped the disk that Dag sent me, the individual files are all there. This is not the March 15 disk, I don't have that yet.

I do not have time to do major housecleaning as Dag requests. I did some, but I really would like someone else to take over the job.

Bill Silvert

>[From Dag Forssell (940317 1345)] Dennis Delprato (940317)  
>Let me begin with frustrated comments on Bill Silvert's note:  
>-----  
>Anticipating that the files on my demodisk would be placed on the BIOME  
>server, I downloaded the csg/Index (in January, as I recall) and found  
>that it was broken down into several indexes, with many files gone since

>CSGintro was created in November 1992.

>

>I have asked Bill Silvert to report on the new arrangements in a post to  
>the net, in a direct post, and by snail mail when I sent the February  
>demodisk. I have also asked Gary to check out the Index and talk to Bill  
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>a few days ago, I failed. The command get csg/Index did not work at all.

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>obsolete and misleading when it comes to the BIOME server. It should be  
>reconfirmed, (if I am mistaken), revised or deleted from CSGintro.

>

>Anyone with access to Internet (most netters) is welcome to test my  
>impression and report on the net.

Date: Thu Mar 17, 1994 5:37 pm PST

Subject: Reorganization random?

[From Rick Marken (940317.1300)] Hans Blom (940317)

>Great idea for a new demo!

Thanks. Then that will be my presentaion in Wales. I guess there's going to be a lot of reorganization that the conference ;-)

>But why necessarily consciousness? Thus far, I have found consciousness to  
>be a non-explanatory term. Might there not be more likely built-in  
mechanisms that can do this job?

I think of whatever is doing the job of reorganization as being "built in". But even if you are right and "consciousness" is not what's involved in reorganization, I am still inclined to think that consciousness is "built in" to many living systems, not just humans. I think consciousness exists in living systems because it has evolutionary significance. I can't believe that consciousness suddenly emerged full blown in one creature -- Homo sapiens. But you may be right; the subject in my little demo may not actually be playing the role of what I think of as "consciousness".

>In me, your concept of "error lines" triggers the association of "emotions".

I suppose that the error lines could be like "emotions"; they are perceptions (as emotions are) and I can imagine them being experienced as emotions. But I think there is more to emotions than just individual error signals; there are perceptions of physiological states, for example. But the subject in the demo is doing more than experiencing error (emotion); the subject is controlling -- by altering the characteristics of other control. The subject is actually controlling several systems at the same time and allocating control to the system(s) that need it the most. I guess the subject is an adaptive control system -- a control system that adapts other control systems (by tweaking their parameters) so that they control better.

>Did you really do those experiments before?

Yes. In about 1984 -- on a Commodore 64 (remember those?). It was while I was developing the "human in the loop" version of the E. coli demo; I just did it for fun (it's VERY easy to write the program, especially when you have time - - I was a professor then; LOTS of time for "non-profit" creativity). At the time that I was doing this I had not thought of the difficulty of controlling multiple "coli" in terms of the point you made -- that it is a way of demonstrating the need for "modular" rather than "all systems at the same time" reorganization.

> Great!

Thanks ;- ) Rick

Date: Thu Mar 17, 1994 8:57 pm PST  
Subject: Try the keyboard

[From Bill Powers (940317.2105 MST)] Hank Folson (940317)

A nice, simple, and correct assessment of E. coli's movement-by-reorganizing. But Hans Blom is correct in saying that this method gets less efficient as more dimensions are involved. I think that we can say that this method, when found in nature, will prove to involve no more than 10 or so dimensions at a time. But that may, indeed, cover all the cases that exist in real organisms -- it could be that organisms are simply what this method can produce, applied at levels from DNA on up!

-----  
Richard Thurman (940317.1145) --

RE: representational momentum.

We can think up all sort of possible explanations for this phenomenon. Before we do, however, I would like to know what the actual data are. In how many subjects does this phenomenon appear, and in how many does it not appear? If it doesn't appear in all of them, then any general model we invent to account for it is wasted effort, because the model (if properly designed) will always show the effect, while real people will only show it sometimes. In that case, the model is a failure, isn't it? Every person who fails to show the phenomenon will be a demonstration that the model is wrong, because for each such person, the model will have predicted appearance of the phenomenon.

It seems to me that discussion of this phenomenon is an echo of what has happened to psychology. Failing to understand normal and ordinary behavior, many

psychologists turned to the odd little quirks and illusions, the oddities and seeming paradoxes, that occasional turn up if one is looking very closely at the details. It's much as if a person who failed to understand the purpose of a piano took to plucking its strings, slamming its lid shut, or tapping on the sounding-board in different places, looking for some amusing or puzzling sound to be brought forth, never thinking to try out the keys to see what they do. "Here," says the experimenter, rubbing steel wool on a tuning peg and listening with a stethoscope to the C# damper, "See if you can explain this sound!"

We have in front of us a great feast of data (to switch metaphors), spread out to our view in plain sight, waiting to be consumed and turned into nourishment. It's impossible to look at any human behavior without seeing evidence of how it all works. The problem is not to find something to explain; it's only which of the goodies before us to pick up first.

What we are waiting for (to switch back) is for someone to try out the keys and discover what this damned machine is really for, and start making some sounds worth listening to.

Best to all, Bill P.

Date: Fri Mar 18, 1994 9:31 am PST  
Subject: Re: Try the keyboard

[From Richard Thurman (940318.1015)] Bill Powers(940317.2105 MST)

Bill, that was a great post! I agree with everything you said. (Which may be a surprise to you. Evidently you are correcting an error you see creeping up in the discussion -- an error in the direction I am taking.)

>It seems to me that discussion of this phenomenon is an echo of  
>what has happened to psychology. Failing to understand normal and  
>ordinary behavior, many psychologists turned to the odd little  
>quirks and illusions, the oddities and seeming paradoxes, that  
>occasional turn up if one is looking very closely at the details.

Yes, this is exactly how I feel about the phenomenon and many others written about in the psychological literature. When I first posted a request for a PCT based explanation of this phenomenon I honestly was expecting to get a reply something like "This is a simple phenomenon associated with hierarchal control systems. Its a side effect exhibited by all systems and can be easily seen in general models of hierarchal control. To observe it yourself, all you have to do is build a four level hierarchy (each level consisting of three separate control loops) and set the parameters such and so ... blah blah blah.."

I can see now that my expectation was a bit naive and modeling the effect may not be a trivial matter after all. Still, I agree that the phenomenon may very well be a triviality and would not even have been explored by cognitive psychology if psychologists understood perceptual control theory.

Upon reflection I guess I was hoping to play the "nothing but" game with cognitive psychologists. Except I wanted to reverse the roles. It seemed to me that it would be a fun exercise publish a paper in Cognitive Psychology that would review the literature on representational momentum, show what the rival explanations were for

this effect and then show how its all accounted for as a side effect of the real thing... control of perception. Its "nothing but" a trivial side effect of perceptual control!

>It's much as if a person who failed to understand the purpose of  
>a piano took to plucking its strings, slamming its lid shut, or  
>tapping on the sounding-board in different places, looking for  
>some amusing or puzzling sound to be brought forth, never  
>thinking to try out the keys to see what they do. "Here," says  
>the experimenter, rubbing steel wool on a tuning peg and  
>listening with a stethoscope to the C# damper, "See if you can  
>explain this sound!"

Yes... and I wanted to be able to answer the representational momentum folks with an answer something like (keeping the metaphor): "Well, of course, what do you expect to hear when you rub a tuning peg with steel wool. Its simply a side effect of ....."

>We have in front of us a great feast of data (to switch  
>metaphors), spread out to our view in plain sight, waiting to be  
>consumed and turned into nourishment. It's impossible to look at  
>any human behavior without seeing evidence of how it all works.  
>The problem is not to find something to explain; it's only which  
>of the goodies before us to pick up first.

Bill, this is sheer poetry. I hope someone is archiving everything you write. We (I) will be pouring over it for years and years.

>What we are waiting for (to switch back) is for someone to try  
>out the keys and discover what this damned machine is really for,  
>and start making some sounds worth listening to.

Okay... Okay... your point is well said, and I agree 100% Still, as one interested in things psychological I can't help but be thrilled when I learn something new about how humans are built and how they operate -- including the, "oddities and seeming paradoxes, that occasional turn up if one is looking very closely at the details."

Let me give you an example. Many months ago you and Avery were having a discussion on modeling "Astro" a spacecraft trying to find its mother ship. You explained to Avery that he would get the results he was after if he modeled it a certain way. Well, it seemed easy to do so I wrote a program (actually I created a stack in HyperCard) with the specifications you said. When completed I noticed that it (the program) was mimicking how my 1 year old twins were running. It was really fun to watch them overshoot the target, oscillate back and forth, and do all sorts of "poor controlling." At one point I set up an experiment in which I asked my wife to pick up a ball and entice the twins to chase her. Her job was to run around the room just out of their reach, change random directions at random times, and vary her speed randomly. It was fun and the twins got a kick out of it (until they realized that the ball was not ending up in their hands like it should). At the end of the exercise I showed her the program for the first time and told her to use the mouse to move the "mother ship" around. She experimented for a few moments and then exclaimed, "Thats Andrew!" It (Astro) was mimicking the exact same locomotion strategies that Andrew used (and not Jared -- his twin brother). She could notice the subtle differences because she was ... well ... their mother and

I could not because I was ... well... new at understanding control theory. It was great to discover, and fun to watch. It was not too many weeks later and Andrew's chasing ability "changed" and he was no longer controlling the same things. (Or perhaps, the same control mechanisms had just become so much better tuned that his control was qualitatively different.

Anyway, sometimes there is joy in discovering the nuances, paradoxes and oddities of being human. Especially when one can observe and appreciate the underlying mechanisms that make us so. For me, PCT has become a fantastic tool to hear, see, and feel the music of life. So far, PCT is definitely "making sounds worth listening to."

Richard Thurman Air Force Armstrong Lab 6001 S. Power Rd. BLDG. 558 Mesa AZ.  
85206-0904 (602) 988-6561 Thurman@hrlban1.aircrew.asu.edu

Date: Fri Mar 18, 1994 2:58 pm PST  
Subject: Re: representational momentum

<[Bill Leach 940318.16:05 EST(EDT)] >[Richard Thurman (940317.0920)]

Richard, as you are quite aware, I am VERY new to all of this thus I do not know much of what has and has not been considered in terms of things like unintended effects in experiments.

My concern is that the vast majority of "Personal Computer" monitors create all images on the screen by "drawing" the image using a scanning electron beam from top to bottom and left to right and at a pretty much "fixed" refresh rate.

"Workstation" monitors typically refresh at much higher rates than do most PC monitors.

Also, things like display persistence varies with different monitors.

Since much of the experimentation with PCT is concerned with the subject's perception of moving images on the display screen it is quite possible that "directional response differences" could be providing perceptual information that is not intended.

I am curious if any consideration has been given to any possible effects?

-bill

Date: Sat Mar 19, 1994 12:32 pm PST  
Subject: BIOME server

[From Dag Forssell (940319 1200)] Bill Silvert (940317)

Bill, I do not mean to offend you, only to make sure the BIOME connection works.  
|:)

>The files called Index are now called 00index since some packages come  
>with Index files. This is common usage on servers.

This is major news, and a key to my latest problem. Sorry I have had to pull it out of you like pulling teeth. I don't mind that you are busy. I appreciate that you are making all the CSG files available and am working with you as best I can, given my reference signals. Please consider this posting to be a clarification and advertisement for your server from the perspective of an E-mail user. Those on Internet can access and navigate the index directories in a much easier, obvious, interactive way, but may still enjoy this report on what is available. I have made some comments and asked questions directed at you after some of the server replies.

Based on your info, I just sent the following message to your server:

To: SERVER@BIOME.BIO.NS.CA

```
help
get csg/00index
end
```

and got the following reply about the index:

```
-----
Subject: /msm/ftp/pub/csg/00index
```

```
Partial Index of Files, Fri Mar 18 03:21:35 AST 1994
Specify full filenames in the form "/pub/csg/filename"
```

```
00index          109
LFG/              - toy grammar development system
News              0 News items relevant to CSG
documents/        - Papers and other text items
mmt/              - Files provided by Martin Taylor
neuron/           - Neuron models for the PC from Duke U.
programs/         = Computer programs for CSG
reviews/          - Reviews distributed to CSG-LIST
simcon/           -
-----
```

Here is an extract from the reply to the "help" command. Bill, this is customized by you for your server. The information about 00index is nowhere to be found. It guess you would want to update this for all your other constituents, not just the CSG crowd.

```
-----
The commands are:
```

```
help          - get this message
ftp           - find out how to access this server by anonymous ftp
index        - get a list of files that are available*
....
```

There is also an Index file in most directories. Thus to get a list of unix programs send mail with the line "get unix/Index".

```
----- Examples -----
```

```
ftp          - retrieves information on using anonymous ftp
index        - retrieves the master system index
```

```

get Index      - retrieves the master index by name (actually pub/Index)
cd unix        - sets directory to pub/unix
uue dl.tar.Z   - retrieves file pub/unix/dl.tar.Z in uuencoded form
get Index      - retrieves file pub/unix/Index
get bsim/Index - error, file pub/unix/bsim/Index does not exist
get /bsim/Index - retrieves file pub/bsim/Index (note leading slash)
cd sys5        - sets directory to pub/unix/sys5
get Index      - error, pub/unix/sys5/Index does not exist
index          - retrieves the master system index (always)
-----

```

Based on this, I sent the following message:

```

get csg/documents/00index
get csg/mmt/00index
get csg/programs/00index
get csg/reviews/00index
get csg/simcon/00index
end

```

and got the following replies:

```

Subject: /msm/ftp/pub/csg/documents/00index
Partial Index of Files, Fri Mar 18 03:21:36 AST 1994
Specify full filenames in the form "/pub/csg/documents/filename"

```

```

00index          119
biblio.pct       88534 Williams PCT Bib, Vol. 6, 1993
csgintro.doc     17139 Introduction to CSG
heirarchy        48568 Hierarchical Behavior of Perception by Marken
mcclel.pct       163940 PCT and Sociological Theory by Kent McClelland
powers.pct       25184 Power's Primer on PCT (draft)

```

```

Subject: /msm/ftp/pub/csg/mmt/00index
Partial Index of Files, Fri Mar 18 03:21:36 AST 1994
Specify full filenames in the form "/pub/csg/mmt/filename"

```

```

00index          113
Control_Builder_1.3b6.sea
                    583168
csg.9206.sea     523776 Hypercard 2.1 stack of recent CSG postings
mailsplitter.bin
                    109440 Program to convert postings to Hypercard stacks
mmt.info         365 Info on Martin Taylor's postings

```

```

Subject: /msm/ftp/pub/csg/programs/00index
Partial Index of Files, Fri Mar 18 03:21:38 AST 1994
Specify full filenames in the form "/pub/csg/programs/filename"

```

```

00index          118
Warning          317
mac/             -
msdos/           =

```



source/ -

Subject: /msm/ftp/pub/csg/simcon/00index  
 Partial Index of Files, Fri Mar 18 03:21:46 AST 1994  
 Specify full filenames in the form "/pub/csg/simcon/filename"

00index	116		
copying	12488		
mycrt.c	1989		
primer1.doc	18768	19167	930517 Intro to PCT using Simcon, Part 1
primer2.doc	13547	13842	930517 Intro to PCT using Simcon, Part 2
primer3.doc	18031	18425	930517 Intro to PCT using Simcon, Part 3
primer4.doc	12952	930517	Intro to PCT using Simcon, Part 4
simcn45.ash	153779	155809	930517 "Burned" simcn45.exe. Use Unburn.
simcn45.exe	95913	95913	930517 Self-extracting Simcon 4.5 + docs
simcn451.uue	60004	60960	930517 Uuencoded simcn45.exe, part 1 of 3
simcn452.uue	59980	60935	930517 Uuencoded simcn45.exe, part 2 of 3
simcn453.uue	14758	14996	930517 Uuencoded simcn45.exe, part 3 of 3
simcon45.c	28157		
simcon45.doc	39822	40329	930504 Writeup of Simcon 4.5
simfiles.	588		
unburn.scr	29261		Debug script for converting simconz.ash

Most, but not all (the uue and ash, unburn parts) of what is in this directory is in the simcon.zip, part of the March demodisk. Should this directory be deleted??

Subject: /msm/ftp/pub/csg/reviews/00index  
 Partial Index of Files, Fri Mar 18 03:21:46 AST 1994  
 Specify full filenames in the form "/pub/csg/reviews/filename"

00index	117		
rosen.tex	31512		Life Itself... by Bob Rosen, rev. by Joslyn
self-mod.tex	30983		Self-modifying systems, George Kampis, rev. by Joslyn

I noted that the help file said:

index - retrieves the master system index (always)

so I decided to try it, and also to follow the trail of directories revealed in previous indexes. Here is my next message:

```
index
get csg/programs/msdos/00index
get csg/programs/mac/00index
get csg/programs/source/00index
end
```

I got the following replies:

Subject: /msm/ftp/pub/00index  
 Partial Index of Files, Fri Mar 18 03:20:05 AST 1994  
 Specify full filenames in the form "/pub/filename"

```

00index          105
ai/              - Artificial Intelligence
atari-st/        - Atari-ST
banyan/          = Banyan Vines support files
bod/             = Biol. Oceanogr. Div. files maintained by George
White
bsim/            = BSIM Simulation Package
cdrom/           - CD-ROM Information
csg/             = Control Systems Group
cstb/           = Canadian Society for Theoretical Biology
database/        = Database information, including SEARCH
ecosys/          = ECOSYS-L Mailing List and Archive
faq/             = Frequently Asked Questions
fish-ecology/    = Fisheries Ecology Archive
fortran/         = Fortran Code and Utilities
gnu/             - GNU Software Distributions
habitat/         = Habitat Ecology Programs and News
hed/             = Habitat Ecology Division Personal Directories
hp/             = HP Calculators
hypertext/       = HyperText programs and examples
incoming/        - Incoming files can be uploaded here
languages/       = Computer Languages
mac/            = Macintosh Programs
math/           - Mathematics
models/          = Spreadsheets and other ecological models
msdos/           = MS-DOS Software
net/            = Networking Programs
oops/           - Object-oriented Programming
perl/           = Perl information and utilities
portfolio/       - Programs for the Atari Portfolio computer
postscript/      = PostScript
public/          - scratch directory for file transfers
sgi/            = SGI-specific Software
sgi.old/         =
text/           - Text processors, etc.
uniform/         = Uniform-Atlantic information
unix/           = Unix Software
x/              - X11 Applications

```

This shows that the command "index" as shown in the "help" file is valid, and allows you to find everything. Here, we get a hint about 00index.

```

Subject: /msm/ftp/pub/csg/programs/source/00index
Partial Index of Files, Fri Mar 18 03:21:45 AST 1994
Specify full filenames in the form "/pub/csg/programs/source/filename"

```

```

00index          125
lfilter.c        7746 Bill Power's Tracking Filter
uud.c            13750 Enhanced decoder, C source
uue.c           4734 Enhanced encoder, C source

```

Subject: /msm/ftp/pub/csg/programs/mac/00index  
 Partial Index of Files, Fri Mar 18 03:21:38 AST 1994  
 Specify full filenames in the form "/pub/csg/programs/mac/filename"

00index 122

Subject: /msm/ftp/pub/csg/programs/msdos/00index  
 Partial Index of Files, Fri Mar 18 03:21:39 AST 1994  
 Specify full filenames in the form "/pub/csg/programs/msdos/filename"

00index	124	
arm1/	-	
arm2/	=	
crowd/	-	
dem1a.exe	128437	Bill Power's demonstration of perceptual control
dem2a.exe	123649	Bill Power's modelling of control
demo1/	-	
demo2/	-	
ecoli/	-	
extrct32.zoo	89013	UUDecoder for Windows or DOS
forssell.zip	1421589	PCT Demos from Dag Forssell
mindread/	-	
pctdocs/	-	
simcon/	-	
sprdsht/	=	
uud.exe	23449	DOS enhanced decoder
uue.exe	17062	DOS uuencoder

Here, I believe that dem1a.exe and dem2a.exe are somewhat obsolete in that they show an old address for Bill Powers. The files included in my demodisk, demo1.exe and demo2.exe are fully equivalent, self-extracting files, but updated with current address and without request for \$ contribution, since Bill waived that in December 93.

Bill, my thought was that you might want to replace dem1a.exe above with the demo1.zip from the March disk alternate. I note that you have defined the subdirectories in the demodisk above and below the file forssell.zip. What do the signs - and = mean? The pctdocs/ directory, for example, is empty, right?

My vision of this directory as updated with options, would be:

00index	124	(what does this number mean?)
forssell.zip	1421589	PCT Demos, March 94 demodisk from Dag Forssell
arm1.zip	90049	PCT Demos, March - subdirectory: arm1
arm2.zip	140570	PCT Demos, March - subdirectory: arm2
crowd.zip	166278	PCT Demos, March - subdirectory: crowd
demo1.zip	90049	PCT Demos, March - subdirectory: demo1
demo2.zip	95015	PCT Demos, March - subdirectory: demo2
ecoli.zip	83029	PCT Demos, March - subdirectory: ecoli
mindread.zip	71443	PCT Demos, March - subdirectory: mindread
pctdocs.zip	219666	PCT Demos, March - subdirectory: pctdocs
simcon.zip	115248	PCT Demos, March - subdirectory: simcon
sprdsht.zip	371783	PCT Demos, March - subdirectory: sprdsht

extrct32.zoo	89013	UUDecoder for Windows or DOS
uud.exe	23449	DOS enhanced decoder
uue.exe	17062	DOS uuencoder

>I unzipped the disk that Dag sent me, the individual files are all  
>there. This is not the March 15 disk, I don't have that yet.

Bill, I understand that the files are all there. My only concern was for netters with slow modems, who might choke on the 1.42 megabytes shown above (1421589 is zip size for February disk). With the arrangement shown above, you can have the best of both worlds.

>I do not have time to do major housecleaning as Dag requests. I did  
>some, but I really would like someone else to take over the job.

Back in November 1992, you made some changes easily once I made specific suggestions. I hope this post has been helpful to you, Bill, not a source of frustration, and an effective advertisement at the same time.

-----

Gary, I suggest that the CSGintro doc BIOME section be reduced (removing some obsolete particulars) and updated to this:

-----

#### HOW TO OBTAIN TEXT AND PROGRAM FILES

A number of ASCII documents and MS-DOS and Macintosh computer programs are available on a fileserver maintained by Bill Silvert. These files can be obtained via anonymous FTP, Gopher, World Wide Web, and e-mail.

#### ANONYMOUS FTP

For anonymous FTP access, connect to biome.bio.ns.ca, logon as anonymous, giving your e-mail address as your password. The CSG files can be found in the directory pub/csg.

Several programs of particular interest for MS-DOS machines can be found in the directory pub/csg/programs/msdos. These are self-extracting files and include Bill Powers's demonstrations of the phenomenon of control and the perceptual control theory model of behavior (demo1 and demo2). Also of interest is the complete PCT bibliography compiled by Greg Williams. This file can be found in the directory pub/csg/documents as biblio.pct.

-----

#### E-MAIL

Document files and uuencoded versions of program files can also be obtained via e-mail. Here are some basic commands for obtaining files and information:

To: SERVER@BIOME.BIO.NS.CA

```
Commands:  help
           get pub/csg/00index
           end
```

"help" requests commands and explanations.  
"get pub/csg/00index" requests index for the csg subdirectory.

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

The pub/csg/00index you receive in response will show additional directories and enable you to send messages to find out what is available in your area of interest.

#### REFERENCES

-----

Once all this is squared away, and Gary has updated the CSGintro.doc (April?), we can ask Bill to update the csgintro.doc in the pub/csg/documents directory.

-----

Best to all, Dag

Date: Sun Mar 20, 1994 8:23 am PST  
Subject: Re: BIOME server

OK, more time on clarification.

>Bill, I do not mean to offend you, only to make sure the BIOME connection  
>works. |:)

Biome works fine, I am sorry that I did not adequately update the information when I renamed the Index files 00index. Since the CSG group is probably the only one that uses the mail server, I don't get much feedback.

>>The files called Index are now called 00index since some packages come  
>>with Index files. This is common usage on servers.

>

>This is major news, and a key to my latest problem. Sorry I have had to  
>pull it out of you like pulling teeth.

Since most users of my server look at the directory contents (as with ftp), I hadn't realized that the change would be so difficult to spot. Sorry.

>Here is an extract from the reply to the "help" command. Bill, this is  
>customized by you for your server. The information about 00index is  
>nowhere to be found. It guess you would want to update this for all your  
>other constituents, not just the CSG crowd.

It is now updated. Actually Dag is the only regular user of the server. No one else has accessed it since March 4, and that was someone who had asked me how to get a specific paper of mine. Aside from people looking for that paper, no one outside CSG has accessed the server since Oct. 11.

>Most, but not all (the uue and ash, unburn parts) of what is in this  
>directory is in the simcon.zip, part of the March demodisk. Should this  
>directory be deleted??

Damned if I know.

>This shows that the command "index" as shown in the "help" file is  
>valid, and allows you to find everything. Here, we get a hint about  
>00index.

>Here, I believe that dem1a.exe and dem2a.exe are somewhat obsolete in  
>that they show an old address for Bill Powers. The files included in  
>my demodisk, dem01.exe and dem02.exe are fully equivalent,  
>self-extracting files, but updated with current address and without  
>request for \$ contribution, since Bill waived that in December 93.

>

>Bill, my thought was that you might want to replace dem1a.exe above  
>with the dem01.zip from the March disk alternate. I note that you  
>have defined the subdirectories in the demodisk above and below the  
>file forssell.zip. What do the signs - and = mean? The pctdocs/  
>directory, for example, is empty, right?

Without going into details about how everything works (there are codes for  
directories, etc.), I'm afraid that I cannot provide full support for learning  
about how this system works and what everything means. If anyone else wants to  
take over the tutorial function, that would be fine by me.

I append the manual entry for the dl program which biome uses to list directories  
and to generate the 00index files:

DL(1) UNIX System V (2 Dec, 1992) DL(1)

NAME

dl - Descriptive ls

SYNOPSIS

dl [options] [file ...]

DESCRIPTION

Dl lists files and directories in the manner of ls(1), but includes a descriptive comment for each file that has a description set. By default, dl lists the file name, size of the file in bytes and the description. If the file is a directory, a hyphen (-) is shown instead of the size, and if the file is a directory that contains other directories, an equals sign (=) is shown.

Descriptions are set by describe(1) and are stored in a hidden file in the same directory as the files for which descriptions are held.

Options:

-d List the date and time of each file. The last

modification date and time are listed after the file size, in the same format used by ls(1).

- e List everything, even inaccessible files. By default, files that cannot be read or executed are ignored.
- t Sort by last modification time. Most recently modified files come first.
- fwidth  
Use a maximum of width columns to display the file name. If a file name is longer than width, it may expand into the area used to show the size. If this overflows, the size and other information will be listed on a separate line.
- R Recursively list any subdirectories encountered, just like ls(1).

#### FILES

For each directory, a .desc.pag and .desc.dir (DBM files) are used to store descriptions.

#### SEE ALSO

ls(1)

Date: Sun Mar 20, 1994 9:19 am PST

Subject: TIME & CONSCIOUSNESS-RKC

<Bob Clark (940320.1215 EST)> Rick Marken (940316.1000)

#### TIME

In your discussion of the operation of the reorganizing system, you suggest (obvious typos fixed):

>When the error is not close to zero error, the subject (like the >reorganizing system) hits the "change" bar. The result may be that >the error starts to get bigger or smaller; if it's the former then >the subject hits the change bar again immediately; if it's the >latter, the subject waits to see how things go.

This strongly implies that time is a controllable variable. I attempted to demonstrate that last summer at Durango. However it seems to have been very rare for anyone to make use of this observation.

Temporal variables are involved in a great many of the higher level perceptual control systems.

#### CONSCIOUSNESS

You further state:

>I believe that Bill understood the problem right off the bat and >suggested that one role of CONSCIOUSNESS is to DIRECT reorganization >to the control systems that NEED it.

Interesting --

does this remind you of some of my discussions of the DME?

Regards, Bob Clark

Date: Sun Mar 20, 1994 11:15 am PST

Subject: MEMORY/MOMENTUM - RKC

<Bob Clark (940320.1400 EST)> Bill Leach (940316.20:07 EST)

Following your graphical display, you suggest:

>what might be happening is that the perceptual image is created  
>from the matrix of visual impulses compared to appropriate  
>references chosen from some sort of image memory.

Isn't this equivalent to suggesting that perceptions (here, "visual") can be recorded and "played back" at a later time (perhaps milliseconds later) as "imagined" perceptions? The neurological details may be unknown, but the experiences are familiar.

Similar continuing experiences are well known -- "after images" of various sorts, "flicker fusion" studies, astronomers searches for moving objects by alternating time-displaced photographs of the same stellar area. I am sure the Netters can supply many more.

MOMENTUM

I know that you did not invent the term "Representational Momentum," but your discussion leads me to remarking, as a physicist, that I am troubled by those who "borrow" terms from physics and grossly misuse them. Such verbal gymnastics are invariably more confusing than helpful.

"Momentum" is the product of mass and velocity. It has both magnitude and direction (a "Vector").

The descriptions of "Representational Momentum" imply a continued existence of one (or more) perceptual signals after the external source is no longer available. This has nothing whatever to do with "momentum" as used in physics.

Instead, it directly relates to temporal duration of perceptual signals within the brain. To me, a much better way to treat this phenomenon would be in terms of "recordings" or "memories." The original (1960) papers specifically included such recording capability.

Regards, Bob Clark

Date: Sun Mar 20, 1994 4:16 pm PST

Subject: Time and consciousness

[From Rick Marken (940320.1600)]



A major aftershock, experienced near the epicenter (I was visiting family in the valley) has waked me from my dogmatic slumbers (or roused me back into my dogmatic wakefulness).

Chuck Tucker -- I don't have your e-mail address here at home so, if you get this today (3/20), send your address to me at [marken@aero.org](mailto:marken@aero.org) I'll send you the latest and greatest version of the "Cause of Control" experiment.

Bob Clark (940320.1215 EST)--

>This strongly implies that `_time_` is a controllable variable.

Not really. The E. coli navigation process works without any explicit representation of time. Also, I don't see how time, per se, can be perceived. Time can be controlled only if it can be perceived; how do we perceive time? Is it perceptible?

>Temporal variables are involved in a great many of the higher level perceptual control systems.

Yes. And I think what is controlled in these cases is something like partial derivatives --  $dx/dy$ . I think Bill P. suggested something like this long ago but I can't recall where he discusses it -- probably BCP.

I said:

>I believe that Bill understood the problem right off the bat and  
>suggested that one role of CONSCIOUSNESS is to DIRECT reorganization  
>to the control systems that NEED it.

Bob replies:

>Interesting -- does this remind you of some of my discussions of the DME?

Yes, I suppose it does. So the DME is the reorganization system? Is there more to it than that? If so, could you explain HOW the DME directs reorganization? Do you have data relevant to how this directing occurs? Some algorithms and some data would make the DME a LOT more interesting to me.

Best Rick

Date: Sun Mar 20, 1994 6:50 pm PST

Subject: Re: BIOME server

<[Bill Leach 940320.20:06 EST(EDT)] >Bill Silvert <[bill@BIOME.BIO.NS.CA](mailto:bill@BIOME.BIO.NS.CA)>

>It is now updated. Actually Dag is the only regular user of the server.

>No one else has accessed it since March 4, and that was someone who ...

Bill, I have tried to access the server but have not been successful. I can't say as I have been terribly persistent but I have tried a couple of times.

-bill

Date: Sun Mar 20, 1994 6:51 pm PST  
Subject: Re: MEMORY/MOMENTUM - RKC

<[Bill Leach 940320.20:11 EST(EDT)] ><Bob Clark (940320.1400 EST)>

I have no problem with the idea that "visual" images are recorded and can be "played back" later.

All that I was driving at is that the phenomenon of "continued motion" after the object has dissappeared could be due to the processing of the existance of multiple fading images and that these multiple fading images provide an "indication" of motion to the subject. When the object dissappears, there are still mulitple images at various intensities continuing to fade at the previous rate even though no new images are being created (for the object of interest). It seems to me that this is a possible source for the incorrectly perceived "final position."

You are right, I did not (and would not have) invented the term.

-bill

Date: Sun Mar 20, 1994 11:55 pm PST  
Subject: Bill L. got shook

[Dag Forssell (940320 1150)] [Bill Leach 940321.00:44 EST(EDT)]

>That (BCP) has to be the most profound thing that I have ever read.

Bill,

Welcome (again) to PCT and CSGnet. It shall be a pleasure to continue our dialog.

Best, Dag

Date: Mon Mar 21, 1994 7:40 am PST  
Subject: Re: I have been shaken

[Dan Miller (940321)]

Bill Leach:

Despite my sometimes dyspeptic posts on CSG-Net, I, too, had the same reaction after reading BCP. It immediately shot to the top of my "really good stuff" list and has remained there for twenty years.

About BCP - Is it possible to get it reprinted? I recommend it to lots of people, and some actually get it and read it. However, I note that Aldine is down to the last few copies. This is criminal.

Better yet, I suppose, would be an update. This is easy for me to say; I don't have to do it. It is a suggestion, however. Perhaps with a new edition we could get it marketed and reviewed in journals, newspapers, etc. The latter I could do,

and with considerations I would write good reviews. (I'm kidding about the considerations part.)

Later, Dan Miller MILLERD@UDAVXB.OCA.UDAYTON.EDU

Date: Mon Mar 21, 1994 8:48 am PST  
From: Bill Silvert  
TO: \* Purposeful Leadership / MCI ID: 474-2580  
CC: CZIKO Gary  
Subject: Disks

Look, I don't have time to follow all the instructions and work through all the disks moving stuff from the PC to biome and putting everything in the right places. Please do the following:

Create a recursive zip archive of everything so that I can move the one file to biome and unzip it to install the entire directory tree.

If there are directories to be deleted, just send a listing and mark DELETE. There is no point sending suggestions, I'm not going to get involved in planning and organizing the server.

Keep in mind that you are practically the only person who uses this server anyway!

Bill

Date: Mon Mar 21, 1994 8:50 am PST  
Subject: Re: BIOME server

><[Bill Leach 940320.20:06 EST(EDT)]

>

>Bill, I have tried to access the server but have not been successful. I  
>can't say as I have been terribly persistent but I have tried a couple of  
>times.

System operators just love messages that say "there is something wrong with your server."

If anyone is having problems with biome, send a message with time, date, and a full description of the problem. I don't even know whether Leach is referring to the mail, ftp, or gopher server!

How about a PCT paper on problem diagnosis and correction?

Bill

Date: Mon Mar 21, 1994 3:45 pm PST  
Subject: Forsell demos

I got a couple of disks from Dag Forsell, one was defective, but everything seemed to be in ZIP archives on the other disk so I've installed that on biome. All files are in directory csg/pctdemos and each set of files is available both as a ZIP

archive and as an expanded directory. I got rid of a bunch of other files and edited csgintro.doc to change Index to 00index. Hope this helps.

Bill Silvert [940321]

BIOME SysOp, Habitat Ecology Division, Bedford Institute of Oceanography.  
P. O. Box 1006, Dartmouth, Nova Scotia, CANADA B2Y 4A2, Attn. Bill Silvert  
InterNet Address: sysop@biome.bio.ns.ca Phone: (902)426-1577.

Date: Mon Mar 21, 1994 4:41 pm PST  
Subject: Visual Imagining Experience

<[Bill Leach 940321.17:52 EST(EDT)] >Bill Powers

Bill;

Mark was more prompt than I though he would be. He actually gave this to me a couple of days ago. I told him that I would review it and post. I don't know if there is anything "new" in this beyond what I already relayed or not but here it is anyway.

If you wish to ask any questions of Mark, I would be glad to be an intermediary as he does not presently have internet access (an unusual condition for him).

Text follows:

Some years ago, I was involved in a computer vision project, when I stumbled across a rather startling demonstration of the human visual system, and the fact that our vision system derives a tremendous amount of information from 3-D motion analysis. Intellectually, I already knew this (anyone who's done any computer vision work realizes that the human visual system has an amazing ability to extract and extrapolate information from raw image data, and current computer vision techniques are a long way from matching our own vision system).

First off, some background information: I was working on a project which involved 3-D vision, with the purpose of analyzing parts on an assembly line to identify defects (errors in assembly) before those parts made it to later assembly stages. What the computer actually saw were "range images" of the objects on the assembly line.

Note:

A range image is a 2-D image where the individual pixel values represent depth information, in my case a simple  $Z = f(X,Y)$  relation, where (X,Y) are the pixel coordinates (contrast this to an ordinary picture where the imaging function  $f(X,Y)$  yields color and brightness values instead). Hence, a range image can be viewed as being like a topographical map, where X,Y equate to longitude and latitude, and Z is the elevation.

[Other types of range image are possible, such as where the X,Y values represent angular information, and the imaging function gives the distance to the observer. Consider an airport radar system, which would use such a "perspective" projection system: distance as a function of attitude and elevation. I mention this just to point out that "range image" covers a variety of similar classes of image, and not specifically an isometric X,Y,Z relation.]

Hence, a range image provides you with true 3-D information about some surface (unlike say a binocular imaging system which \*infers\* 3-D from a pair of 2-D images). For our purposes here, I will just refer to this as 3-D vision, although purists may argue that true 3-D analysis would involve an imaging function  $P = f(X,Y,Z)$ , where for example P represented density information for points (X,Y,Z) inside a solid object.

Now, I routinely displayed these range images on our graphics system, where the pixel value was usually used directly as a brightness value. Hence, I'd see a grey scale picture, where the brighter a point was, the higher it was. Now, since I was used to looking at these images, and knew what I was looking at, I could study such an image and tell pretty much what was there -- although the computer could sometimes do a better job than I could!

One day, for reasons I no longer remember (I think I was trying to generate some pretty pictures for a glossy brochure), I decided that I wanted to view the data "from a different angle", both literally and figuratively. Since the image data essentially represented a set of points in three-space (a set of X,Y,Z triplets), I decided that I could do a projection of the data points to make a new picture, where the viewpoint was from some other location, such as off to the side rather than directly overhead. Hence, I converted the range image into a projection image of a surface (I believe I used a simple isometric projection to create the images, although I may have used a perspective projection instead - I don't remember).

Now, this worked pretty well. By transforming the image into something resembling closer to what our ordinary visual system uses (a 3D projection onto 2D), I created an image which was easier for the average person to understand. Now they could look at the image and "see" the objects in it. Not too surprising a result -- this was what I was trying to do in the first place. But now the fun began ....

The natural problem which cropped up when trying to generate one of these images was that of trying to determine the "best" position from which to view the data. So I started generating different images, to pick one which looked particularly appealing (in a purely aesthetic sense), as well as to find a viewpoint which made it easiest to see and visually analyze the objects in the image. In fact, it turned out that different viewpoints each had their advantages and disadvantages, as a feature which became highlighted in one view may become obscured in another view, while a previously obscured feature now became easily visible. Again, not too surprising a result. Still, I wanted to pick one or two particularly "good" views.

Now, although these images were much easier to look at and interpret than the original range images, it should be noted that they were still not the easiest things to examine. Since I had no information in the original images regarding the nature of the surfaces I was looking at (other than distance to various points), the projection images weren't any better. There was no color or texture or even normal brightness information for instance. The projected images just showed boxes or plates or wire-frames in place of the original pixels. I believe that I still showed pixels in the background more dimly than those in the foreground (giving somewhat the illusion that the view was lit from the front), but this is not too significant. Although the projected images were much easier to understand than the original range images, they still had to be studied a bit to really get a feel for what you were looking at, particularly when looking at smaller details.

So I started playing with different viewpoints to show the images from. I found that looking at the objects from an elevated side view was a good start -- say, lowering the viewpoint to 30-45 degrees, rather than the original directly overhead view of 90 degrees elevation. So I just arbitrarily picked some intermediate elevation angle. I then set out to pick a good azimuth angle (moving to the left or the right). So I generated a sequence of images in relatively small angular steps (probably about 5 degrees). I generated enough views such that I covered all sides, that is, a full 360 degree sweep of my viewpoint around the object.

The astonishing thing came when I started to display these images in sequence, at high speed, so that the individual images became frames in an animation. Now, instead of looking at a set of still images from different angles, I instead saw my object rotating in front of me . . . and this is where I got my shock!

All of a sudden, every nook and cranny and oddity of my surface became instantly and obviously visible. \*It was no longer necessary to study the image. Everything was immediately obvious, at an intuitive level.\*

Our human visual system is great at analyzing still pictures, much better than any computer program so far has become, yet this is obviously nothing compared to its ability to analyze \*moving\* imagery. By adding the element of motion (in three-space) to the images, I was able to invoke a whole new aspect of my visual system, so that the recognition of objects and surfaces became "trivial". No new data had been introduced, all of the information in all of the projected images came from the original range image. In fact, each individual frame involved a \*loss\* of some data, since changing the perspective angle caused some things to become obscured. I could look at a whole set of these individual frames and recover this "lost" information, but it still required a fair amount of inspection and conscious analysis of the images. But when motion was introduced, what I was looking at became obvious. I even noticed things about the image which I had never particularly noted in the originals (to my chagrin!).

Now, when you read this, you may well react with a "so what?" type of attitude. Conceptually, the observations described here aren't really that surprising -- at an intellectual level. The fact that our visual system derives a lot of information from a variety of sources, including visual cues related to motion and spatial perspective changes, is hardly surprising. Still, the actual result when I did this experiment came as a shock, because of the way the data in the image suddenly jumped out at me when I rotated the images in `real-time'. Now I knew a fact deep down at the emotional level -- I \*felt\* it rather than merely "knowing" it. [Ahhh! ... the thrill of discovery!]. I also felt now that this information had a lot more use than as mere academic curiosity. In fact, two things in particular now have piqued my interest, and suggest themselves as future avenues of study:

I believe that there is probably a fundamental clue here about our 3D visual system and how it compares to anything we've done with computers. It strongly suggests to me that some sort of 3D analysis mechanisms are built into our "hardware", and that we may well derive a lot more information from motion analysis than we do from static image analysis (such as looking at edges and textures). Obviously, we do the latter forms of analysis too, probably also partially at a "hardware" level, but perhaps its fundamental importance is overrated.

I also wonder at the possible applications for such information. How many of us have looked at things like X-ray photographs, CAT-scans, or other such image which meant very little to us but meant a lot to a trained observer? Could similar techniques which exploit the properties of our visual system make the information in these images readily apparent to the casual observer (and all the more useful for the trained observer)?

-----cut here-----

-bill

Date: Mon Mar 21, 1994 5:37 pm PST  
Subject: Re: BIOME server

<[Bill Leach 940321.18:55 EST(EDT)] >Bill Silvert <bill@BIOME.BIO.NS.CA>

Bill;

<sheepish grin>

Sorry, I was not trying to report a problem since it had been too long since I last tried.

I know that I had trouble with the name server resolving "biome.bio.ns.ca" but that should not be your problem. Currently (ie: at this very moment, I am trying again to access the server) but am having dismal success. However, again it looks like internet problem and not server problems.... ie: I logged in ok but performance is miserable and downloads mostly fail (timeout).

-bill

Date: Mon Mar 21, 1994 5:37 pm PST  
Subject: Re: Bill L. got shook

<[Bill Leach 940321.18:27 EST(EDT)] >[Dag Forssell (940320 1150)]

Thanks Dag. One sure can wish that there really were more than 36 hours in any one day... I mean if there were only... say and even 40 then I could read a lot more of this material that I have on PCT that much sooner!

I know that "elegant" is not sufficient reason to "buy into" a theory but still... The sheer beauty of the theory... The ability to explain "normal" behaviour, common "abnormalities" and probably even behavioural problems related to physiological "defects"... To see how control systems in conflict (internal-internal or internal-external) can adequately explain observed behaviour...

To rationally explain what is usually termed "irrational" behaviour as a function of reorganization... The "over-powering" effects of errors associated with "intrinsic"...

I may well not be able to "look" at an interpersonal relationship the same way that I used to... ever again. There is a lot to think about in this.

-bill

Date: Mon Mar 21, 1994 6:46 pm PST  
Subject: MEMORY/MOMENTUM - RKC

[Micheal Fehling 940321 1:39 PM PST] In re <[Bill Leach 940320.20:11 EST(EDT)]

> I have no problem with the idea that "visual" images are recorded and can  
> be "played back" later.

Speaking as one who did a great deal of work in this area some time ago, there is actually a significant problem with this claim. The evidence heavily suggests that visual imagery is not "recorded" (at least not faithfully) and is reconstructed rather than "played back."

There is one exception--so called eidetic imagers who seem to indeed to be able to faithfully store and remember exact visual patterns. However, eidetic imagery is more of an interesting anomaly than a basic phenomenon in terms of which to understand human memory.

-- Prof. Michael Fehling, Director --  
Laboratory for Intelligent Systems  
Dept. of Engineering-Economic Systems  
321 Terman Center  
Stanford University  
Stanford, CA 94305-4025  
phone: (415) 723-0344  
fax: (415) 723-1614  
email: mrf@lis.stanford.edu

Date: Mon Mar 21, 1994 5:38 pm PST  
Subject: Re: MEMORY/MOMENTUM - RKC

<[Bill Leach 940321.19:00 EST(EDT)] >[Michael Fehling 940321 1:39 PM PST]

I think you noticed that I quoted "played back". That was no accident (and that was even BEFORE I read the "memory operations" section of BCP).

-bill

Date: Mon Mar 21, 1994 5:38 pm PST  
Subject: Re: I have been shaken

<[Bill Leach 940321.18:49 EST(EDT)] >[Dan Miller (940321)]

>twenty years.

I hope that I too will hold it in esteem for that long at least.

I know that I will have to re-read it at least several times but I want to get through as much of the PCT literature as quickly as I can first.



I must agree that Ed's books (at least "Love Guarateed") are excellent for a "real world" feel for PCT.

I find it hard to believe that anyone's explanation of what PCT IS though could be any better than Bill's.

-bill

Date: Mon Mar 21, 1994 7:09 pm PST  
Subject: BCP on the Internet?

[from Gary Cziko 940321.1716 GMT]

A previously shaken Dan Miller (940321) says:

>About BCP - Is it possible to get it reprinted? I recommend it to  
>lots of people, and some actually get it and read it. However, I  
>note that Aldine is down to the last few copies. This is criminal.

So since when are we limited to the archaic print media? This is 1994. Why not put Behavior: The Control of Perception on the Internet to share with the world?

There are already hundreds of books on the Internet as part of Project Gutenberg, mostly older classics. They can be downloaded via FTP, Gopher, Whole World Web, Mosaic, etc. Volunteers scan and/or type there favorites into text files. Why couldn't the same be done for a modern classic? I even believe that there is one very active, dedicated, and fanatical PCTer who has a scanner and might even be crazy enough to want to lend his sevices to such a project. (Hi, Dag!).

Of course, it would still be great to have the book reprinted and/or updated. But whether or not it is, the Internet access is still (I think) a great idea.

What do you think, Bill P.? Can you get copyright clearance for this? I might be able to get room on a local machine with Gopher access for the files, unless of course Bill S. insists on organizing the entire project on his BIOME server ;-)

--Gary

P.S. To take a look at Project Gutenberg, those who can do FTP magic should check out mrcnext.cso.uiuc.edu: pub/etext. Here is a partial index of what's available.

++++  
This is an index of the first 100 Project Gutenberg Etexts [gutindex.100] mh

[Pre-1991 etexts are now in> cd /etext/etext90,  
[These 199x etexts are now in> cd /etext/etext9x]  
[Do a dir \*.zip or dir \*.txt to see exact names.]

[Short index is updated every day. . .get 0INDEX.GUT from /etext/articles]

Mon Year	Title/Author	[filename.ext]	##
Jan 1994	The Complete Works of William Shakespeare [LOF]	[shaks10x.xxx]	100C

Jan 1994	Ludwig van Beethoven, 5th Symphony in c-minor #67	[lvb5s10x.xxx]	99
Jan 1994	A Tale of Two Cities, by Charles Dickens [CD#1]	[2city10x.xxx]	98
Jan 1994	Flatland, by Edwin A. Abbott [Math in Fiction]	[flat10xx.xxx]	97
Jan 1994	The Monster Men, by Edgar Rice Burroughs	[monst10x.xxx]	96
Dec 1993	The Prisoner of Zenda, by Anthony Hope	[zenda10x.xxx]	95
Dec 1993	Alexander's Bridge, by Willa Cather [Cather #3]	[alex10x.xxx]	94
Dec 1993	Tom Sawyer Detective, Mark Twain/Clemens/Wiretap	[sawr310x.xxx]	93
Dec 1993	Tarzan, Jewels of Opar, Burroughs [Tarzan #5]	[tarz510x.xxx]	92
Nov 1993	Tom Sawyer Abroad, Mark Twain/Clemens/Wiretap	[sawy211x.xxx]	91
Nov 1993	Son of Tarzan, Edgar Rice Burroughs [Tarzan #4]	[tarz410x.xxx]	90
Nov 1993	NAFTA, Treaty, Annexes, Tariffs [from September]	[naftxxxx.xxx]	89
Nov 1993	Price/Cost Indexes from 1875 to 1989 [Est to 2010]	[price10x.xxx]	88
Oct 1993	The World Factbook, US CIA, 1993 Edition	[world93x.xxx]	87
Oct 1993	A Connecticut Yankee, Mark Twain/Clemens, Wiretap	[yanke10x.xxx]	86
Oct 1993	Beasts of Tarzan, Edgar Rice Burroughs [Tarzan #3]	[tarz310x.xxx]	85
Oct 1993	Frankenstein/Mary Wollstonecraft Shelley	[frank10x.xxx]	84
Oct 1993	Frankenstein/Mary Wollstonecraft Shelley [italic]	[frank10a.xxx]	84a
Sep 1993	From the Earth to the Moon, Jules Verne	[moonxxxx.xxx]	83
Sep 1993	Ivanhoe/Scott/OBI/Wiretap [US only please]	[ivnh10x.xxx]	82
Sep 1993	Return of Tarzan, Edgar Rice Burroughs [Tarzan #2]	[tarz210x.xxx]	81
Sep 1993	The Online World/de Presno [Shareware]	[online11.xxx]	80C
Aug 1993	Terminal Compromise/NetNovel, Win Schartau	[termc10x.xxx]	79
Aug 1993	Tarzan of the Apes, Edgar Rice Burroughs [Tarzan#1]	[tarzn10x.xxx]	78
Aug 1993	House of the Seven Gables, Nathaniel Hawthorne	[7gabl10x.xxx]	77
Aug 1993	Adventures of Huckleberry Finn, Mark Twain/Wiretap	[hfinn10x.xxx]	76
Jul 1993	Email 101 by John Goodwin	[email025.xxx]	75C
Jul 1993	Adventures of Tom Sawyer, Mark Twain/Wiretap	[sawyr10x.xxx]	74
Jul 1993	Red Badge of Courage, Stephen Crane	[badge10x.xxx]	73
Jul 1993	Thuvia, Maid of Mars [Mars #4]	[mmars10x.xxx]	72
Jun 1993	Civil Disobedience, Henry David Thoreau	[civil10x.xxx]	71
Jun 1993	What Is Man? Mark Twain [Samuel L. Clemens]	[wman10xx.xxx]	70
Jun 1993	The 32nd Mersenne Prime, Predicted by Mersenne	[32pri10x.xxx]	69
Jun 1993	Warlord of Mars, Edgar Rice Burroughs [Mars #3]	[wmars10x.xxx]	68
May 1993	Black Experience, Norman Coombs	[blexp10x.xxx]	67C
May 1993	The Dawn of Amateur Radio, Norman F. Joly	[radio10x.xxx]	66C
May 1993	The First 100,000 Prime Numbers	[prime10x.xxx]	65
May 1993	Gods of Mars, Edgar Rice Burroughs [Mars #2]	[gmars10x.xxx]	64
Apr 1993	The Number "e" [Natural Log]	[ee610xxx.xxx]	63
Apr 1993	A Princess of Mars Edgar Rice Burroughs [Mars #1]	[pmars10x.xxx]	62
Apr 1993	The Communist Manifesto, Karl Marx/Friedrich Engels	[manif10x.xxx]	61
Apr 1993	The Scarlet Pimpernel, Baroness Emmuska Orczy	[scarp10x.xxx]	60
Mar 1993	Descartes' Reason Discourse, Rene Descartes	[dcart10x.xxx]	59
Mar 1993	Paradise Regained, John Milton	[rgain10x.xxx]	58
Mar 1993	Aladdin and the Magic Lamp, Traditional	[alad10xx.xxx]	57
Mar 1993	NREN, by Jean Armour Polly	[nren210x.xxx]	56C

Feb 1993	The Wonderful Wizard of Oz, L. Frank Baum	[wizoz10x.xxx]	55
Feb 1993	The Marvelous Land of Oz, L. Frank Baum	[ozland10.xxx]	54
Feb 1993	LOC Workshop on Etexts, US Library of Congress	[locet10x.xxx]	53
Feb 1993	The Square Root of Two	[2sqrt10x.xxx]	52
Jan 1993	Anne of the Island, Lucy Maud Montgomery [GG#3]	[iland10x.xxx]	51
Jan 1993	Pi [circumference/diameter]	[pimil10x.xxx]	50
Jan 1993	Surfing the Internet, Jean Armour Polly	[Surf10xx.xxx]	49C
Jan 1993	The World Factbook, US CIA, 1992 Edition	[world192.xxx]	48
Jan 1993	Clinton's Inaugural Address, US Pres Bill Clinton	[clintonx.xxx]	na
Dec 1992	The Gift of the Magi-O Henry [This is too short to zip, and will join xmasx.xxx]	[magi10.txt]	na
Dec 1992	Anne of Avonlea, Lucy Maud Montgomery [GG#2]	[avon10xx.xxx]	47
Dec 1992	A Christmas Carol Charles Dickens	[carol10x.xxx]	46
Nov 1992	Anne of Green Gables, Lucy Maud Montgomery [GG#1]	[anne11xx.xxx]	45
Nov 1992	Song of the Lark, Willa Cather [Cather #2]	[song10xx.xxx]	44
Oct 1992	Dr. Jekyll and Mr. Hyde #2 Robert Louis Stevenson	[hydea10x.xxx]	43
Oct 1992	Dr. Jekyll and Mr. Hyde #1 Robert Louis Stevenson	[hyde10xx.xxx]	42
Oct 1992	The Legend of Sleepy Hollow, Washington Irving	[sleep10x.xxx]	41
	[The Plain Vanilla ASCII Etext has been withdrawn at the request of NUSIRG]		
Sep 1992	NorthWestNet NUSIRG Internet Guide	[nusirgxx.xxx]	40C
Sep 1992	Hitchhiker's Guide to the Internet, Ed Krol	[hhgi10xx.xxx]	39
Aug 1992	The Hackers' Dictionary of Computer Jargon	[jargn10x.xxx]	38
Aug 1992	The 1990 US Census [2nd], US Census Bureau	[uscen902.xxx]	37
Jul 1992	The War of the Worlds, H.G. Wells [Herbert George]	[warw10xx.xxx]	36
Jul 1992	The Time Machine, H.G. Wells [Herbert George]	[timem10x.xxx]	35
Jun 1992	Zen & the Art of Internet], Brendan P. Kehoe [Zen has NOT been withdrawn from circulation at the request of the author]	[zen10xxx.xxx]	34
Jun 1992	The Scarlet Letter, Nathaniel Hawthorne	[scrlet10x.xxx]	33
May 1992	Herland [for Mother's Day], Charlotte P. Gilman	[hrlnd10x.xxx]	32
May 1992	Sophocles' Oedipus Trilogy [Three Greek Plays]	[oedip10x.xxx]	31
Apr 1992	New Etext of Bible [KJV] [From many editions]	[bible10x.xxx]	30
Apr 1992	Data From the 1990 Census, US Census Bureau	[uscen901.xxx]	29
Mar 1992	Aesop's Fables [Advantage] [Our Second Version]	[aesopa10.xxx]	28
Mar 1992	Far From the Madding Crowd, Thomas Hardy [Hardy1]	[crowd13x.xxx]	27
Feb 1992	Paradise Lost [Raben] [originally in all CAPS]	[plravn11.xxx]	26
Feb 1992	1991 CIA World Factbook, US CIA, 1991 Edition	[world91a.xxx]	25
Jan 1992	O Pioneers! Willa Cather [Cather #1]	[opion10x.xxx]	24

Jan 1992	Frederick Douglass, Narrative of the Life of	[duglas10.xxx]	23
Dec 1991	Roget's Thesaurus	[roget12x.xxx]	22
Dec 1991	Roget's Thesaurus	[roget11x.xxx]	22
Nov 1991	Aesop's Fables	[aesop11x.xxx]	21
Oct 1991	Paradise Lost, John Milton	[plboss11.xxx]	20
Sep 1991	The Song of Hiawatha	[hisong11.xxx]	19
Aug 1991	The Federalist Papers	[feder12x.xxx]	18
Jul 1991	The Book of Mormon	[mormon13.xxx]	17
Jun 1991	Peter Pan [for US only]**, James M. Barrie	[peter14a.xxx]	16
May 1991	Moby Dick [From OBI]*, Herman Melville	[mobyxxxx.xxx]	15
Apr 1991	1990 CIA World Factbook, The US CIA	[world12x.xxx]	14
Mar 1991	The Hunting of the Snark, Lewis Carroll	[snark12x.xxx]	13
Feb 1991	Through the Looking Glass, Lewis Carroll	[lglass16.xxx]	12
Jan 1991	Alice in Wonderland, Lewis Carroll	[alice29x.xxx]	11

[These two Roget's are not exactly the same]  
\*Moby Dick is missing Chapter 72

\*\*Please do not download Peter Pan outside the US

Aug 1989	The Bible, Both Testaments, King James Version	[kjev10xxx.xxx]	10
Dec 1984	The Bible, The New Testament, King James Version	[biblexxx.xxx]	xx

The Bible and Shakespeare represented the entire effort for the 1980's and the Bible alone is about 1,000 times larger than our first file, the U.S. Declaration of Independence. [So is Shakespeare.]  
[The Shakespeare Was Never Released]

Dec 1979	Abraham Lincoln's First Inaugural Address	[linc1xxx.xxx]	9
Dec 1978	Abraham Lincoln's Second Inaugural Address	[linc2xxx.xxx]	8
Dec 1977	The Mayflower Compact	[mayflxxx.xxx]	7
Dec 1976	Give Me Liberty Or Give Me Death, Patrick Henry	[liberxxx.xxx]	6
Dec 1975	The United States' Constitution	[constxxx.xxx]	5
Nov 1973	Gettysburg Address, Abraham Lincoln	[gettyxxx.xxx]	4
Nov 1973	John F. Kennedy's Inaugural Address	[jfkxxxxxx.xxx]	3
Dec 1972	The United States' Bill of Rights	[billxxxxx.xxx]	2
Dec 1971	Declaration of Independence	[whenxxxxx.xxx]	1

Date: Mon Mar 21, 1994 8:57 pm PST  
Subject: Comments from Phil Runkel

[From Dag Forssell (940321 1845)]

With today's mail arrived the following from Phil Runkel. I know many netters know Phil and appreciate his thoroughness. I certainly do. I have deleted detailed comments, but leave those comments I think will be of interest to netters in light of our discussions about feelings.

-----  
5070 Fox Hollow Road  
Eugene OR 97405-40  
17 March 1994

Dag Forssell

23903 Via Flamenco  
Valencia CA 91355--2808

Dear Dag:

Because my mind is busy with several things, I sat down to type a note telling you I would not be reading your draft of "PC: Useful Management Insight." But just out of curiosity I flipped through the pages, my eye got caught by this and that, and now I find that I have read the paper. By the way, that's a nice encomium that Matt Gibbons wrote for you.

This paper, like the others, has good stuff in it. You do pack key ideas into a small space. Your inventive diagrams I find very helpful. In fact, I'll probably borrow some of your ideas for diagrams--giving proper credit. I think each topic in this paper is a topic that some manager--not all--would like to know about. I am not entirely at ease, however, with the mix of topics. The paper does not seem to have a center--a single focus. Whether that is a serious fault for your purpose, I cannot say.

Now I'll turn to specific points.

Page 2, ... End of that paragraph. Not what people experience inside themselves. The old S-R theory says that it is the event in the environment, happening at the right time, that causes the action, regardless of what the perception and action feel like, or how they are understood, inside the organism. At least, that is what the older theory said. I have heard that most S-R theorists ("behaviorists") have patched the theory to soften that stark claim. If you want to look into the changes, the issue of the American Psychologist for November 1992, vol. 47, no. 11 is wholly devoted to the current state of Skinnerian psychology. One article in it is entitled, "Case histories in the great power of steady misrepresentation."

....

I like your ideas for diagrams. I think you have made very helpful additions and alterations to earlier diagrams. And your own original diagrams are fine, too. The way the diagrams in this paper are tied together, each helping the others, is very nice. In Exh. 5, however, I would not use the label kinds of perception. There is nothing in the perception (that is, the internal process) that distinguishes "relationships" from "family" or "school." You are distinguishing physical situations, or relations with the external world, or what is being represented. I find it hard to choose a simple word to substitute for "kind," but calling those perceptions "kinds" seems to me like calling trucks that haul canned beans, that haul books, and that haul candy different "kinds" of truck.

{I am changing to "subject of perception." -- Dag}

...

I am very glad to learn about the recent writing of Plooiij and van de Rijt-Plooiij. Except for their article in Marken 1990, I have no note of their writings since 1986. I'll go to the library for the two articles you cite.

.....

Page 12, first column, fourth line under the heading. . . . as a leader--or as anything else. [I'm not proposing that you add those words; I'm just making a comment to you.]

Second line of paragraph 1. Whoops! "...is determined by their environment." You said at the outset that that was one of the psychological conceptions that was not true--or at least insufficient. Next line: They do too control means. I control my perception of the path to the library because doing so is the means to reach my goal of getting a certain book off the shelf, which is in turn the means to getting a certain piece of information I have in mind. But it is true that we usually care very little about the means as long as the means is bringing us nearer to the goal set at a higher level of the neural hierarchy.

{The statement Phil objects to is: "Leaders and followers alike act only to produce and maintain intended perceptions. How people act in order to do this is determined by their environment. People control \*results\* (their perceptions), not the \*means\* used to produce those results (their actions)." I think I will ignore Phil's comment. -- Dag}

Three lines down. I do not agree that the leader's first task is to ensure that everyone perceives the common goal. First, it is impossible to do so. You can invite people to adopt the goal you describe to them. And if a person's understanding of what you describe sounds to him as if doing that will not interfere much with his pursuit of his own goals, then he will, at least for a while, help you toward that goal--though only to the extent that his picture of what you want matches yours, and that match is never perfect. Second, to have an organization that functions well enough to be useful to almost everyone in it, it is not necessary for all to strive for a common goal. It is necessary only for people to stay out of one another's way so that they can pursue their own goals. And most of us have goals that can be pursued more easily (in our modern world) as members of organizations than alone. So we often don't care whether we reach our goals by helping the boss--OK, why not? It's as good a path as another. So we do it, and the boss is happy. That doesn't sound very inspiring, and most managers probably don't find that view very flattering, but that is all that is necessary for an organization to function well enough. And, as a matter of fact, if it functions that well, it will not be long before people begin to discover the joys of cooperation, and at least some parts of the organization actually become joyous places to be. And I'd make similar comments about paragraph 2.

{Again, Phil engages in selective reading, I think. The statement he objects to is: "Because associates control what they perceive, the first task of a leader is to ensure that everyone is able to perceive the common goal in terms of all the perceptual variables that make it up-- what the multiple dimensions of the goal are." Note that I did not say to compel everyone to adopt the goal, just to ensure ability to perceive the dimensions of it.}

The remarks I have just made, as well as being comments on PCT, are also remarks about how to do organizational consulting. And you will find further remarks below that explain how I would do things differently from you. My sentences here sound as if I am very confident about what I would do, though actually I cannot be sure without being in the actual situation and seeing there what I actually do. I am actually describing here only my best guess about what I would do, given your few words describing the situations. In giving you these opinions, I am not claiming that I am a better consultant than you. I am not saying that you should be doing things the way I would do them-or think I would do them. I am saying only, "There is what you would do; here is what I would do." And if my words give you no food for thought, well, that is the risk I take.

Page 12, second column. You have said several times that you can ask questions to find out about internal standards (reference signals). You have advised asking questions. That's fine. But getting reliable estimates of internal standards usually requires some time and skill. It often requires the kind of investigation I described on pages 151-152 of Cast Nets & Test Spec. But maybe there is not room here for that kind of discussion.

Same column, your list of what to do near the bottom. I would also call the other person in, or maybe do this whole thing with all the members of the work-group present. And in regard to the last paragraph, I would let the two do their own negotiating, though I would sit with them as a third-party helper if they wanted me to do so.

Page 13, at the third bullet. I always ask about feelings. Feelings indicate where the caring lies, where the internal standards are that have higher priority. Furthermore, the person will have better knowledge of himself and his capabilities if he is aware of the possibilities for action that arouse his feelings. And other people will be better able to avoid obstructing the person if they know what arouses the person's feelings. Of course, a certain discipline is useful in talking about feelings. It is no help merely to cry out in agony.

Fourth bullet. Absolutely. Couldn't agree more. This same argument holds, in my mind, for not pressing people to follow the boss's goals. That's taking over his responsibility for choosing his goals. But it is all right, even good, to offer the boss's goals--as long as you don't punish the person if he declines to take up those goals. Fifth bullet: I agree. It is no help to ask people to defend past errors. (I wish politicians did not feel they have to do so.)

Paragraph under the bullets. People often don't know what their priorities are in the sense of what kind of purpose they will first pursue in the next opportunity for action. (Often, for example, an opportunity to pursue one goal shows up before an opportunity to pursue a goal of higher priority, and it looks to bystanders and even the person himself as if what he said earlier about priorities is wrong, when it isn't.) It is often helpful for the person just to be aware of conflicts among his goals. Next paragraph. Here, in contrast to your earlier remarks about common goals, you urge the reader to try not to set goals "for people."

....

I expect you will find some of this a nuisance. I hope you also find some of it helpful. Regards to Christine.

Sincerely,

Philip J. Runkel  
-----

Best, Dag

Date: Tue Mar 22, 1994 7:03 am PST  
Subject: Re: BIOME server

><sheepish grin>

>Sorry, I was not trying to report a problem since it had been too long  
>since I last tried.

Understood. Sorry if was overly snarky.

>I know that I had trouble with the name server resolving  
>"biome.bio.ns.ca" but that should not be your problem. Currently (ie: at  
>this very moment, I am trying again to access the server) but am having  
>dismal success. However, again it looks like internet problem and not  
>server problems.... ie: I logged in ok but preformance is miserable and  
>downloads mostly fail (timeout).

The server has two names, biome.bio.ns.ca and biome.bio.dfo.ca, and the IP number is 142.2.20.2 if your DNS can't find it.

Ftp users should be warned that we are on a fairly slow link and there can be delays, especially when large ftp transfers are in process (we have an image processing group on our subnet!).

There are times when the server is down for extended periods. This is not a biome problem! Due to limited disk space the ftp and gopher servers are on another machine (NFS if you know what that is) and when that is down, the biome servers go down. Unless CSG wants to buy me another gigabyte of disk space there is nothing I can do about it!

Bill Silvert [940322]

Date: Tue Mar 22, 1994 7:11 am PST  
Subject: Re: Comments from Phil Runkel

<[Bill Leach 940322.08:01 EST(EDT)] >[Dag Forssell (940321 1845)]

Indeed, that was interesting and I see that Phil is yet another outstanding member of the group.

I feel that he supports a position that I take with feelings. I also got the impression that 'feelings' are a result of some change in intrinsic error conditions and therefore overwhelm rational considerations (from reading BCP). Thus, it is probably not wise to ever totally ignore them.

I can only add that I believe that most people also respond better to people that 'care' about how they feel. So while I agree with Phil, I emphatically agree with both of you that such dealing will not be useful if additional steps to go beyond feelings are not taken.

I know that my overall attitude about dealing with feelings is (and no doubt will continue) to change as I learn more.

-----  
>Second line of paragraph 1. Whoops! ...

This is really interesting to me. I think that this is an example of a very difficult area to deal with. For example (being a little loose with terminology here): "We all KNOW that changes in our environment can change the way that we behave."



This is so "obvious" that almost the entire edifice of traditional psychology is based upon it. Common experience also "tells" one that it is true.

Of course common experience lets the fact that the statement is missing THE point "leak" in once in awhile too.

I think that we will always be facing the problem with relating the importance of the environment and its' actual relevance (or lack of) to behaviour when discussing PCT in "real world" situations.

It is generally valid to say: Changing the environment that a subject is operating in will change the subject's behaviour. [Of course for this to be true, the change must effect a controlled perception of the subject.]

Thus, I think that there will always be some disagreement about just how to phrase parts of a presentation. You both know that changes in (or failure to change) the environment CAN result in behavioural changes and that in a sense these environmental changes can be thought of as "triggers" for the behaviour changes. No one questions the idea that one suddenly finding that the building that one is in "is on fire" will "trigger" a behavioural change.

The question then is "does the use of such a common way of expressing something detract from the reader's understanding of "Control of Perception?" My take is that the answer is sometimes yes and sometimes no. The work would become too cumbersome if you "pause" at every line to again state that such 'triggers' do not cause the behaviour but rather the resulting perception etc....

-----

>Three lines down. I do not...

I tend to agree even more with you Dag on this one than with Phil.

We as people, tend to think that "others" have the same desires and goals that we do. In a certain general way this is undoubtedly true but as any negotiator knows that is rarely the situation in the specific case. If nothing else, the priorities are usually different and often the essential goals are also different.

However, most people recognize that others, that they interact with, have goals (especially if they recognize that they themselves have goals). It is also not usually too difficult to get someone to realize that if members of a group share a common goal that "work" will proceed more smoothly and effectively.

What many people do have a great deal of trouble understanding is how an "organizational goal" (the boss's goal) is or can be their own goal. An effective leader IS one that is able to "cause" a follower to 'internalize' a 'group goal' (or goals). This doesn't mean (at least to me) that the leader gets the subject to accept a goal of 'second quarter profits up by 2%' (unless the subject is also a major stockholder and has a goal of more income).

Rather the leader helps the subject to establish a "structure" of goals from the "organizational goals" to the subject's goals. This structure must be "real" to the subject to be effective. That is, merely telling the subject that "this is how it is" is pretty much useless.

Assuming that just "creating an environment where everyone can achieve their own goals" will result in "organizational success" is, in my opinion, naive at best. The 'literature' and most personal experience abound with examples of almost miraculous "behavioural changes" of individuals when they just perceived the existence of an 'organizational goal' (and undoubtedly related same to some goal(s) of their own).

I perceive that something along the lines of what I just said is what you are expressing in your work and if my perception is correct then I agree with your position and even oppose some of what I understand of what Phil is saying.

In a "real world" work place there are lots of factors that must be considered. While "property rights" are experiencing a most profound assault in society today and many people really believe that the owners of a business should have little to no "rights" in the control of that business, these same people have an altogether different attitude about their own property.

In general, the company does have the right to expect an employee to contribute to the success of the company and should have the right to terminate an employee that fails to make such a contribution. OTOH, many of us recognize that "IT IS IN THE BOSS'S OWN BEST INTEREST" to try to create and support the sort of environment that causes people working to satisfy their own desires to also be satisfying the boss's desires.

It is not always (ever?) possible to establish an environment that will allow every individual to meet company goals by the act of meeting their own. When not, the individual should be "given the chance to seek other opportunities."

I think that the approach that I perceive that you are proposing is a step in the right direction. First, you make a real effort to help bring into alignment (or allow the subject to recognize an existing alignment) personal goals of the boss and the subject. Not only will one likely improve organizational performance by attempting to do this but this is really the only way that you can honestly determine that an employee's goals are not compatible with the "company's" (short of theft, extortion, physical violence and the like).

-bill

Date: Tue Mar 22, 1994 10:55 am PST  
Subject: Re: BCP on the Internet?

>So since when are we limited to the archaic print media? This is 1994.  
>Why not put Behavior: The Control of Perception on the Internet to share  
>with the world?  
>  
>There are already hundreds of books on the Internet as part of Project  
>Gutenberg, mostly older classics. They can be downloaded via FTP, Gopher,  
>Whole World Web, Mosaic, etc. Volunteers scan and/or type there favorites  
>into text files. Why couldn't the same be done for a modern classic? I  
>even believe that there is one very active, dedicated, and fanatical PCTer  
>who has a scanner and might even be crazy enough to want to lend his  
>services to such a project. (Hi, Dag!).  
>  
>...unless of course Bill S. insists on organizing the entire project on

>his BIOME server ;-)

No, I don't want to do any organizing, but I wanted to point out that Internet publication is not just the serving of text documents. WWW (World Wide Web) servers can handle hypertext, graphics, even animation and sound. WWW Clients are freely available for Windows, Mac, and most Unix workstations. Some of my own papers are available from the server here at biome (URL <http://biome.bio.ns.ca/>).

If you write a paper using any of the common word processors you can save it as an RTF file and use an automatic RTF -> HTML converter to generate a suitable WWW file.

I can't put in much work on this, but if someone sends me a relevant RTF file I'll convert it and install it on the biome server. No promises that it won't be a total mess though!

Bill Silvert

Date: Tue Mar 22, 1994 12:17 pm PST  
Subject: Reply to Bill Leach on Systems Theory

[Cliff Joslyn, 940322] >[Bill Leach 940316.09:25] >>[Cliff Joslyn, 940315]

>ST scientists should .. present a united front against their own that  
>abuse the principles of ST and science in general.

I've never seen ST scientists present a unified front about anything.

>We are talking about a particular field of science that is being used  
>as the justification for fundamental alterations of human society.

I continue to dispute that ST or Systems Science as a particular discipline has ANY weight, positive or negative, in public policy formulation. Simulations are used, but the vast majority of scientists doing modeling and simulation are either ignorant or disdainful of ST per se.

>In my mind a "true Systems Theorist" would not  
>consider a model reliable until validated and even then would want to  
>recognize the limits of the model's predictive power.

OK.

>>And exactly who would these people be, now?

>

>I know that several of the names have appeared here in the postings of  
>others but I would have to do a bit of research myself to find them (your  
>name was not in the group :-) ).

Without specifics you have no argument, only rhetoric.

>>Honestly, Bill, your perception of what ST is is so far removed from my  
>>own that I find it difficult to respond. I would be VERY interesting to  
>>hear from you a concise description of exactly what you perceive ST to be.

>

>Actually, I think it would be more interesting to hear what you consider  
>ST to be... more useful too.

But I've already done that, many times over the past few weeks. In particular, I posted an entry specifically on Systems Theory from an upcoming Dictionary of Philosophy written by myself and Francis Heylighen. Read the archives.

Cliff Joslyn

Date: Tue Mar 22, 1994 12:17 pm PST  
Subject: Hierarchical Perception

[From Rick Marken (940322.1130)]

Bill Leach (940321.17:52 EST(EDT) posts comments from Mark:

>The astonishing thing came when I started to display these images in  
>sequence, at high speed,

>All of a sudden, every nook and cranny and oddity of my surface became  
>instantly and obviously visible.

This is an interesting phenomenon. It seems similar to one reported by J. J. Gibson (and easily demonstrated; a great version can be seen at the Exploratorium in SF). A wire frame cube dangles from a string behind a transparent screen. There is a light source that comes from behind the screen so that the lines of the cube are projected onto the screen. When the cube is stationary, what you see is a 2-D pattern of lines on the screen. When the cube is rotated (so that it spins slowly on the string) it "bursts" into fabulous 3 D and you see it's "cubic-ness", which was not apparent when the cube was stationary. When the cube is stopped, the 3-D and the "cubic-ness" go away.

I interpret this as evidence of hierarchical perception. The perception of 3-D here depends on having lower level perceptions of configuration. My guess is that the stationary cube produces some level of output from a configuration detector. When the cube is rotated, there is now a big input from an "object" detector that takes the temporal sequence of configurations over time as input.

Anyway, my guess is that what the animation allows (in Mark's example) is the possibility of perceiving the image using "higher order" perceptual functions.

Best Rick (the Exploratorium freak) Marken

Date: Tue Mar 22, 1994 3:30 pm PST  
Subject: Emotions; goals in organizations

[From Bill Powers (940322.1550) MST] Bill Leach (940322.0801)

>I feel that [Phil] supports a position that I take with  
>feelings. I also got the impression that 'feelings' are a  
>result of some change in intrinsic error conditions and  
>therefore overwhelm rational considerations (from reading BCP).  
>Thus, it is probably not wise to ever totally ignore them.

The chapter on emotions was deleted from BCP by my editor. It's in LCS-2. My guess is that emotions are felt somatic states, resulting mainly from preparation to back up the learned hierarchy's control processes. Strong emotions therefore go with big error signals. The error signals reset lower-levels reference signals for action, in the neural hierarchy, and also reference signals for biochemical and physiological states, via the hypothalamus. The change in physiological/chemical state is the feeling component of an emotion; the goal in the hierarchy is the cognitive component.

Feelings are perceptions, aren't they?

-----

RE: goals in organizations.

Here's an assignment to see if you can really think like a PCTer. Write me an essay on exactly what is involved in one person getting another to adopt a particular goal, and knowing what the other person has actually adopted.

-----

Tom Bourbon (940322.1015) --

Got your post with line-enders, all in good shape.

Best, to all, Bill P.

Date: Tue Mar 22, 1994 3:35 pm PST  
Subject: Re: BIOME server

<[Bill Leach 940322.10:08 EST(EDT)] >Bill Silvert <bill@BIOME.BIO.NS.CA>

>Understood. Sorry if was overly snarky.

Not a problem. I will continue to play with it a bit but am concerned that I may not be able to establish a useable link. I do not ftp directly but rather direct the host to establish the ftp session and then I initiate a zmodem transfer between my host and my machine for the actual download. The host then ftp's the file and transfers it to me via zmodem. Unfortunately the host does this on a block by block basis. Thus, network congestion is a serious timeout problem.

>NFS

Yep, quite aware of what NFS is and the potential results of a remote mounted filesystem being "down".

-bill

Date: Tue Mar 22, 1994 3:40 pm PST  
Subject: Any progress?, Perception and imagery

[From Rick Marken (940322.1500)]

I'm wondering how Martin Taylor is doing at resolving the (non-existent) relationship between IT and PCT. As you may recall, in our last episode Martin said that uncertainty about the disturbance given the perceptual signal,  $U(d|p)$ ,

exists and can be measured in terms of the dependence of the optimally delayed derivative of perceptual signal on the derivative of the disturbance variable. Since Bill's demonstration that there is no such dependence (when there is good control) we haven't heard much from Martin. Any progress, Martin?

Micheal Fehling (940321 1:39 PM PST) to Bill Leach (940320.20:11 EST(EDT))

>there is actually a significant problem with this claim [that "visual"  
>images are recorded and can be "played back" later]. The evidence  
>heavily suggests that visual imagery is not "recorded" (at least not  
>faithfully) and is reconstructed rather than "played back."

Nice to hear from you Michael. It's been a long time.

I think "reconstructed" is a good description of how visual imagery is handled in PCT. When you "imagine" an apple you are (according to the model) sending a reference to a control system requesting a particular perception ("apple"). You produce this perception by setting references for lower level perceptions that contribute to the perception of "apple": a particular perception of color, shape, texture, etc. The reference signals for these lower level perceptual variables are "short circuited" when we imagine; they are played directly back into the perceptual function of the system receiving the reference for perceiving (or, in this case, imagining) "apple". It is in this sense that imaginations are "reconstructed"; actually they are "constructed" (by perceptual functions) in the same way as perceptions. In fact, the only difference between imagined and "real" perceptions (in PCT) is in the ultimate source of the inputs to the perceptual function; when we perceive "apple" the ultimate source of the perception is sensor activity (or the environment, for us shameless realists). When we imagine "apple", the source of the image is the short circuited (or "played back") reference signals for lower level perceptual variables.

The experience of a perceived apple differs from that of an imaged apple in many ways -- but mainly in what might be called "fidelity". The difference in fidelity (in the PCT model) can now be seen to result from two possible sources; 1) unlike the perceived apple, the imagined apple cannot be based on intensity (lowest) level perceptions since these perceptions can ONLY be caused by activity in the sense receptors 2) the degree of "apple" signal that comes out of the "apple" perceptual function depends on how many of the lower level perceptual variables are present at the input. In perception, all these lower level variables are present at the input to the "apple" perceptual function -- and they are at the right level -- because there is actually an apple out there causing them. But when we imagine an apple (especially if we are not skilled at doing this -- some people apparently cannot do it at all) we might fail to "re-play" all of the "right" perceptual inputs because we have not learned (memorized) all the right references to set in order to get a good, clear apple image.

>There is one exception--so called eidetic imagers who seem to indeed to be  
>able to faithfully store and remember exact visual patterns.

I think an eidetiker is a person skilled at producing all the lower level references required to "re-perceive" an intended perception (as an imagination). It MAY be that some of the INCREDIBLE eidetikers (like Stromeyer's lady who fused Julesz patterns when given one side a DAY after the other) can, somehow, set references for very low level perceptions -- lower than what the ordinary person can set.

Best Rick

Date: Tue Mar 22, 1994 7:31 pm PST  
Subject: Re: Reply to Bill Leach on Systems Theory

<[Bill Leach 940322.18:35 EST(EDT)] >[Cliff Joslyn, 940322]

>I've never seen ST scientists present a unified front about anything.

<chuckle> I suppose I have to cry uncle on that one.

>I continue to dispute that ST or Systems Science as a particular  
>discipline has ANY weight, positive or negative, in public policy formulation.

Ok, then you tell me... Where in the HELL are these insane "models" coming from that continually predict the end of civilization about once a week on the evening news?

>Without specifics you have no argument, only rhetoric.

OK, fair enough...

How about Jay Forrester, Dennis and Donna Meadows? (all from MIT, from an easy source).

Cliff, I doubt very much that I would really want to argue with you about the value of ST as I suspect that you practice the field. Since you seem quite at home here then I must assume that you hold the principles of modeling real systems as put forth most elegantly by Bill Powers as supreme.

Research into other aspects of modeling do not bother me. It is only when someone CLAIMS to have valid predictions about a physical process using a model that is CLAIMED to be a physical process model but that by demonstration has PROVEN that it is NOT such a model, that makes my blood boil!

-bill

Date: Tue Mar 22, 1994 10:20 pm PST  
Subject: Re: Hierarchical Perception

<[Bill Leach 940322.18:48 EST(EDT)] >[Rick Marken (940322.1130)]

>This is an interesting phenomenon. It seems similar to one reported  
>by J. J. Gibson (and easily demonstrated; a great version can be seen

Actually Mark and I talked quite a bit about that particular phenomenon but concluded that it differs in some very important aspects. The 3-D wireframe model is one that has astonished me a couple of times (I have several rather powerful 3-D modeling programs here) but I immediately recognized that one of the very strong visual cues is that when the object is rotated, certain wires rotate in one direction and others rotate in an opposite direction. This is significantly different in that there actually is additional information present in the rotating image than is present in the stationary image.

Last month, I might not have used the term "higher order" perceptual functions but now, yes, I agree. Mark and I had talked about this one some years before I had ever heard of PCT and even then we concluded that human image processing seemed to have different functional processing capability.

Of course a beauty of PCT is that it seems to provide a physical explanation for this visual behaviour that both makes some sense and seems to logically follow from a consistent design!

BTW, I have a commitment from Mark to read BCP when I have finished the appendix.

-bill

Date: Tue Mar 22, 1994 10:41 pm PST  
Subject: Re: Emotions; goals in organizations

<[Bill Leach 940322.20:29 EST(EDT)] >[Bill Powers (940322.1550) MST]

I am into LCS-I right now but it will be "slow going" for a couple of weeks at least.

Question:

I don't think that this is directly something that PCT has been able to deal with yet but wonder...

Many (most?) of us have had an experience where some sort of "emergency" occurred where "time seemed to slow down." I know that for myself, this has happened several time though fortunately none very recently.

What I remember is that a great amount of detail about the event is retained for some time following the event. During the event, motion actually appears to have slowed (ie: the internal "clocking rate" appears to have increased). Almost all senses appeared to be "sharpened" such that you remember even things that would normally be "silly" to remember. And finally, you feel physically "drained" for a significant period of time following the event.

It seems to me that this "shock" must be at least somewhat "electrical" in nature. I do not know the chemical transport rates for the brain but it seems to me that they probably could not be great enough for adrenlin (or any other chemical activator) to actually travel through the brain quickly enough to initiate this reaction.

The question then is; Has this been studied and basically what are the conclusions?

>Essay

I'm gonna sorta "bag" this one for now... again time. However, I will take a small "shot" at it now:

The assumptions that I am making are:



1. The subject is known as a casual friend or work associate. 2. This is a normal "real world" situation:
  - A. Usual relationship apply (not a controlled experiment)
  - B. No investigative power exists, only normal observation under normal conditions.
3. Fully voluntary conditions (ie: not a boss or other legally or socially compelling person)
4. The goal is legal, ethical and would not violate common moral codes.

If one wishes to get someone to adopt a goal, I think that it would first be necessary to attempt to apply THE TEST.

Seriously try to determine what perceptions the subject is presently controlling for now while recognizing the limitations in employing just observation. Be particularly alert to behaviour during upsets to routines for clues to underlying goals. Also be attentive to "areas of excellence" and behaviour where the subject consistantly appears quite happy or content for clues. Conversely, the obvious presence of indications of stress in the subject may provide clues to goals that the subject is not able to control.

Armed with this potential background information, interview the subject. Ask about goals and dreams but recognize that depending upon the relationship to the subject, the subject may well "modify" the expressed goals, dreams, likes and dislikes as a means of influencing the perception of the interviewer.

By this, I am talking about trying to determine what goals might exist that could be related to the GOAL. This involves first attempting to "get a hint" as to what the subject might be controlling for and verifying though a combination of checking consistancy with other verbal expression and actual behaviour.

Naturally it must be kept in mind that inconsistancies observed could be caused by the observer's perceptions not matching the subject's for the particular event rather than just an error in the perception of the subject's references by the observer.

Next step would be to try to relate the GOAL to one or more of the goals that the subject seems to have. If the GOAL can be related to an expressed goal that also has behavioural confirmation in such a way as to show that meeting the GOAL also meets or significantly enhances attainment of the subject's goals success is likely.

It is also quite important that all of the goals involved are specific. It is not likely that trying to "tie" a specific GOAL to a vague goal such as "being happy" will be successful. Tying the GOAL to a specific activity that is likely to make the subject happy OTOH may be useful.

Evaluation:

Gotta go... have some goal of my own that must be addressed.

-bill

Date: Wed Mar 23, 1994 2:05 am PST

Subject: BCP on internet; Prejudice; ftp; memory

[From Bill Powers (940323.0100 MST)]

RE: BCP on the internet.

Gary Cziko seems to have thought this one up. I'm still considering the idea, Gary. Perhaps I would be more encouraged to do it if I heard that you are going to put your new book onto Gutenberg, in the public domain, when it comes out.

I, of course, would love to have free access to all journals and books published world-wide: the intellectual Free Store. Authors who withhold their works from free distribution to others just because they enjoy getting those little royalty checks once a year are money-grubbing tools of capitalist oppression, twisting the goals of enlightenment into the base purpose of maintaining elitist distinctions between the haves and the have-nots.

Aldin/deGruyter will decide by the end of March whether to do another printing of BCP. If they decide not to, all rights will revert to me, and Fred and Perry Good will probably take over publishing it. I still plan to enjoy those little royalty checks. There are always libraries!

It's not a "crime" that Aldine-Degruyter are considering terminating publication. They kept my book in print for 21 years, despite sales of as few as 20 books per year.

-----  
Bill Leach (940316.0925 EST) et a number of alias --

Systems theory is an occupation, not an idea. A system theorist can have good ideas or bad ideas. I am against bad ideas and will speak out against them freely. To condemn an entire occupation, however, is simply a form of prejudice, like condemning all priests or policemen or doctors or nuclear power engineers or rat psychologists or information theorists or behaviorists. Or control theorists.

When I get fed up with certain people in some occupations, particularly just on the basis of what they write, I remind myself that I might actually meet them some day, face to face, and might actually find that I like them. It would then be very embarrassing to recall my own intemperate references to their habits, motives, or personalities that I may have publicly delivered.

Just remember: we human beings reserve our most extreme hatreds for those whom we have hurt the most.

-----  
Bill Leach (940322.1008 EST) --

>The host then ftp's the file and transfers it to me via  
>zmodem. Unfortunatly the host does this on a block by block  
>basis. Thus, network congestion is a serious timeout problem.

I do it the same general way, except that I ftp into a file in my allotment of host disk space. Then I quit ftp and download the file to my PC. Never had a problem.

The myth of multitasking is that you get more work out of the CPU. That's true only if you don't try to get more work out of the CPU. When you are sharing a 50 MIPS machine with 200 users, you have a 0.25 MIPS machine for your own use, and you're probably completely disk-bound. I have had some amusing conversations with

the manager of a big powerful IBM system, who sneered at desktop computing while he showed me that he had the response-time of his system to user commands down to 3 seconds, average. I told him it would drive me nuts to have to use one of those slow clunky mainframes.

At least when you ftp directly from mainframe to mainframe, you are not introducing extra overhead by trying to get the same machine to construct blocks and then try to squeeze them through the line to you while ftp sits there drumming its fingers. You have to be kind to these big old locomotives; they run out of steam very easily.

-----  
From Rick Marken (940322.1500) --

Nice analysis of "reconstructing memories." I would increase the emphasis on the level at which remembering takes place. You can remember a room as a rectangular parallelepiped, or as a room in a house you used to live in with all its objects, windows, doors, rug, etc.. People vary enormously over individuals and over subject matter in their ability to remember details of form and action, and even ordering of events. I think this depends largely on the level at which they begin the reconstruction. There's no reason to think that memories are seriously distorted; many of the mistakes, I would guess, are due to not starting at a low enough level. Another bunch of mistakes, of course, arises from stitching together low-level memories that did not actually occur at the same time -- that's called imagining.

Another source of mistakes is using high-level functions to reconstruct high-level memories from low-level memories that were acquired before the high-level functions even existed. You can put an adult interpretation on childhood memories, remembering attitudes that you couldn't possibly have had then, but are perfectly capable of having now. I sometimes think that a lot of childhood trauma that are remembered are things that would seriously traumatize an adult, but which the child simply accepts as the way things are. Sort of like going home after a party and waking up at three in the morning, recalling a remark that slid past you at the party, and realizing that you've been insulted. You were perfectly happy at the party, but you wouldn't have been if you had applied a higher-level interpretation to what was said about you. This is especially poignant if you were drunk at the party and are remembering when you are sober.

As Bob Clark has pointed out, and as I described in BCP, remembering is a skill that can be learned. If your indexing system is haphazard, you'll have no more luck retrieving memories on which to build reconstructions than you would have retrieving computer files. On the other hand, excellence at handling memories is not necessarily an advantage; I have heard of and known people with astonishingly accurate memory who couldn't get away from the past sufficiently to handle the present-time world. I think we become good at remembering when doing so serves present-time purposes. Me? I have found it advantageous to remember only the times when I was right.

Best to all, Bill P.

Date: Wed Mar 23, 1994 2:21 am PST  
Subject: Re: transmitting goals

[From Bill Powers (940323.0300 MST)] Bill Leach 940322.2029 EST

Good start on the essay, but you're mainly in a practical-advice mode. Try to describe what it is to have a goal, to describe a goal, and to know what a goal is. In you mention of the TEST, you're on the right track.

To narrow this down, as a hint, how would you use PCT to explain how you can describe a goal to another person, and what happens inside that other person in the process of understanding what you mean? Remember that all your communications have to come out of your mouth as pressure waves at the lowest level of organization, and they enter the ears of the other person as pressure waves.

Best, Bill P.

Date: Wed Mar 23, 1994 8:53 am PST  
Subject: FADING IMAGES - RKC

<Bob Clark (940323.1040 EST)> Bill Leach (940320.20:11 EST)

>... the phenomenon of "continued motion" after the object has  
>disappeared could be due to the processing of the existence of  
>multiple fading images and that these multiple fading images provide  
>an "indication" of motion to the subject.

Yes, this would be a consistent description of the phenomenon. However, "fading" appears to be more of a matter of retinal chemistry than of higher level processes.

How does one describe "an "indication" of motion?"

To me, memory -- that is "recording with play-back" -- is a better description. A recording is not necessarily either complete or accurate. In addition, there are other related systems in which recording may occur.

There is also the general question of accessibility of recordings.

The concept of "recording with play-back" is useful because it can be applied to many situations.

This concept of memory has been included in PCT from early days.

Regards, Bob Clark

Date: Wed Mar 23, 1994 9:02 am PST  
Subject: VISUAL EXPERINCE - RKC

<Bob Clark (940323.1045 EST)> Bill Leach (940321.17:52 EST)

This "Visual Imagining Experience" from "Mark" (who? I don't recognize the name) is fascinating.

How is this related to the levels of the hierarchy? Is this a "purely visual" experience? It seems likely to me that several levels are likely to be involved.

Was this a binocular phenomenon, or could a single eye have the experience?

How would this be described in PCT terms?

Can the perceptual variable(s) be identified?

Regards, Bob Clark

Date: Wed Mar 23, 1994 9:03 am PST  
Subject: RECONSTRUCTION - RKC

<Bob Clark (940323.1050 EST)> Michael Fehling (940321 1:39 PM PST)

>The evidence heavily suggests that visual imagery is not "recorded"  
>(at least not faithfully) and is reconstructed rather than "played back."

Could it be that the recordings are incomplete and/or inaccurate?

Could recordings also be formed at levels above the retina and its direct projections?

Can "reconstruction" occur without availability of items to compose the new assembly? And, if these items should be incomplete or distorted, how accurate would be the result of "reconstruction?"

What levels of the hierarchy could be involved in such "reconstruction?" And how would the process be controlled?

What are the variables that are perceived?

Regards, Bob Clark

Date: Wed Mar 23, 1994 9:49 am PST  
Subject: BCP on internet

[from Gary Cziko 940323.1614 GMT] Bill Powers (940323.0100 MST) says:

>Gary Cziko seems to have thought this one up. I'm still considering  
>the idea, Gary. Perhaps I would be more encouraged to do it if I  
>heard that you are going to put your new book onto Gutenberg, in the  
>public domain, when it comes out.

I don't think my publisher (still working on that one) would approve of such a venture for a new book that promises to make lots of money for them and for me (let's see, if we can sell 500,000 copies with a royalty of \$2 each . . .). But if sales petered down to 20 copies or so a year after 20 years I think I would consider it. The problem is most science books may not be worth reading after 20 years. Somehow I think BCP is different.

Perhaps one way to go would be to put SOME of it on the Internet--perhaps just the first and last chapters, including information on how to order the book. But the main thing as I see it is to keep BCP available, either in print or in bytes.--Gary

Date: Wed Mar 23, 1994 2:39 pm PST  
Subject: Re: BCP on internet

>I don't think my publisher (still working on that one) would approve of  
>such a venture for a new book that promises to make lots of money for them  
>and for me ...

Actually several books are now published both commercially and on the Internet. The first was Zen and the Art... where when the second edition came out the first was free. Others, like Tracy LaQue's Internet Companion are available in both.

The rationale seems to be that the publicity is worth it, and the readers who are really interested will buy a real book.

Bill Silvert [940323]

Date: Wed Mar 23, 1994 6:49 pm PST  
Subject: Re: BCP on internet; Prejudice; ftp; memory

<[Bill Leach 940323.07:41 EST(EDT)] >[Bill Powers (940323.0100 MST)]

>ST

You are right of course and the situation is even worse since I have a living example of a ST practitioner right here that appears to embody the genuine interests of good science.

>ftp

Not an option here. I used to have a great HP host that I could do that with but now I am limited to BIX and do not have any way to use allocation space for an ftp operation.

>The myth of multitasking is that you get more work out of the CPU.  
>That's true only if you don't try to get more work out of the CPU.

I'm also sure that you are aware of the myths in the above statements. If two or more users are using a single CPU at full rate when available then yes it is true that they would likely obtain better performance using two machines each with half the capacity. In reality, even that is not quite true.

Multitasking as opposed to MultiUser is a significant time saver for the single user. One of the major reasons that I actually do HATE DOS boxes is that they are single-tasking machines and all attempts at multitasking are a horrible kludge (ie: TSRs and Windows). I almost can't imagine life without a multitasking computer available and without fail, I become quite upset whenever I have to do anything "real" on a DOS box.

Network machines such as the old Apolo's (and probably the HP's now) provide even greater performance than even exclusive use of the machine would provide (probably true of at least some of the VAX clusters too).

Mainframes do not impress me favorably and never really have. They are well suited for massive batch operation (payroll) or single programs requiring significant computing power (RELAP5, Criticality calculations and the like).

The systems managers for all of the systems that I worked with in the past considered a half of second to be the worst case response time that could be accepted. I tend to think that such a delay would still be acceptable if a type-ahead buffer exists (usually does) and some sort of visual cue arrives within a tenth of a second normally.

>Memory recall

This is all rather interesting to me. I have felt for sometime now that my own recall visualizations are rather weak. While I sometimes feel or even say that my memory is not very good, I don't really believe that is true. I can often remember, say a hardware configuration in great detail BUT not visually (normally). I don't get a "picture" in my mind of most things that I remember.

I can sometimes draw things "from memory" in great detail but again the "image" does not appear even though the resulting drawing may be quite exact.

OTOH, I do remember experiencing almost "total recall" a couple of times in my life. A situation where "everything" was remembered with realism, a realism that was greater than a high fidelity audio/video recording.

The most vivid of these (and never even approximated in extent or level of detail) occurred when I was taking an Exam. Throughout that exam, everytime I read a question, I would see and hear the class discussions relevent to that question. Besides "aceing" the exam, I was a bit "shook" when it was over.

-bill

Date: Wed Mar 23, 1994 6:50 pm PST  
Subject: Re: transmitting goals

<[Bill Leach 940323.08:14 EST(EDT)] >[Bill Powers (940323.0300 MST)]

OK, I have printed this latest message on the essay and will stick it on the wall in the office. It is clear to me that I must defer to some tasks that require a great deal of attention from me for awhile.

-bill

Date: Wed Mar 23, 1994 8:41 pm PST  
Subject: Re: FADING IMAGES - RKC

<[Bill Leach 940323.20:38 EST(EDT)] ><Bob Clark (940323.1040 EST)>

>However, "fading" appears to be more of a matter of retinal chemistry

Yes, I presumed so however, it still results in a perception at higher levels since the signal IS actually there and "fading."

>To me, memory -- that is "recording with play-back" -- is a better

Yes, in particular, I like the description given by Bill a few messages back. It seems to fit the sorts of variations in experience that people actually describe.

-bill

Date: Wed Mar 23, 1994 9:09 pm PST

Subject: Re: VISUAL EXPERINCE - RKC

<[Bill Leach 940323.20:42 EST(EDT)] ><Bob Clark (940323.1045 EST)>

Mark is an electrical engineer friend that I have known for a number of years.

>How is this related to the levels of the hierarchy? Is this a ...

>Was this a binocular phenomenon, or could a single eye have the experience?

It seems likely (strictly conjecture here) that the phenomenon is strictly one based upon hierarchy. That is, the total amount of "information" presented in the two cases is exactly the same and only the method of visual presentation varies (in fact without rotation the projection actually has an information loss).

It would seem that looking at a flat intensity modulated evokes the "edge detection" visual capability and possibly the object recognition capability though the latter is not likely dealing with anything in the way of objects for which it is "tuned."

Turning the display into a "histogram" appeared to increase the subject's ability to discern conditions of interest slightly. This may well be because we are better able to distinguish spacial relationships in 3 dimensions (flat CRT, single display, no special glasses) than spacial relationships in what appear to be 2 dimensions.

The dramatic effects were when the histogram was rotated. During rotations was when the defects of interest were shockingly obvious. They literally "stood" out.

Again, the conjecture there is that the level of visual processing that deals with moving objects has "difference detection" that is very poor in lesser levels of processing.

Once seen in the rotating image, the defects could then be easily picked out on either of the other two displays but indeed some had not been found prior to seeing them in the rotating image display.

One of Mark's personal conjectures is that we really do not have a 2D processing system but rather use the 3D system. I am not sure what significance that might have but it seems both reasonable and likely relevant.

>How would this be described in PCT terms?

By someone more knowledgeable than I?

>Can the perceptual variable(s) be identified?



I suspect that they probably can be identified but again, probably not by me. I think that one would really have to do some more basic experiments than were done here. There is a great deal of information present all of the images. Too much, I think, to be able to do more than just conjecture about what was going on.

-bill

Date: Wed Mar 23, 1994 9:47 pm PST  
Subject: Re: BCP on internet

<[Bill Leach 940323.21:06 EST(EDT)] >[Gary Cziko 940323.1614 GMT]

>...20 years I think I would consider it. The problem is most science books  
>may not be worth reading after 20 years. Somehow I think BCP is different.

Actually most of the ones that were worth the reading the first time are still worth reading many decades later.

-bill

Date: Thu Mar 24, 1994 7:56 am PST  
Subject: Re: representational momentum

From Tom Bourbon [940323.0902]

Another try at sending this. The conversation on this thread is over, but here was what I tried to say six days ago.

From Tom Bourbon [940317.1141] Richard Thurman (940317.0920)]

>I was really hoping one of the old hands at this would recognize it  
>immediately and explain it all to every ones satisfaction, along with  
>some rather simple experiments that would 'prove' it to the world. Alas,  
>instead I may have to actually do some modeling and run some subjects  
>myself. I'm sufficiently curious to give it a try, but its out of my  
>field of research and I feel a little like a fish out of water. Perhaps  
>thats the nature of PCT.... forcing one to look across disciplines so  
>that we can get a fuller -- richer-- picture of how we (living things)  
>operate.

Richard, \*before the modeling\*, THE TEST. Determine whether the perceptual phenomenon is controlled, or uncontrolled. If you don't do that first, what will you model? Should you try to model RM as a controlled perception, or as a "side effect" of control?

>Bill Powers (940315.2130 MST)

>

>>This is most likely a phenomenon of perception, to be  
>>explained by the dynamical properties of perceptual functions.

>..

>>the

>>neural signal would take time to build up to an asymptotic value,

>>and after the cessation would take time to decline to zero again  
>>-- leading to a continued impression of motion for a short time  
>>after the actual motion had stopped.

>

>Ahhh!!! This just provided one of those moments of insight. I had  
>been looking at this as a top-down kind of thing -- Higher levels of  
>the hierarchy doing their reference setting thing. I see now that you  
>(and Tom, and Rick) are looking at this from the bottom up.  
>Perceptions percolating up from intensities to and through transitions.

I'm not sure we are looking at it from the bottom up. I think we are urging you to get a handle on the kind of phenomenon you are working with. I'd characterize your approach, not as "top down," but as "rush to model before I test for a controlled variable." On that reading, our "position" would be "do no modeling before its time," where 'its time' means \*after\* I find a likely controlled variable. :-)

When you draw the top-down vs bottom-up distinction, you are probably thinking of the reference signal working in a top-down fashion, with perceptual signals "percolating" bottom-up. Am I right? In a PCT model, there is no way to isolate those two parts of the loop, not any more than we can model a control system as \*either\* "inside the system," \*or\* "outside the system." Both sides are present and working, all the time. You express an interest in modeling the RM phenomenon as though it results from rapidly terminating a reference signal; we (BP, RM and I) seem to have converged on the possibility that the phenomenon arises in a perceptual input function. What is needed now is some experimental work to determine the "real" nature of the phenomenon; \*then\* the modeling.

>I had also not taken into account the idea of neural firing rates needing  
>to rise and decline. How does one model this with a digital computer?  
>I don't see this being taken into account in any code you or anyone else  
>has distributed (e.g. the Byte articles, the Primer Series).

We haven't done that yet.

>>This is all worth experimental investigation -- by someone who  
>>isn't up to the ears in other experimental investigations.

>

>Understood. Actually I'm amazed that you, Tom, Rick, Martin, Bill L.  
>and others can get anything done at all with all the network traffic  
>that this list seems to generate. Add that to trying to actually get  
>'real' experimental investigations done and I'm sure it creates quite  
>a strain on time.

Do we have \*you\* fooled! You think we do other things? ;-))

> I appreciate everyone's willingness to support my  
>attempt to understand and apply perceptual control theory.

My thoughts, exactly. Thanks to you for helping me in my attempt.

Here is another line of work that might bear on your questions. There is some really nice psychophysical work, some of it by Wayne Hershberger and Scott Jordan (PCTers -- Wayne is on the net) showing that visual perceptions of a flashing light begin to move \*before\* a person begins to move her or his eyes. (W.A. Hershberger & J.S. Jordan (1992), Visual direction constancy: Perceiving the

visual direction or perisaccadic flashes. In E. Chekaluk & K.R. Llewellyn (Eds.), *The role of eye movements in perceptual processes*. Elsevier. pp. 1-43).

The psychophysical work meshes nicely with physiological work using recordings from single cells in the parietal cortex of rhesus monkeys. There, the locations of the "receptive fields" of many cells change \*before\* the animal makes saccadic movements of its eyes. The pre-movement shift would result in the appearance that a stationary flashing light was moving across the eye, before the eye moved. The timing of the physiological effect is the same (milliseconds of resolution) as the timing of the psychophysical results of Hershberger and Jordan -- and the direction of the pre-movement displacement of the flashes is the same in both cases.

The physiological study is: J-R Duhamel, C.L. Colby & M.E. Goldberg (1992), *The updating of the representation of visual space in parietal cortex by intended eye movements*, *Science*, 255, 90-92.

The effects described in these two lines of work seem to match what we would expect if setting a reference signal for a new position of the eyes leads to a new reference level for location. The pre-eye-movement perception is of the stationary flashing light "moving" in the direction \*opposite\* the direction of the intended saccade. It's as though a new reference is set for where the center of the visual field (the line of gaze) should be, then the flashing light is seen (and physiologically "registered") as moving away from "where it really is" to "where it is supposed to be" -- all before the eyes begin to move. When the eyes move, they create the "trajectory" of the flashing light that occurred in the pre-movement experience. Some pretty neat stuff.

Later, Tom

Date: Thu Mar 24, 1994 8:01 am PST  
Subject: Re: Perception & Imagery

From Tom Bourbon [940323.1530]

Apologies for any typos. I'm reduced to composing and posting from directly inside the mailer routine on our server. About a third of the messages I send this way seem to make it; none I send to csg-1 have made it by any means I have tried during the past week or so.

Rick Marken [940322.1500] replied to Michael Fehling and Bill Leach on the subject of remembered or imagined perceptions. The gist of the reply was that imagined and remembered perceptions are some kind of re-played perceptual signals. Rick spoke of people as replaying the right perceptual signals, and of a few people who seem to have "failed" to do so. Careful! This business of saying that thoughts and imaginings are copies of the original things can get you into trouble. They have a long history of colorful and imaginative names and descriptions: "gentle fires that do not burn," "petites perceptions," "pale copies," "weaker versions," and so on. But the occurrence of these ghosts and phantoms is not universal and the notion that they are necessary element of thought was shot down some time ago by Oswald Kuple and his associates at Wurzburg.

Oswald and the gang were always among my "heroes" in the history of psychology. (Anyone who knows me very well is aware I have hardly \*any\* heroes in traditional

psychology.) I always suspected that, like me, they were non-imagers. For a number of years, I collected descriptions of us non-imagers from the psychological literature: we were "otherwise intelligent people," "unable to perform the necessary pre-cognitive processing," "lacking in the necessary physiological mechanisms," "apparently convinced that we did not experience imagery," "curiously insistent that we did not experience imagery," and so on. When I was a senior psychology major, I first realized that many (otherwise intelligent) people really meant it when they spoke of seeing images. I had always thought it was simply a strange figure of speech. Listening to and reading all of the completely misinformed characterizations of us non-imagers gave me some of my deepest insights into what it might be like to be in a genuine minority, listening to the majority talk about what kind of person you "really are."

For now, just be careful how literally you think of imagery as a necessary condition for perceptual control.

Now, to see if I can escape the dreaded LOCAL QUE!

Later, Tom

Date: Thu Mar 24, 1994 8:07 am PST  
Subject: Lags: second try

From Tom Bourbon [940321.1117]

The following returned mail was waiting in my mailbox when I logged on thos morning, after several hours of unsuccessful attempts. (All is not yet well with my e-mail.) I posted these data back on 2 March, when I thought they might be relevant to the then hot discussion about delayed information in the perceptual signal, or some such thread. Here is a second try, 19 days later.

=====

Date: Wed, 2 Mar 94 11:25:11 CST  
From: "Tom Bourbon" <tbourbon@heart.med.uth.tmc.edu>  
To: csg-1@vmd.cso.uiuc.edu  
Subject: Lags

From Tom Bourbon [940302.1101]

This post summarizes a brief paper I didn't hand out at the most recent meeting of CSG. It is about correlations between handle positions or changes in handle positions, and other variables measured in a that pursuit tracking task. I calculated the correlations after I introduced various temporal lags (delays) between the handle positions and the other variables.

TIME-DELAYED CORRELATIONS BETWEEN VARIABLES IN A TRACKING TASK:  
ARE THERE HIDDEN CAUSAL VARIABLES?

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Something I had planned to present at  
the annual meeting of the Control Systems Group,  
Ft. Lewis College; Durango, CO; 27 July - 1 August 1993,  
but for some reason I didn't.

The question of whether control might be explained by legitimate stimulus-->effect (independent variable-->dependent variable) models arises from time to time on the Control Systems Group computer net. The question came up late in 1992. In January, 1993, I used data saved from a simple pursuit tracking task to run a few calculations in which I tested the possibility that the positions of the handle and cursor at one moment are more highly correlated with some variable, or transformation of a variable, at an earlier time, rather than at the same moment. I was looking for possible candidates for the role of "previously undiscovered causal stimulus." I thought that perhaps change in handle position (dh) at some later time might be a "response" to a change in a "stimulus" at an earlier time. These calculations are all from actual data; there is no modeling.

The results of the calculations are shown in the accompanying Table 1. Correlations with lag = 0 are from the person's original data set during the tracking task and are like those reported many times for tracking tasks. For the undisturbed condition, when I introduced temporal offsets for the handle and cursor, their correlation (r) with the position of the target rose from .995, with no delay, to .998 with delays of 133 to 200 msec, which correspond to the perceptual lags used by Bill in his models. The r between handle and cursor declined, slowly, with increasing delays. Correlations between the position of the handle and the change in cursor-target separation were low for all delays, as were correlations between change in handle position and change in cursor-target separation.

For the disturbed condition, correlations were highest with delays from 133 to 200 msec for cursor versus target, and for handle versus the difference between cursor and target (the latter difference is a measure of the effective disturbance on the controlled relationship of cursor relative to target). Again, the small increase in correlation was maximal during the range of delays corresponding to Bill's modeled perceptual lag. Changes in handle position (dh) correlated poorly with the potential "change stimuli:" change in cursor-target separation, and change in difference between target and disturbance.

In these simulations, there was no evidence for a previously-undiscovered "stimulus," occurring earlier in time and triggering a change in handle position at a later time. I replicated this procedure with several other sets of tracking data. The results were essentially the same as those reported here.

=====

Table 1. Correlations from two runs by a person, whose original data are shown for lag = 0 msec, on a pursuit tracking task with a target that moved slowly, driven by a table of smoothed random numbers. In the first run the cursor was undisturbed; in the second it was randomly disturbed. Each run lasted 60 seconds and each variable was calculated or sampled every 1/30 second; the data record for every variable contains 1800 values. (For pictorial examples of the task and of similar, unlagged, data sets, see W. T. Bourbon, et al. (1990), On the accuracy and reliability of predictions by control-system theory, *Perceptual and Motor Skills*, 71, 1331-1338.) In the correlations, for every non-zero lag, the original

data records were used, with the handle and cursor offset in time by "lag" msec relative to other variables then all correlations were recalculated. (That is, other variables at [time = z] were correlated with handle or cursor at time = z + lag[.] )

No Disturbance: Mean (c - t) = -0.178 pixels; SD = 2.263

Disturbance: Mean (c - t) = 0.042 pixels; SD = 3.928

No disturbance:

Variables	Lag (msec)									
	0	33	66	106	133	167	200	533	1500	2000
c vs t &										
h vs t	.995	.996	.997	.997	.998	.998	.998	.989	.889	.758
h vs c	1.000	1.000	.999	.999	.998	.997	.995	.975	.856	.758
h vs c-t	-.141	-.178	-.190	-.202	-.214	-.227	-.239	-.350	-.518	-.550
dh vs dc-t	-.303	-.480	-.486	-.496	-.508	-.511	-.511	-.397	-.137	-.068

Disturbance:

Variables	Lag (msec)									
	0	33	66	106	133	167	200	533	1500	2000
c vs t	.984	.986	.987	.988	.989	.989	.990	.984	.897	.831
h vs d	-.722	-.707	-.709	-.710	-.712	-.713	-.714	-.720	-.700	-.677
h vs c	.674	.664	.662	.659	.657	.655	.652	.621	.507	.428
h vs t	.660	.659	.658	.658	.658	.658	.657	.646	.564	.492
h vs c-t	.075	.012	-.000	-.012	-.025	-.037	-.049	-.163	-.345	-.382
dh vs dc-t	-.435	-.553	-.557	-.561	-.561	-.563	-.570	-.488	-.178	-.080
h vs d-t	-.993	-.994	-.995	-.996	-.997	-.997	-.997	-.995	-.921	-.853
dh vs dd-t	-.064	-.090	-.084	-.077	-.070	-.063	-.057	.013	.165	.210

KEY:

For actual positions of cursor and target on the screen, and of handle relative to center of its range, and for actual values of disturbance:

h = handle; c = cursor; t = target; d = disturbance.

For differences between magnitudes of variables, at times t and t+1:

dc-t = change in the difference (cursor - target);  
 dd-t = change in the difference (disturbance - target);  
 dh = change in position of handle, (time z + 1) - (time z).

=====

Is there any "information" here that might inform the discussion of information in perception?

Later, Tom

Date: Thu Mar 24, 1994 8:07 am PST  
Subject: Re: Time and consciousness

From Tom Bourbon [940321.1433] Rick Marken (940320.1600)]

>  
>A major aftershock, experienced near the epicenter  
>(I was visiting family in the valley) has waked me  
>from my dogmatic slumbers (or roused me back into  
>my dogmatic wakefulness).

Southern California -- Shocking! And after-shocking!

>Bob Clark (940320.1215 EST)--

>  
>>This strongly implies that time is a controllable variable.

>  
>Not really. The E. coli navigation process works without any  
>explicit representation of time. Also, I don't see how time,  
>per se, can be perceived. Time can be controlled only it can be  
>perceived; how do we perceive time? Is it perceptible?

Time isn't a controlled variable in the PCT model of E. coli navigation, but time  
\*is\* a feature of the model. The memory for E. coli is modeled as a leaky  
integrator, so its time constant and the recent history of the perceptual signal  
determine when the next tumble will occur. Or am I off the mark?

Later, Tom

Date: Thu Mar 24, 1994 8:12 am PST  
Subject: Re: PCT-ish ideas

From Tom Bourbon [940322.1700]

Another try on a post I tried to send 6 days ago. It just returned from the  
abominable local que. Just to let you know I'm watching and trying to say  
something.

From Tom Bourbon [940316.0959] Bill Powers (940314.1800 MST)]

>  
>RE: PCT-ish ideas.  
>  
>The concept of other approaches which seem to have anticipated or  
>at least to have used PCT comes up frequently. It shows that the  
>actual nature of PCT has not yet been appreciated.

>  
>PCT is a theory of behavior, a explanation of how behavior works  
>at many levels from motor behavior to cognitive behavior. It  
>proposes that the focus of behavior is on controlling  
>perceptions; making the perceived world match what is desired,  
>with higher levels telling lower ones what states of their  
>perceptions to desire. It shows how the properties of a negative  
>feedback loop can be used to explain behavior.

>  
>The visible actions involved in this process are mostly

>misleading, because the point is not to control actions, but to  
>control effects that the actions produce in the world. Behind the  
>theory is a fairly solid science of control systems, so we can,  
>or can hope to, model specific behaviors using control theory.

. . . [Here Bill presented a (yet another!) perfectly clear discussion of the  
phenomenon of control.] . . .

>Where you need PCT is to explain how such things could possibly  
>occur. What is a mental image or a goal, such that it could  
>direct physical actions on a physical world to create its own  
>fulfillment? How can a goal that does not yet exist in reality be  
>brought into existence by any action? Why does this not require  
>the future to affect the present? How can an action be said to be  
>intentional when the intended result doesn't actually occur? What  
>directs actions so they produce intended results even when the  
>environment changes?

>  
>These are the kinds of questions that PCT answers. They are not  
>questions about what happens. They are questions about how  
>what happens could possibly happen in a physical or neural  
>system. PCT is about HOW IT WORKS.

>  
>Tom Bourbon (940310.1022) said

>  
>>PCT is about how our own behavior (behavior as actions that  
>>affect the environment) is *\*out of our control\**.

>  
>That's not quite right. PCT is a theory from which we can deduce  
>that actions are not controlled. The statement that actions are  
>not controlled is a prediction of the theory, not the essence  
>of the theory.

Agreed. But, as you say next, often we can (more or less) easily determine whether certain actions are themselves controlled, or are the uncontrolled means by which an organism (perhaps a person), or a manufactured control system, or an organism (person) modeled as a control system, controls certain of its own perceptual signals. Uncontrolled actions that serve the end of controlled perceptions are a fact of the phenomenon of control, whatever the system that does the controlling. Given an occurrence in which we determine that a person produces "behavior for the sake of controlling perceptions," *\*then\** we are in a position to use the model(s) from PCT to represent the person and to test the adequacy of our application of the models in that case. In my reply to Bill L., I should have made that point more clearly -- as I am trying to do in the exchanges with Richard Thurman concerning "representational momentum."

> We can easily verify this prediction by looking at  
>real examples of control behavior, and showing that if the action  
>were controlled to follow any preselected course through time,  
>the variable it is supposed to control couldn't be controlled. If  
>the driver selected a preferred way of moving the steering wheel  
>and then succeeded in moving it in that pattern, the car would  
>soon go off the road. So we can show that the prediction holds up  
>in real experience.

>



>But the theory doesn't say that: the theory says that the output  
>quantity is a function of the difference between a reference  
>signal and a perceptual signal, and so forth, which reduces to a  
>set of system equations that we can solve for the various variables.

Absolutely, positively.

> When we do so, we find that the action depends  
>primarily on disturbances, not on the intended state of the  
>controlled variable. Indeed, if the action is selected in  
>advance, it becomes an independent variable, and solving the  
>equations under that condition shows that no control will happen.  
>From that result, we can generate a verbal statement to the  
>effect that action is not controlled by a control system, and  
>find the required mapping of the model onto specific behaviors  
>that will show that this is, indeed, the result.

And that's the point I \*intended to make\* with Bill L., but I slipped a little.

Later, Tom

Date: Thu Mar 24, 1994 10:00 am PST  
Subject: Re: Perception and imagery

[From Rick Marken (940324.0900)] Tom Bourbon (940323.1530)

> Rick spoke of people as replaying the right perceptual signals, and of a  
>few people who seem to have "failed" to do so. Careful! This business of  
>saying that thoughts and imaginings are copies of the original things can  
>get you into trouble.

Ooops. I heartily apologize for what was a completely unintentional jab at you  
"non-imagers". Actually, I did acknowledge your existence in my post when I said:

>But when we imagine an apple (especially if we are not skilled at doing this  
>-- some people apparently cannot do it at all) we might fail to "re-play"  
>all of the "right" perceptual inputs because we have not learned (memorized)  
>all the right references to set in order to get a good, clear apple image.

The "some people apparently cannot do it at all" was a reference to people who  
can't form visual images. I said "apparently" because you "non-imagers" are as big  
a mystery to us imagers as we are to you non-imagers. I didn't mean to use the  
word "fail" in the sense of "short-coming". I think non-imagers must image at  
different levels than us imagers -- sequence, programs, principles -- without all  
those pesky sensations and configuration images getting in the way.

We should really think up some nice, simple experiments on control of imagery! Are  
you familiar with R. Shepard's mental rotation studies? Are you able to compare  
the images and decide whether they are the same or not? When I do it, I don't feel  
like I'm actually rotating a visual image; it's more abstract. We really need some  
PCT based imagery data.

Best Rick

Date: Thu Mar 24, 1994 1:19 pm PST  
Subject: Perception and imagery

[Michael Fehling 940324 11:35 AM PST] In re (Rick Marken--940324.0900)

>Rick said:

>

>We should really think up some nice, simple experiments on control of  
>imagery! Are you familiar with R. Shepard's mental rotation studies? Are  
>you able to compare the images and decide whether they are the same or not?  
>When I do it, I don't feel like I'm actually rotating a visual image; it's  
>more abstract. We really need some PCT based imagery data.

In addition to the Shepard and Metzler study on mental rotation, you might look at Steve Kosslyn's work. Steve has written two excellent books on the subject, "Ghosts in the MInd's Machine" and "Image and Mind." In these he describes some ingenious experiments on imagery and memory, imagery and thought, etc.

Incidentally, since many of the imagery experiments are demonstration studies, they offer useful examples of behavior which PCT might explain. Would PCT help us to understand these imagery phenomena better than Kosslyn's cognitive explanations? A careful re-examination of Kosslyn's phenomena in PCT terms would provide a powerful demonstration of PCT's explanatory power.

Michael Fehling

Date: Thu Mar 24, 1994 9:04 pm PST  
Subject: Re: PCT-ish ideas

<[Bill Leach 940324.22:47 EST(EDT)] >Tom Bourbon [940322.1700]

>And that's the point I \*intended to make\* with Bill L., but I slipped a little.

An I appreciate the effort... and effort that will no doubt be needed again and probably many times.

I believe that I understand the concepts, particularly with respect to physical actions, but truly appreciating and applying the PCT principles to "situations" is not always easy.

The visual "after effects" phenomenon and related things probably are not "controlled perceptions" in the proper sense. These things are more likely anomlies (where they appear) that are a result of "signal enhancement" processes for other perceptual purposes.

Another way of looking at what I am trying to say is that the effects that we see may be similar to the idea of an engineer testing the transient response of a Radar Antenna Pedestal by inducing a step change in the reference signal and then complaining about transient overshoot when his step change was well beyond the design specifications.

-bill

Date: Fri Mar 25, 1994 6:36 am PST  
Subject: Re: perception and imagery

[From Bill Powers (940325.0600 MST)]

Tom Bourbon, Rick Marken, Michael Fehling (940323 etc) --

Re: mental imagery

I think that vivid mental imagery arises only when one imagines at the sensation level. This is, I believe, rare -- it is certainly beyond my imaging abilities except for certain vivid dreams.

Imagine a child's alphabet block: a cube with letters showing on three surfaces. If you have sensation-level imagery, this mental image will interfere with what you are looking at in present time and you will blank out the present-time visual field for a moment, or at least suppress it. If you are imagining sensations, you will see the texture of the wood block, the serifs (or lack thereof) on the letters, the colors of the letters, the shadow of the block, and the differences of illumination on the various planes. You may even see that the block is not a perfect cube.

When you imagine at the sensation level, the experience at all higher levels is just as if a real block were present in the visual field, although it may lack some features. You can perceive whether the block is moving or stationary, how big it is relative to other objects, what the letters are in terms of symbolization, and (if you imagine several blocks) what the letters spell and what the word means in terms of other experiences. Most of the higher-level interpretations that would arise if the block were real would arise from the imagined block. Of course the knowledge that you're imagining would also most likely exist at the higher levels, so interpretations based on the reality of the block would be different. You wouldn't try to give the block to someone else. If that bit of knowledge were missing, you would be hallucinating.

Imagining at a higher level provides the same information but does not include the sensations. At the configuration level, you can imagine a cube without assigning it any color, texture, or shadings. It can still be incompatible with present-time perception: try looking at a cup while you're imagining a cube. If you're like me, the cubeness is overwhelmed by the cupness unless you close your eyes. However, at the relationship level you can imagine a cube enclosing or inside the cup, and then both can coexist. But unless you're imagining at the sensation level, only the cup will appear solid and real; the cube will be there, but only as an "idea." There's a "cubeness" signal, but no sensations.

The higher the level of imagination, the less specific the imagery to any particular perceptual input. If you imagine a cat at the category level, you may actually just think "cat" -- that is, that word, without seeing any cat-shape in imagination. You experience catness, but no cat.

Then there's computation-level imagination. If I have three cats and you have two, together we have five cats. If I were imagining strictly at that level, "cat" would be the same as "x" -- just a place-holder. A person who imagines at the configuration level might have images of cat-shapes twining between his feet (transition) and meowing (event) etc., which would probably make it harder to

think of this simply as an example of  $2x + 3x = 5x$ . The mnemonist S., described by Luria, had just this sort of problem: generalizing was almost impossible for this person; problem-solving involved manipulating actual situations in imagination, like stories. The color of the cats and their unwillingness to stay in one group would have been important considerations for S. If the cats had been fighting he wouldn't have been able to see how many there were altogether.

Artists seem able to imagine at low levels of perception. When Mary makes a line-drawing of a horse, she puts the pen somewhere on the horse and simply draws a continuous line around the horse -- ears, eye-bump, nose, neck, chest, leg, hoof, hock, belly, and so on. It looks to me like tracing a stencil of a horse, but the stencil isn't anywhere that I can see. It's amazing.

Imagination can happen in all sensory modalities, of course. Imagine reaching out and picking up a cold cup. Or imagine pushing a glass sideways until it falls off the table onto a concrete floor -- crash, tinkle. Imagine sucking a lemon. Imagine sticking a pin into the palm of your hand. Imagine the tune of Mary Had a Little Lamb. Imagine a little lamb, with mint jelly (not you vegetarians). Imagine being actively seasick.

You can't consider memory separately from imagination; the only difference is that memories purport to be replays or reconstructions of something that actually happened. We are so good at constructing imagined happenings that "memories" can easily contain a lot of details that never happened together. I don't know how we tell the difference.

Have you ever dreamed that you were reading something? When I do (sometimes holding a book and, as Mary puts it, reading through my eyelids), I get a definite sense of readingness, of following the words, of some sort of story going on -- but as soon as I try to make out the words a little more clearly, I realize that there aren't any words. All I'm getting are the higher-level experiences that would be arising if I were actually reading, without any actual reading. This sort of experience is helpful in understanding how specialized the levels of perception are. The computation level is ONLY concerned with computations -- it doesn't care what the symbols mean or why the computation is going on.

So -- some early-morning musings. Tom, I don't think your situation is very unusual. You just don't imagine at the sensation level. How about in other modalities than vision? Sound? Smell? Taste?

Best to all, Bill P.

Date: Fri Mar 25, 1994 8:48 am PST  
Subject: An important recent publication for PCT

FROM CHUCK TUCKER 940325

On Tuesday I received my latest issue of THE SOCIOLOGICAL QUARTERLY, the journal of the Midwest Sociological Society which contained the Presidential Address by Clark McPhail entitled "The Dark Side of Purpose: Individual and Collective Violence in Riots" (Citation: McPhail, Clark. 1994. "Title." THE SOCIOLOGICAL QUARTERLY 35:1-32.) The abstract reads:

A review of riot and riot participation research requires rejection of structural strain and deprivation-frustration-aggression explanations. The complex and varied riot phenomena to be explained require a model of purposive actors. Competing models are reviewed. A perception control theory explanation of riot participation is supported by experimental and ethnographic evidence.

This is important because it speaks to those that claim that PCT is only applicable to the movement of lines on the monitor (which it does also) and not to phenomena that are interactional and social.

Soon we should see the publication of Kent McClelland's paper in *SOCIOLOGICAL PERSPECTIVES*, the journal of the Pacific Sociological Association.

Both of these publications should be useful to our efforts to convince other that PCT should be taken seriously.

Regards, Chuck

Date: Fri Mar 25, 1994 9:37 pm PST  
Subject: To Bill Leach on dynamic modeling

[Cliff Joslyn 940325] >[Bill Leach 940322.18:35 EST(EDT)]

>Ok, then you tell me... Where in the HELL are these insane "models"  
>coming from that continually predict the end of civilization about once a  
>week on the evening news?  
>How about Jay Forrester, Dennis and Donna Meadows? (all from MIT, from an  
>easy source).

OK, now we're getting somewhere. Jay Forrester is in fact a systems theorist, and in fact forms a link from the dynamic modeling crowd to ST. I would point to George Richardson (*Feedback Thought*, U Penn, 1991) as his intellectual descendant, although he's more of a micro-modeler.

I regret that I am not deeply knowledgable about Forrester's specific work (e.g. *Industrial Dynamics*, *World Dynamics*), other than knowing generally about dynamic modeling methods and his DYNAMO language. While I believe that his work has been influential on others, I do not believe that it has been so in his capacity as a general systems theorist, but rather for his specific methods. He is not a current active modeler (is he even alive?).

I'm a bit more familiar with the Meadows' work. I don't believe that they draw from ST specifically, except to the extent mentioned above that they build on Forrester's work. My understanding is that *Limits to Growth*, from 1972, is truly apocalyptic, and also rather crude. It was highly influential, undoubtedly beyond its true significance, just because it was novel.

It should be noted that truly GLOBAL models like the Meadows', linking human and non-human macro-ecology with economics, are quite rare. This is partly because validation ability decreases with generality. So they have little competition that would naturally serve a self-correcting feedback function in a scientific community.

However, their work also fostered a huge, mostly POLITICAL, backlash, much of which was also undeserved. Their current book Beyond the Limits is an attempt to improve their model and also answer their critics. I heard them interviewed on the radio, and they seemed to have a fine scientific attitude. The controversy surrounding them remains highly politicized.

>Cliff, I doubt very much that I would really want to argue with you about  
>the value of ST as I suspect that you practice the field.

Indeed. I try to. Well, at least I CLAIM to. To the extent I can get PAYED to do it. You know, kind of like control theory. ;->

>Since you seem quite at home here then I must assume that you hold the  
>principles of modeling real systems as put forth most elegantly by  
>Bill Powers as supreme.

I hold the Powers' model of the architecture of living (but not all REAL) systems as a central, but not the sole, part of my intellectual world view, based as it is on systems theory and cybernetics.

>Research into other aspects of modeling do not bother me. It is only  
>when someone CLAIMS to have valid predictions about a physical process  
>using a model that is CLAIMED to be a physical process model but that by  
>demonstration has PROVEN that it is NOT such a model, that makes my blood boil!

Well yes, that would be a problem. Do you have an example?

Cliff Joslyn

Date: Sat Mar 26, 1994 3:49 pm PST  
Subject: Re: To Bill Leach on dynamic modeling

<[Bill Leach 940326.13:22 EST(EDT)] >[Cliff Joslyn 940325]

Cliff;

Thanks for the excellent response message. I suppose that it is probably often that a scientist finds that once he (or she) has "said something" to the media, that it is all but impossible to correct misinterpretation. The is unfortunately yet another "attitude" expressed by many members of the scientific community and that is the idea that a "little stretching" of the truth is "an overall good" if it results in (additional) funding.

Add to that the conduct of people like Dr. Sagan, Dr. Lovis, Dr. Schneider and others that flatly admit that "not revealing doubts", "stretching the truth" and outright "activism" or legitimate concerns of science. Mix that in with a bit of media hype and you have a situation where rational people begin to wonder about the entire "scientific community"... wonder if their integrity is no greater than that of typical politician!

>I hold the Powers' model of the architecture of living (but not all  
>REAL) systems as a central, but not the sole, part of my intellectual  
>world view, based as it is on systems theory and cybernetics.

Maybe it is I that is overly simplistic but just what is wrong with Bill's standards for ANY 'real world' model?

I don't see his view as being a problem with using other modeling techniques as long as one recognizes that when models disagree with reality, you can claim neither "reality being wrong" (and yes there are those that do), nor that the model is necessarily "telling" you anything useful short of "you don't know what is going on."

I don't have a problem with statistical models BTW, but INSIST that one recognize that such models potentially suffer from serious deficiencies when it comes to prediction. That does not mean that they are not useful nor even reliable. It does mean however, that their very usefulness and reliability can be the very "source" of misunderstanding about actual processes.

>>Models

>Well yes, that would be a problem. Do you have an example?

The very climate models that we have been talking about for starters. The so called "Atmospheric Ozone" models for another.

-bill

Date: Sat Mar 26, 1994 6:35 pm PST  
Subject: Uncontrolled perception, Myths

[From Rick Marken (940326.1730)] Bill Leach (940324.22:47 EST)]

>The visual "after effects" phenomenon and related things probably are not  
>"controlled perceptions" in the proper sense.

They are definitely not controlled perceptions. I think it's important to remember that we can perceive without controlling perceptions. I think of it this way (which, of course, is the right way): what we experience is the state (firing rate) of afferent neurons. The firing rate of each neuron is the state of some perceptual variable -- an intensity, sensation, configuration, etc. From the perspective of the person who IS these afferent neurons, the experience is one of loudnesses, colors, boxes, arpeggios, cadenzas, C programs, religions, etc. You can experience these perceptions (the firing rates of afferent neurons) without doing anything about them; the radio is as loud as it is, the computer is the color it is, the box is box shaped. We control perceptions when we do something to bring them to a particular level -- we adjust the loudness of the radio, paint the computer, break the box into a flat palette.

An aftereffect is just a perception that conflicts (based on higher order perceptions) with what we think to be "the actual state of the world". For example, when I stare at a waterfall for a minute and then look at the stationary rock cliff next to it, the rock seems to be drifting upwards. The movement of the rock is a real (transition level) perception which I think of as "illusory" because I believe that the rock remains stationary (a possibly fanciful belief here on the Pacific plate). Controlling was involved in holding my gaze on the waterfall and then moving it to the adjacent rock cliff. But the aftereffect is just a perception; it is not a controlled perception, a fact which can be

demonstrated with a rotating spiral disk. Just stare at the rotating spiral for a little while (the staring does involve a control process but you have no control over rotation of the spiral). Now I abruptly stop the spiral; you have done nothing but you now see the spiral rotating in the opposite direction. The state of this movement perception changes without you doing anything about it. It's purely a result of the nature of the perceptual function that produces the neuronal firing rate that we experience as movement one way or the other (depending on the rate of firing).

-----

In honor of Passover, Easter (and the real event -- the equinox) and in the hopes of stirring up this sleepy net with the only kind of talk that seems to get the passions stirring -- religion and politics -- here is my completely unsolicited comparison of the Passover and Easter myths -- from a PCT perspective.

First, it is interesting to note the similarity between the two myths (at least, I perceive something similar about them). This similarity is not in the details -- the lower level perceptions -- but at a higher level; possibly the principle level. I perceive in both myths the principle of "rebirth" or "renewal". In the Passover myth, the Hebrew people are mistreated by Pharoah and finally escape to freedom; they are reborn as a people. In the Easter myth, Christ is mistreated by the Pharisees (anyway, by the "conventional religionists" of the time), to the point of being killed, and then he is literally reborn. Both myths seems to convey the general spirit of the equinox as well -- which is springtime, when the world is "reborn". So the myths teach us something about hierarchical perception; perceiving a principle in many different lower order perceptions.

Now comes the controversial part.

Even though the two myths seem to have the same "deep structure" (how's that, Avery?), I find the Passover myth to be extremely unpleasant; it doesn't make me feel good. It doesn't lift me. (This is unfortunate because every year I have to go to the family seder and join in the retelling of this myth in gory, longwinded detail -- my step-father is Orthodox; oy vey!). On the other hand, I find the Easter myth moving and beautiful. Even though both myths are about rebirth, I find one moving, the other flat. Why?

It could be because I'm a "self hating" jew (I'm actually nuts about myself) or a jew for jesus (I actually believe only in atheism and PCT) but I think the reason is based on something else (that IS related to PCT). The "rebirth" of the Hebrews is achieved by violence (the ten plagues); the rebirth of Christ is achieved the PCT way -- by going up a level (in the myth, of course, it's up to the level of god). I have heard all kinds of justifications for the Passover myth being the way it is; but frankly my response to it is largely emotional. The Passover myth feels bad and I think it has to do with the fact that I don't like the approach to conflict resolution described in the myth and celebrated at the seder. The Easter myth, on the other hand, is pure PCT. Here is a guy (Jesus) who could not have been given more shit -- humiliation, pain--the worst "disturbances" I can imagine, and yet he does not fight back or use magic powers to hurt his enemies. What he does is try to get them to go up a level -- quite a remarkably PCT myth. Bad stimuli (and they don't get much badder than what came at JC) do not CAUSE bad behavior in Jesus. The oppression didn't control Jesus; Jesus was in control of his oppressors (in the PCT sense).



I think the people who wrote the Easter myth solved the conflict of oppression (the one described in the Passover myth) in the PCT way; by going up a level. I think that 2000 years ago there was the sudden emergence in a few people (at least, on this side of the globe -- there were apparently already many in the east and possibly in Africa) of a level of perception that is higher than any described in the so called Old Testament. The emergence of this level of perception made it possible for people to see what had been taken as requirements -- thou shalt's -- as options to be used in the service of achieving higher level goals. I think this level of perception also emerged briefly in the east -- in the form of the Buddhist myths. Unfortunately, most people who believe in these myths (I think) cannot perceive them at the same level as their authors. Hence, we get, for example, the christianity of today, which thinks solving problems by war is just fine -- as long as god's on your side.

There. That was my attempt to apply PCT to biblical archeology. That should ignite some sparks, I hope.

Best Rick

Date: Sun Mar 27, 1994 9:16 am PST  
Subject: Re: Uncontrolled perception, Myths

<[Bill Leach 940327.10:53 EST(EDT)] >[Rick Marken (940326.1730)]

Thank you Rick. I was not sure that I was on "solid ground" with that assertion. I am living on the east coast so I don't currently have the other problem. :-)

It was my sense that while we no doubt learn a great deal about human physiology through such anomalies, their appearance is more of "glitches in the operation" than evidence of some sort of profound insight into behaviour.

>Religion, Passover etc.

That is interesting though no doubt I will have to think further about what you have said.

Finished Ed's "Love Guaranteed" and am about through "Freedom from Stress". Ed really "lays it out there"! The manner in which he relates PCT to "everyday living" is impressive indeed. I am not getting the "Ah!" feeling from reading his work such as I did from BCP but OTOH there is a lot of "of course that is right" sort of feelings and recognition that he shows one how to "cut through the cr\*p" to get to the issues.

I had a Christian counselor friend read "Love Guaranteed". His only strong concern was the apparent failure in "dealing with guilt." I know that this was a perception that I developed in reading the book (but I did not mention this to him prior to our discussion of the book).

My impression is that he did not really understand the significance of PCT but he and I tend to "operate" with a very different "world view" filter. I tend to suspect that he "automagically" accepted some of the presentation (based upon his own success in counseling and some of his own experience in dealing with significant life events). OTOH, I perceive that he does not have an "engineering

bent" at all so I tend to suspect that much of the implications in the presentation were missed.

Trying to think about "guilt" from a PCT standpoint:

"Guilt" can be a very strong emotion and as I have believed for many years (and I see PCT supporting), guilt is the result of internal conflict. From a PCT standpoint, I would see guilt as a case of stress where there IS some recognition by the subject of the nature of the stress. That is, in the case for stress called "guilt" there is at least some awareness on the part of the subject for the existence of and the "nature" of the conflict.

In the "Christian" philosophy the term "Sin" could in a general way be taken to be the violation of a moral standard. If this moral standard is indeed an internal standard for the subject, then "Christian Sin" and "guilt" are much the same thing (and for my purposes in this discussion Sin as perceived by an outsider that does not result in feelings of guilt for the subject is not a concern. I am also not trying in this to deal with the "guilt trips" that many "religious" try to impose upon others. Guilt is indeed involved in those cases but the standards in conflict may have little or nothing to do with the sort of thing that I am trying to discuss here).

The Christian philosophy maintains (roughly) that the solution to a problem with "Sin" is to 1) "repent", 2) "ask forgiveness of the offended" and 3) "ask forgiveness of God".

It seems to me that as with most long standing religious beliefs relating to "social behaviour" (that is interaction with other people), there are significant elements of truth present when one gets down to the basics.

To handle guilt, it seems that "repentance" could be seen as essentially "coming to terms" with yourself; figuring out what is really the problem, planing and then acting appropriately. In my opinion, Ed handles this part quite well in "Freedom from Stress" (in what I have read so far).

What I am getting to (awkwardly I realize) is a potential component of item 2 above: "Sense of justice"

Essentially, I suppose that his PCT examples cover this as well but so far the "standard" has not been mentioned. If one were counseling along the lines of Ed's examples and the standard came up from the subject then the same process of dealing with it should apply.

Getting someone to resolve their behavioural inconsistencies will reduce a major fraction of the stress they were experiencing. However, if they perceive that some action of theirs has harmed someone else then there is still an unresolved conflict... failure to satisfy their concept of justice.

It can well be that the proper solution is to re-evaluate the perceived "hurt" and take no action. OTOH, it may be necessary to perform some behavioural act to correct the error including "asking for forgiveness". It also may well be that I am seeing this "conflict" as too important.

Item 3, of course depends upon the subject's "systems concepts" but could be handled as in any other conflict.

-bill

Date: Sun Mar 27, 1994 12:19 pm PST

Subject: RICK & TIME - RKC

<Bob Clark (940327.1510 EST)>

Rick Marken (940320.1600) Subject: Time and consciousness

For ease of communication, I am discussing "TIME" separately from "CONSCIOUSNESS."

>The E. Coli navigation process works without any explicit  
>representation of time.

In the discussions of E Coli, the observer clearly perceives and, at times, discusses temporal variables. For many purposes, people treat time as an independent variable. Derivatives with respect to time (velocities, accelerations, etc) are also familiar. Predictions and planning revolve around perception of time.

I have not suggested that "all living things" can perceive time. But there is much evidence that humans do. "Lower" animals may or may not. A subject for study.

>I don't see how time, per se, can be perceived. Time can be  
>controlled only if it can be perceived; how do we perceive time? Is  
>it perceptible?

Yet, in the post which led to my remarks, you suggested a conscious time delay between pushes of the bar. How can you do this unless you can perceive time?

Perhaps the logic should be reversed: "If a variable is observed to be controlled, then it must be perceptible."

Many temporal variables are controlled. Consider scheduling of meetings, planning dynamic programs, measurement of reaction times. etc. Some of these variables involve derivatives and some, like your proposed delayed bar pressing, are concerned only with duration.

You quote me:

>>Temporal variables are involved in a great many of the higher level  
>>perceptual control systems.

>Yes. And I think what is controlled in these cases is something  
>like partial derivatives --  $dx/dy$ .

Here you seem to agree with me. Perhaps you are restricting your agreement to time derivatives, rather than time as a perceptual variable. That is, using "dt" but not "t." Can the derivative be meaningful without perception of the variable?

In other posts you often use "time" and "temporal variables" as though they are among your own perceptual variables. Not so?

You ask (quoted above):

>how do we perceive time?

This is an important and intriguing question. However, there are sensory perceptions where the mechanism is not known. This is particularly true for the individual. Consider a toothache. How is it perceived? Yes, we can talk about nerve endings, etc, but to one who knows only that it hurts, it is still a perceptual variable. Indeed, how many people know how hearing works? Or orientation sensors? Many people have very little knowledge of their own sensory systems.

But the perceptions are there.

"Control" and "controllable variable" are important concepts. And perception of the related variables is necessary for control. However, there are a great many perceptual variables that cannot be controlled by the observer. Color of grass. Clouds in a sunset. Mountains in the distance. Taste of vanilla. Smell of frying bacon. And many more.

A color (perhaps of paper) can be selected from among colored sheets. This involves comparing the available colors with a remembered (imagined) color.

A painter can control the color he applies by selecting the pigments that he mixes. Even if he is trying to match a color he sees, It is necessary for him to mentally record that color as he looks back and forth, comparing it with the color of his paint.

Have I made my point with respect to "time?"

"Consciousness" will be discussed separately.

Regards, Bob Clark

Date: Sun Mar 27, 1994 7:20 pm PST  
Subject: yet another yet another yet another yet another

<[Bill Leach 940327.22:03 EST(EDT)] >NET

What, if any, is the PCT position on hypnosis?

-bill

Date: Mon Mar 28, 1994 1:23 pm PST  
Subject: Neg. feedback & time

From Tom Bourbon [940328.1351]

The latest issue of Science (18 MArch 1994, Vol. 263) contains three articles on biological circadian rhythms in fungi and fruit flies. Together, the three articles merit a blurb in the "Perspectives" section: Terry L. Page, "Time \*Is\* the Essence: Molecular Analysis of the Biological Clock," pp. 1570-1572. Check it out, if you want to see an example of a "negative feedback loop" as one is

construed in contemporary biological science. Afterwards, ponder the common assertion that PCTers are unfair when they say that many life scientists conceive of a control system as an input-output device, with behavior as its output.

Later, Tom

Date: Mon Mar 28, 1994 4:54 pm PST

Subject: Re: representational momentum

[From Richard Thurman (940328.1420)] >Tom Bourbon [940323.0902]

Sorry to leave this thread hanging for so long. Last week was one of those +crunch+ weeks and I was unable to attend to the PCT traffic as much as I would have liked. One must make a living though!

Concerning representational momentum --

>>I may have to actually do some modeling and run some subjects  
>>myself. I'm sufficiently curious to give it a try, but its out of my  
>>field of research and I feel a little like a fish out of water.

>Richard, \*before the modeling\*, THE TEST. Determine whether the perceptual  
>phenomenon is controlled, or uncontrolled. If you don't do that first, what  
>will you model? Should you try to model RM as a controlled perception, or  
>as a "side effect" of control?

Tom, I was a bit surprised by the forcefulness of your expression in this post. My first reaction was, 'of course I will do the test -- who said I wouldn't?' But upon reflection I realize that you have been telling me this all along, and because I already -- internally-- realized that 'doing the test' is an integral part of 'doing PCT' I had basically minimized your suggestions. That, coupled with my wild speculations must have provided quite a disturbance to you.

Ok, I am done speculating (for the moment). Can you give me any suggestions for how to go about doing 'the test' in this particular context. I have thought about doing the following:

1) Since I am not completely convinced that the phenomenon of representational momentum is 'real' I plan on replicating the standard experiments. While I believe that the researchers get the results they say they do, I am not convinced that the results are not just a statistical fluke. I want to see the same patterns emerge with an individual, not with an average individual.

2) Given I can find an individual who exhibits the same kind of behavior typified in the literature, I will conduct a series of 'tests for the controlled variable.' This will consist of doing as you and Rick have suggested. That is, find a way to turn the uninformative S-R experiment into a control task. Most likely I will do this by having the subject watch a figure rotate for three frames. Then I will place a fourth figure at various degrees of rotation (from the third) and ask the subject to rotate the figure so it matches the third frame. I should be able to find two phenomena. First, the subject should rotate the figure so that it does not match the third one. Instead, the subject should rotate it slightly beyond the third. Second, the subject should not rotate the figure when it is within some degree of tolerance. What I mean is, at some point the rotated fourth figure will

probably be exactly (or close enough) where the subject would have placed it, and the subject will do nothing.

This reflects about half of 'the test'. Now what?

>> I had  
>>been looking at this as a top-down kind of thing -- Higher levels of  
>>the hierarchy doing their reference setting thing. I see now that you  
>>(and Tom, and Rick) are looking at this from the bottom up.  
>>Perceptions percolating up from intensities to and through transitions.  
>  
>When you draw the top-down vs bottom-up distinction, you are probably  
>thinking of the reference signal working in a top-down fashion, with  
>perceptual signals "percolating" bottom-up. Am I right? In a PCT model,  
>there is no way to isolate those two parts of the loop, not any more than we  
>can model a control system as \*either\* "inside the system," \*or\* "outside  
>the system." Both sides are present and working, all the time.

Yes, as soon as I posted the message I realized that it was inappropriate to look at the process as either 'top-down' or 'bottom-up.' But not (I think) for the same reasons you say. You are referring to a single control loop. I was referring to a hierarchy of loops, starting with intensities and ending somewhere around the transition level.

>You express  
>an interest in modeling the RM phenomenon as though it results from rapidly  
>terminating a reference signal; we (BP, RM and I) seem to have converged on  
>the possibility that the phenomenon arises in a perceptual input function.  
>What is needed now is some experimental work to determine the "real" nature  
>of the phenomenon; \*then\* the modeling.

Ok -- just give me some pointers and I'll give it a shot.

>Here is another line of work that might bear on your questions.

>The effects described in these two lines of work seem to match what we would  
>expect if setting a reference signal for a new position of the eyes leads to  
>a new reference level for location. The pre-eye-movement perception is of  
>the stationary flashing light "moving" in the direction \*opposite\* the  
>direction of the intended saccade. It's as though a new reference is set  
>for where the center of the visual field (the line of gaze) should be, then  
>the flashing light is seen (and physiologically "registered") as moving away  
>from "where it really is" to "where it is supposed to be" -- all before the  
>eyes begin to move. When the eyes move, they create the "trajectory" of the  
>flashing light that occurred in the pre-movement experience. Some pretty  
>neat stuff.

That's fascinating! It also reminds me of something I had wondered about for some time. About a week after I had read BCP I was watching a PBS show on cocaine addiction. One bit of research really startled me and seemed to confirm some of the connotations of hierarchal control and setting references. In one experiment, they hooked up a former addict to several monitors, set a box in front of him and left the room. When he opened the box he found some cocaine and other drug paraphernalia. The monitoring equipment showed that for a brief time the subject's body was producing the same sensations that he would have received if he had taken

the drug. It's as if his body were telling him, 'Feel like this!' Is this in line with PCT?

Actually this addiction study shows the exact opposite of the vision studies. In the addiction study the research indicated that the subject was experiencing a kind of phantom 'high.' To match the vision studies, the subject should have experienced a phantom 'low.' Or perhaps the monitoring equipment was not sensitive enough to determine if it was a high or a low -- only the the same neural pathways were firing.

Fascinating stuff. . . .Richard Thurman

Date: Mon Mar 28, 1994 5:08 pm PST

From Ed Ford (940328.1640) To All...

It's been nearly three months since I've posted anything on the net. Time goes quickly.

Have been working a lot out of town and writing a new book and just haven't kept up with the net. The book, now in manuscript form, is called Discipline For Home And School. It just sort of developed as a result of having worked in schools all over the country, especially with two local inter-city elementary schools (with lots of discipline problems), where I have spending a great deal of time, much in a non-paid capacity, but learning lots. Have been working especially with children in the classroom, time-out room, in all sorts of ways. It's very interesting working with living control systems, both adult and children, and finding out what works and what doesn't. It continually reconfirms PCT and helps one develop a profound appreciation for the guy behind it all. Understanding and explaining PCT with models is one thing, and I have a profound respect for those who do this work and have learned a lot from them, but working with children with a wide, and at times destructive, variety of reference signals and perceptions is something else. It is very different. Helping them deal with their worlds such that they can succeed (for those who want to) is very challenging. This is dealing live, without rehearsals. I love it but to succeed, you have to continually think and hang on to your sense of humor. The traditional approach of discipline is s-r hasn't worked, otherwise no one would look to me for help. I define discipline as teaching children to respect the rights of others through responsible thinking (not behavior, a word everyone uses). The book should be available July 1st, given the critiques I've sent to selected readers pass muster, or at least aren't too challenging. Making sure everything fits and makes sense and, more importantly, is practical is my goal.

Bill Leach...

Thanks for the comments on FFS and LG. Regarding guilt. The feeling of guilt is caused when a person establishes a reference signal about something they would have liked to have done differently in the past and, as a way of dealing with the past, continually dwells on what might have been. This is trying to change something over which there is no control, like a dog chasing its tail. To me the solution comes from satisfying one's goals in the present. My experience over the years with clients tells me that when a person is doing well in the present, the past becomes less intrusive. Once a person begins to build confidence in their ability to satisfy their internal goals, then past failures seem less real and

becomes, as I said, less intrusive. I believe you said the man who had the problem with the absence of guilt in my book was a minister. He should look to his model. Christ never dealt with the past expect as something to change in the present, but rather would admonish his listener to "be good and sin no more" in the present.

Concerning Dag's work....

For those who had trouble with the role-play I helped Dag with, the purpose is to teach the process, not how to work with difficult cases. Once the process is learned, then there are reference points to go to for the manager/counselor as the client offers excuses. I spent 5 years teaching industry and this was highly successful. I just prefer working in corrections and schools. More of a challenge.

Best, Ed

Ford, 10209 N. 56th st., Scottsdale, AZ Ph.602 991-4860

Date: Mon Mar 28, 1994 5:43 pm PST  
Subject: Review of "Freedom From Stress"

[From Rick Marken (940328.1500)] Bill Leach (940327.10:53 EST)

>Finished Ed's "Love Guaranteed" and am about through "Freedom from  
>Stress". Ed really "lays it out there"! The manner in which he relates  
>PCT to "everyday living" is impressive indeed. I am not getting the  
>"Ah!" feeling from reading his work such as I did from BCP but OTOH there  
>is a lot of "of course that is right" sort of feelings and recognition  
>that he shows one how to "cut through the cr\*p" to get to the issues.

Dag Forrsell has been after me for at least a year to write a review of "Freedom From Stress". So here goes:

"Freedom From Stress" begins with a clear statement (in Ch. 1, "Misery") of the PCT perspective on the nature of personal problems: our problems are not caused by outside events (like other people) but by ourselves; the way to solve personal problems, therefore, is to change one's self, not to change others (which is impossible anyway). The question, of course, is how and what to change in order to solve our problems. Chapter 2 suggests that we do this by figuring out what is most important to us -- we "prioritize" and determine what perception we want most to get under control -- and work to get it where we want it.

The problem with chapter 2 is that it seems to suggest what might be called a "hallucinatory" approach to getting perceptions under control. In Chapter 2, Bob decides that he wants to make things better in his marriage; he wants to get closer to his wife but she's always doing things that make Bob uncomfortable -- criticizing, arguing, etc. Bob perceives his wife as "a cold, stupid bitch" (apparently, Bob does not like to be close to cold, stupid bitches -- no accounting for taste). It is suggested that the way Bob can solve his problem -- to get closer to his wife -- is to change the way he perceives her. Bob is asked "Last night, when she [the wife] was after you about something, suppose you had thought about her as warm and loving ... do you think you would have told her to get off your back?" Bob realizes that he would not have told off a warm loving wife and concludes "I guess you treat people according to how you are thinking about them at the time".



It sounds to me like the solution being recommended here is for Bob to perceive his wife as warm and loving when she is "after him about something". This is what seems hallucinatory to me. The perception of someone being "after" you is (for me) on the other end of the same perceptual scale as "being warm". Asking Bob to perceive his wife as warm seems like asking a person to perceive a hot burner as cold. People who change their perceptions in this way can solve their problems -- but they create others in the process; for example, their flesh get's burned off while they hold the hot burner that they perceive as cool.

Perhaps I am reading Chapter 2 incorrectly; maybe the recommendation is to become conscious of (and control) other levels of perception of the wife. This would make a great deal more sense but it would also take some explaining; the levels of perception are described in Ch. 2 but it would help if this description were related to the earlier discussion of how perceiving the wife differently could help solve Bob's problems. If it were pointed out that being "after you about something" might be a disturbance to one kind of perception (like you perception of your own right to do things as you wish) but not a disturbance to another kind of perception (like you perception of the marriage relationship -- which involves doing what satisfies one self as well as each other; the being "after" you is just the wife's efforts to coordinate the relationship from her side) then the discussion would be clearer (to me).

Whatever is meant by "perceiving in a new way" or "constructing a different perception" should be made very clear because it seems to be fundamental to the therapeutic argument of "Freedom From Stress". That argument seems to be that we can make things better (that is, control better) by learning to perceive things differently. For example, in Ch. 5 we learn that "quality time" works (to improve relationships) because "it will allow you to construct a new meaning for \_wife\_ ...You see, the reason quality time works is that we construct perceptions of people based on how we have experienced them." (p. 57). So again we see the same point made in Ch. 2; you can make things better by learning to perceive them differently.

The emphasis in "Freedom from Stress" is on solving problems by "re-perceiving". What seems to be missing from "Freedom From Stress" is the PCT idea of resolving problems by "re-wanting". I think the basic point of "Freedom From Stress" is that the outside world doesn't cause our problems because we can perceive that world as we like; we have control of HOW we experience the world. But we don't really have control of how we perceive the world; our perceptions of the world can (and do) change as a result of reorganization -- but we can't control HOW we will end up perceiving after this kind of learning process.

I believe that PCT would emphasize the fact that our freedom from stress does NOT come from our control of HOW we perceive; it comes from our control of what we want. Freedom from stress comes from recognizing that perceptions are stressful (or not) only because WE have determined that certain states of those perceptions are the only ones that will do. We are the one's who determine (by our wants) what is stressful. We are also the one's who determine our own conflicts -- by having incompatible wants; wants that demand two different states for the same perception.

Conflict is a "want" problem; but, consistent with the emphasis on constructing perception, "Freedom From Stress" sometimes describes conflict as though it were a perceptual problem. In Chapter 6, conflict is said to be "created by the

disharmony you're experiencing. You perceive that you can't get everything you want either at home or with Fred". This may just be a different way of saying that conflict IS having incompatible wants (not incompatible perceptions) but it sounds to me like conflict is being cast as perceptual problem, Reading this discussion of conflict, I would conclude that the way to solve such conflicts (based on what I have read earlier in the book) is to learn to perceive things differently. Of course, the only way to solve conflicts (without reorganizing) is to learn to WANT things differently.

"Freedom From Stress" has some nice advice for people who have personal problems. But I think it focuses too much on solving problems by "re-constructing" perceptions and too little on solving problems by "re-setting" wants. Both are possible; it's possible to change how we perceive; it's possible to change what we want. But I think it's not even feasible to do the former in a non-random manner (except by becoming conscious of perceptions at different levels) while the latter can be done using the method of levels.

I think it would be great if Ed's next book explained, in the easily accessible manner in which he wrote "Freedom From Stress", how to do the "method of levels".

Best Rick

Date: Mon Mar 28, 1994 7:46 pm PST  
Subject: Re: Hello again!

<[Bill Leach 940328.21:05 EST(EDT)] >Ed Ford (940328.1640)

Welllll, good to 'meet' you sir and you are both welcome and deserving of the praise.

>Guilt

I agree that the past is the past and that worrying about the past is worse than useless (much worse in fact).

I think that the ideas that are "stirring around" in my head about "guilt" are probably related to what we usually call "a sense of fail play."

Personally, I think that I am relatively "guilt free" but it definately was not always that way for me. At one time, I essentially allowed others to "put guilt trips" on to me. I believe that I am rather immune to that sort of thing anymore.

However, if I "wrong" someone, even if I can not really "undo" the wrong, I probably would still feel that I should attempt to "make it up" to the other person.

As I try to view this from a PCT perspective, a person in such a situation needs first to decide if they really do feel that they have done something to someone else for which they may owe some "debt." If the conclusion is "no" then forget about it but if the answer is yes, then a specific plan of action should be designed.

Carry the plan out... and then forget about it.

-bill

Date: Mon Mar 28, 1994 10:16 pm PST  
Subject: Re: Review of "Freedom From Stress"

<[Bill Leach 940328.22:40 EST(EDT)] >[Rick Marken (940328.1500)]

This should be fun :-)

>... change their perceptions in this way can solve their problems -- but  
>they create others in the process; for example, their flesh get's burned  
>off while they hold the hot burner that they perceive as cool.

Nicely put Rick but OTOH, I feel that Ed has done a good job of explaining something that anyone working in the "motivational" field already knows is true. While there might be some lack of precision in his explanation and I suppose that he might have gone into more specific details... but basically, I feel that he has gone a long way toward explaining something in a useful fashion.

I recognize that there is not a whole lot of difference between what Ed seems to be saying there (form a perception of wife as a warm loving person when experience with her is not consistent for that perception) and telling someone to be psychotic.

However, the reality is that when we do manage to induce ourselves to perceive someone in a "postive" manner, our own behaviour actually does usually "cause" that other person to begin to themselves behave consistent with the perception that we "forced".

As Ed quite clearly points out however, when employing his PCT methods a persons perception of another person WILL change and the change may not necessarily be "postive". Of course, at least some of us believe that if employing PCT methodology to a relationship results in termination of the relationship, that in itself is probably a good or positive event.

>The emphasis in "Freedom from Stress" is on solving problems by "re-  
>perceiving". What seems to be missing from "Freedom From Stress" is ...

The techniques that Ed does mention and the criteria for them do seem correct. He do not try to tell the subject what they should do for "quality time" but rather he trys to explain what characteristics such events must have to be able to support a "new" perception.

>I believe that PCT would emphasize the fact that our freedom from stress  
>does NOT come from our control of HOW we perceive; it comes from our  
>control of what we want. Freedom from stress comes from recognizing that  
>perceptions ...

I think that you are correct but again may be missing the point. The techniques that Ed uses result in an acutal change in reality.

He is not trying to cause the subject to change their wants (though he certain does insist that the wants be examined) in the postulated examples because, I suggest, that the "wants" appeared to be realistic and achievable.

In particular and I suppose that this is an unverifiable opinion, there is probably a strong, literally undefinable, desire for companionship that is fundamental to being human. This desire can be "put off", delayed, suppressed or whatever but not without some level of stress.

In the "real world" I don't think that Ed is being unreasonable to try to encourage people to use techniques that are likely to enhance interpersonal relationships because of their propensity for affecting the perceptions of others.

>But I think it focuses too much on solving problems by "re-  
>constructing" perceptions and too little on solving problems by "re-  
>setting" wants. Both are possible; it's possible to change how we perceive;

Again, I see what he is saying as an effective means of achieving the wants (once correctly defined) by recognizing the wants of others. Obviously if the method that he proposes does not work (as will certainly be the case at least once in awhile) then the wants will have to undergo a bit or modification (ie: the spouse may have to change, the employer might have to change or whatever).

-bill

Date: Tue Mar 29, 1994 6:53 am PST  
Subject: St. Marken's Theology

[From Oded Maler (940329)] \* [Rick Marken (940326.1730)]

\*

\* In honor of Passover, Easter (and the real event -- the equinox) and  
\* in the hopes of stirring up this sleepy net with the only kind of talk  
\* that seems to get the passions stirring -- religion and politics --  
\* here is my completely unsolicited comparison of the Passover and  
\* Easter myths -- from a PCT perspective.

I'll respond to this provocation just in order not to frustrate you for being wrong about my controlled perceptions. I must admit that I am rather ignorant in the new testament and I'm confused concerning the details of Jesus going up and down the stairway to heaven, so I'll not comment on that. [Btw, I read a very nice book, "The gospels according to Jesus" by the great Portugese writer J. Saramagou].

As for the Passover myth, contrary to your alleged introspection, it is the very same ethos that you have internalized so well. "Spill your heated anger on the gentiles who did not know you, on kingdoms which didn't pronounce your name" - what a better summary can there be to your attitude toward established psychology? A true (but silent) scream, coming from the depth of the small PCT ghetto, of the despised, the marginalized and the rejected, but nevertheless the holders of the real Truth.

Imagine God's anger on the psychology departments: budgets cut by the NSF and the NIH, journal subscriptions cancelled, nobody comes to their conferences, popular journalists ignore them, their experimental findings are refuted, thousands of graduate students liberated from futile slavery, wandering towards the promised land of PCT. Is that such a bad vision?

Admit that you just don't like Matzot. :-)

Best regards and a happy spring to all

--Oded

Date: Tue Mar 29, 1994 10:49 am PST

Subject: Time, Stress & Sainthood

[From Rick Marken (940329.0930)] Bob Clark (940327.1510 EST)

>In other posts you often use "time" and "temporal variables" as  
>though they are among your own perceptual variables. Not so?

Perceptions that happen over time can be computed by functions that don't explicitly include time. The program that simulates E. coli movement simply increments a variable (like a voltage); the rate of increase in this variable is proportional to perceived chemical gradient at any instant. When the variable reaches a threshold value it "discharges" and there is a random tumble. These events happen in time, but time is not explicitly represented in the process.

I don't rule out the possibility that time, per se, can be perceived. I just don't think it's necessary to perceive time in order to be able to perceive and control variables that occur over time. The reason I think so is because we can model control of simple temporal variables (like rates and accelerations) without including time explicitly in the perceptual computations.

Bill Leach (940328.22:40 EST) --

>I feel that he [Ed in "Freedom From Stress"] has gone a long way toward  
>explaining something in a useful fashion.

No doubt. I was only reviewing "Freedom From Stress" in terms of how well it seemed to incorporate conclusions that might have been derived from PCT.

> the reality is that when we do manage to induce ourselves to  
>perceive someone in a "positive" manner, our own behaviour actually does  
>usually "cause" that other person to begin to themselves behave  
>consistent with the perception that we "forced".

This is true -- and it follows from PCT. But I think it is misleading to describe this as a "change in perception". We see people more positively when we change our wants (reference signals) so that what we are perceiving becomes closer to what we want; the perception of the person remains the same (unless we try to control that other person -- not a good strategy) but our goals relative to that person have changed. When we change our goals like this our actions do change -- we are "nicer" to the person because we are no longer trying to get the person to change (to be like that old goal). The hierarchical PCT model suggests how we might go about changing our goals (wants). It's called "going up a level". I would like to see a "pop psych" book like "Freedom From Stress" explain how this is done -- so that other therapists as well as the people using the book -- could do it themselves. It's a VERY useful technique.

>As Ed quite clearly points out however, when employing his PCT methods a  
>persons perception of another person WILL change and the change may not

>necessarily be "postive".

Remember, perceptions are VARIABLES! There are two ways perceptual variables can "change"; within and across dimensions. Within dimension change is what occurs in ordinary control. When you adjust the temperature of the shower you are changing a perceptual variable (temperature) within its dimension (temperature). Within dimension change, when controlled, is always "positive" because you are always bringing the perception to the value on its dimension that you want. Across dimension change is what happens in reorganization; it's when we perceive the world in a NEW WAY. For example, across dimension perceptual change occurs when I see that a screwdriver can be a wedge.

When I read about "changing perceptions" in "Freedom From Stress" I tend to interpret it as across dimension change. This is because within dimension change is just "control" so it really needs no special name -- and it doesn't sound like Ed is advocating that we change certain perceptions -- like the perception of the warmth of a wife -- by using control (this would involve trying to do things to the wife that would change the perception of her to "warm"). So I think Ed is advocating across dimension change in perception; this can only result from reorganization (which is unpredictable) or from looking at things from a different perspective (a different level in HPCT). I think Ed must mean the latter, in which case (as I suggested in the review) this "change in perception" could be described more clearly (I think).

>In the "real world" I don't think that Ed is being unreasonable to try to  
>encourage people to use techniques that are likely to enhance  
>interpersonal relationships because of their propensity for affecting the  
>perceptions of others.

Nor do I. But there are lots of books out there, each with thousands of adherents who will say that they have been helped by the techniques suggested in the book. Heck, people swear by (or have sworn by) est, scientology, religion, orgone boxes, behavior mod and tons of other nonsense. They all work to some degree because they are all done by people who have reasonable intuitions about what makes people happy.

My criticisms of "Freedom From Stress" (such as they are) are not criticisms of the wisdom of the advice given in that book; they are criticisms (questions really) of how that advice is being related to the PCT model.

Oded Maler (940329)--

>I'll respond to this provocation just in order not to frustrate you  
>for being wrong about my controlled perceptions.

Actually, I was going for the people who would be upset by the idea that the Easter and Passover stories are myths. I thought you knew this stuff was make believe?

>As for the Passover myth, contrary to your alleged introspection, it  
>is the very same ethos that you have internalized so well.

Ah, you do know it's make believe. You just think it's a good myth -- an allegory of the PCTers fighting the conventional psychological establishment. Well, call me crazy, but somehow I see a difference between trying to get people to change their

minds by presenting them with scientific data and getting them to change their minds by presenting them with the death of their first born sons.

>Admit that you just don't like Matzot. :-)

I love Matzot. I think the myth is lousy (in many ways). Let me list them: 1) violent approach to conflict resolution 2) following liberation from slavery Hebrews immediately kill every living Canaanite and set up an empire in which slavery is de rigeur (though very humane, thank you) 3) god plays favorites 4) god sets up pharaoh by "hardening his heart" and then throws shit at innocent people.

The problem with religion is that people can't just say -- "yeah, that's a lousy story. Let's just forget it." This is because people think this stuff is more than a story -- they think it's TRUE. There's not much we can do about this; it just means that people who love people (like poor Salman Rushdie) have to be aware of this idiocy and be a little more careful. C'est la vie.

Just so that you don't think I'm playing favorites, by the way, the Christian mythology has it's flaws too, of course, not the least of which being that it sells itself as being TRUE. The Christian myths also suffer from the same problem as the Hebrew myths -- they play favorites. The big flaw in the Christian mythology is the idea that you can ONLY achieve grace or peace or whatever by "accepting Jesus". This is a very sorry idea in a world in which we know there are billions of people, each trying to control for a different system concept and, pretty soon, all of them will be connected by e-mail. I think it's time to treat these mythologies as what they are -- works of art. Not all art is worth keeping.

In nomino patrie, y figlie y spiritus sanctus (vel, pretty close, nu?)

St. Rick

Date: Tue Mar 29, 1994 5:40 pm PST  
Subject: RICK & CONSCIOUSNESS-RKC

<Bob Clark (940329.1945 EST)>

Rick Marken (940320.1600) Subject: Time and consciousness

I am considering "CONSCIOUSNESS" separately from "TIME" for ease of communication.

In a previous post (940316.1000), you said that Bill:

>... suggested that one role of CONSCIOUSNESS is to DIRECT  
>reorganization to the control systems that NEED it.

And I asked:

>>does this remind you of some of my discussions of the DME?

Your response:

>So the DME is the reorganization system? Is there more to it than  
>that? If so, could you explain HOW the DME directs reorganization?

The DME is NOT the reorganization system. Yes, there is "more to it than that."  
The DME does NOT "direct reorganization."

>Some algorithms and some data would make the DME a LOT more  
>interesting to me.

This is more a matter of observations and definitions of terms than one of algorithms.

#### OBSERVATIONS.

Judging from the nature of your message to me, you have probably considered such matters as: should I respond at all? How should I phrase my response? Should I use questions? or assertions? Shall I be blunt? Friendly? Interested? How about courtesy and politeness?

Did your behavior in this process suggest the random action typical of the original Reorganizing System?

Were you aware of any intrinsic error?

People are observed "making decisions" and reporting and discussing their decisions. What are they perceiving? What events do you perceive when you are "making decisions?"

#### REORGANIZING SYSTEM VS DME.

##### ORIGINS. Reorganizing System.

According to my recollections, our early discussions (before the 1960 papers) led to the initial version of the Reorganizing System concept. Although I have not consulted Bill, I think he would generally agree with the following greatly condensed version of its origins:

Problem. After recognizing the relevance of the basic negative feedback system, a suitable source of reference signals corresponding to the specific feedback signals was needed. A source of such reference signals could consist of recordings of those same feedback signals that could be "played back" later as reference signals. Such a set of memories would provide explanations for many observations. This concept could apply throughout the hierarchy.

Another problem. Given the hierarchical concept for the over-all organization, how could such a learned structure be developed from the original genetically determined systems?

##### DEFINITION, Reorganizing System.

The answer proposed (by Bill, I think) was the Reorganizing System, originally called the "Neg-entropy" or "N" system (see the 1960 Paper or Living Control Systems I). This system was carefully specified, as included in B:CP, to include no more than absolutely necessary to account for observations. Hence its sensitivity to Intrinsic Error and operation by random changes in the existing systems.

This is repeated in B:CP, Chapter 14, Learning, p 187:

+The reorganizing system, however, does not sense behavior or its  
+effects on the environment. It senses only intrinsic quantities,  
+and its reorganizing outputs are based only on the amount of



+intrinsic error that exists. Therefore--and this property is  
+exceedingly important to this theory--the process of  
+reorganization is independent of the kind of  
+behavior being reorganized. It depends only on the effects the  
+behavior has on intrinsic state.

That is essentially the original definition of the Reorganizing System, as presented in the 1960 paper.

ORIGINS. Decision Making Entity.

Later, as noted in B:CP p 224, Bill notes the question of "what flips the memory switch. "One" flips it!"

DEFINITION.

Similar to the origin of the Reorganizing System, The Decision Making Entity, "DME" is proposed as an answer to this problem. In order to make these decisions, it must have certain characteristics, abilities, and meet certain conditions for its operation. I am not at all sure that my suggestions are adequate, but modifications may be useful.

The DME resembles the Reorganizing System in some ways. But it differs in many ways. It must have full access throughout the learned system, including memories, in order to examine, compare and apply recordings. That is, to "make decisions."

MODIFICATIONS of the Reorganizing System.

First, p 199, Bill suggests that the Reorganizing System could operate "selectively." Here he points out that:

+If the Reorganizing System could sample perceptual signals from, and  
+send test stimuli to, any part of the hierarchy, selectively,  
+reorganization could be limited to those systems in active use."

To me, this is a drastic change in the concept of the Reorganization System. It is no longer restricted to sensing Intrinsic Error and random (that is, "unplanned") actions, but has been given full access to the learned hierarchy.

CONSEQUENCES.

Further, p 199, Bill recognizes that this is a change. He observes:  
+This new arrangement, ..., gives the model as a whole two completely  
+different types of perceptions: one which is a representation of the  
+external world, and the other which is a perception of  
+\_perceiving\_. And we have given the system as a whole the ability  
+to produce spontaneous acts apparently unrelated to external events  
+or control considerations: truly arbitrary but still organized  
+acts. ... we are now talking about awareness and volition.

Page 200, after discussion of volition and awareness, I find:

+The mobility of awareness is striking. While one is carrying out  
+a complex behavior like driving a car, ... one's awareness can  
+focus on efforts or sensations or configurations of all sorts, the  
+ones being controlled or ... or musing over some past event or  
+future plan.

CONSCIOUSNESS. Also p 200:

+This leads to a working definition of \_consciousness\_.  
 +Consciousness consists of perception (presence of neural currents  
 +in a perceptual pathway) \_and\_ awareness (reception by the  
 +reorganizing system of duplicates of those signals, which are all  
 +alike wherever they come from).

These modifications of the Reorganizing System give it some of the characteristics needed by the DME. The Reorganizing System could have been extended further to include Decision Making Ability. I don't know why Bill didn't add this capability to the Reorganizing System.

As often results from assigning a label to some item, giving a label to this entity (the "DME"), has led to additional observations and concepts that offer promise for improved communication with other people.

These topics merit further analysis and discussion.

Regards, Bob Clark

Date: Tue Mar 29, 1994 11:25 pm PST  
 Subject: Re: Time, Stress & Sainthood

<[Bill Leach 940329.18:46 EST(EDT)] >[Rick Marken (940329.0930)]

OK, we are dealing with a lack of precision in terms here then I suppose.

Let me try to see if I sort of understand...

Assuming "Bob's" relevant wants and perceptions are:

W1) Happy  
     P1) Must have "nice marriage"  
        W2) Nice marriage  
           P2) Must have "nice wife"  
             W3) Want "nice wife"  
 conflict -> P3) Have "cold fish"  
 conflict -> P4) Have not "nice wife"  
 conflict -> P5) Have not "nice marriage"

A review would reveal that some options exist for resolution:

- O1) Change W1
- O2) Change W2
- O3) Change W3
- O4) REALLY change W3 (as in new wife)
- O5) Induce wife to change to "nice wife"

Seems likely that changing W1 is rather improbable (contrary to the apparent nature of some of the folks that we all run into from time to time).

P1 and P2 are both internally generated perceptions and should be subject to change (though I'll admit that I am not sure that I really understand how this would be done). If the perception P1 could be changed then W2 could be changed and conflicts "go away."

The same relationship should apply to P2 and W3.

For this discussion it is probably also safe to assume that there are several other "wants" that will influence any decision process such as; wanting to keep promises, not wanting "hassle" of divorce, possibly not wanting a "stigma" from divorce or "failing" in marriage.

In addition there is likely relevant perceptions concerning such things as; wife was not always a "cold fish" (wife was at one time perceived as being "nice") and thus it should be possible that she can again "become" a "nice wife", some sense of responsibility for wife being a "cold fish", and likely a sense that divorce is permanent (or at least the worse choice for "conflict resolution").

It is reasonable to decide that O5 might be the best thing to attempt first. To do that then:

Set a W4) Want to perceive current wife as "nice"

Now then the question is (I suppose), how do you "set" a new W4? It seems to me that what is required is to do just what I perceive Ed as having said:

- 1 Decide to perceive her in the fashion that you want.
- 2 Decide to emphasize the pleasant experiences with her.
- 3 Decide not to react to things that normally act as "triggers."
- 4 Perform actions that re-enforce the desired perceptions (lists and quality time).

Now (assuming that any of this is considered acceptable), I admit that items 1 -> 4 are all wants and not perceptions.

If I am thinking correctly in all of this, the original "wants" are not changed but rather new wants are established that should bring into control perceptions that either were not being controlled or were being controlled for the wrong "want". If these perceptions are indeed the proper perceptions and the new "wants" control them to a level that will be perceived by the wife as meeting "wants" of the wife then the original "wants" will be satisfied by the shift in perceptions that will occur.

In my "over-simplistic" approach here, I recognize that to set "new wants", it is likely that at least in some cases existing wants will have to be either changed or dropped.

If you were able to get through this... is this in some way what you were driving at?

-bill

Date: Wed Mar 30, 1994 7:48 am PST  
Subject: Re: To Bill Leach on dynamic modeling

Robert W. Clyde >[Cliff Joslyn 940325]

When the subject of system dynamics finally came up on this list my surfer's head lit up a bit to offer response.

I'm pleased to confirm that any rumors of death (physical or intellectual) are (hopefully) very premature. In fact he's scheduled as one of the leads at the 1994 International System Dynamics Conference July 11-15, University of Stirling, Stirling, Scotland which I'm fixing to attend. Other leads include Peter Senge of the Sloan School (The Fifth Discipline) and reps from High Performance Systems, Inc., producers of Stella II and itthink software, much more friendly successor to Dynamo computer program.

RWC

Robert W. Clyde  
Associate Professor  
Augsburg College, Minneapolis, MN  
612-330-1146

Date: Wed Mar 30, 1994 9:32 am PST  
Subject: Misc

[From Bill Powers 930430.0600 MST]

Either I woke up dumber this morning or several people on the net woke up smarter yesterday. Some really illuminating posts today!

Rick Marken (940329.0930) --

Your comments about time sent a shock through me. Of course! In our simulations, time is used but is not represented. The only way to represent time in a simulation is to set a variable  $t$  to zero, and then on every iteration say  $t = t + 1$ . But all that does is count iterations -- if the frequency of the system clock were varying randomly, the simulation would never know the difference.

You said the other day that time may really be handled as the derivative of one variable with respect to another. Isn't that in fact how we mark the passage of time? We feel our breathing or our pulses, look at the sun, moon, and stars, or more recently, glance at a clock or a calendar. We judge the lapse of time between events by referring to some other set of repetitive events which we take to occur "uniformly" in time. But if you remove all those events, what is there to perceive about time? Nothing. Time "itself" is not a perception. It is a relationship among perceptions. If all our clocks (of all kinds from biochemical on up) were to vary their frequency with respect to "true" time, we would never know it.

We like to write formulas like  $s = at^2/2$ . We should really write  $t = \sqrt{2s/a}$ . Einstein had a glimmer of the same thought, when he realized that "time" could be different in two different inertial frames.

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Your discussion of changing perceptions was also illuminating. I wish I had thought of the "within perception" and "across perception" way of putting it.

Ed Ford is really talking (in one context) about the "within perception" idea, which does not require reorganization, but the change he is advocating relies on accumulating new experiences, not on either forcing another person to behave differently or hallucinating the other person behaving differently. This is what his use of "Quality Time" means. Ed advocates making a plan for spending considerable amounts of time with another person doing things pleasing to both of

you. The result of this is to build up a store of memories in which you can legitimately and without hallucinating perceive the other person as being pleasant and good to be with.

Not being a theoretician, Ed doesn't dwell on why this works. We who like to explain things before we can bring ourselves to do them, however, can see there are two processes going on. First, in order to make a plan for a new way of behaving, the person MUST move up a level. It is necessary to look AT the reference signals that normally operate automatically and determine how you are behaving. The only way to look AT a reference signal is to occupy the next level up -- which also happens to be the level where you must be to CHANGE a reference signal consciously. To make a plan for quality time, two people must move up a level enough to change the normal routine behaviors that are simply playing out the conflicts that are causing the problems. Instead of coming home, flipping on the TV and opening a beer under the mutely resentful glare of your wife (which will soon cease to be mute), you think "Oh, yeah, we made this plan, didn't we, and Ed is going to ask if we actually did it. Uh, honey, uh, should we try that going for a walk thing?"

Going for a walk, if both people actually enjoy it, will start building up memories of pleasant association with the other person; I think Ed is right about that. But to my mind, the most important thing has already happened by the time the walk begins: the people involved have somehow managed to move their awarenesses to the level where they can change the routine reference signals that normally run their lives -- the plans they are already carrying out. You can't flip on the TV and open a beer, and at the same time be going for a walk. Ed makes a big deal about committment to a plan. A plan isn't just a good intention; it's something you promise Ed to do, and promise your wife to do, promise yourself to do. The biggest step is making the committment, because to really make it you have to be working at the level that can actually alter the plan under which you're already working.

So Ed's approach is actually a sneaky way of getting people to move up a level. It isn't the particular new plan that matters; what matters is being in a position from which you can actually change plans. Ed has a way of doing that. There are probably lots of other ways, but Ed's way has one great advantage: he's actually doing it with real people.

The same goes for prioritizing. Ed never tells people what priorities to choose, which goal to place before others in importance. He just persuades them to consider the conflicting goals, and to assign some sort of relative importance to them. Sneaky, again. In order to do that, where must the person be operating from? Obviously, from a level where the various (now lower-level) goals can be observed! And that is also the level from which they can consciously be changed. As long as you're working at the level that is governed by those goals, all you can do is experience the conflict. Only from a higher level can you alter the goals and resolve the conflict.

Again, "prioritizing" is just one way of doing this. What matters is being in a position to consider the goals together. You might actually alter the goal-settings, or replace some goals with others. But the important thing about telling people to prioritize is that it does get people to go up a level, and Ed is actually getting them to do this and not just talking about it.

Of course all this requires reorganization, which is random and unpredictable in its effects. Reorganization is not under conscious control; it simply starts when errors are big. So you have to learn to allow errors to be felt. If you recognize that reorganization is unpredictable, you won't expect it to work the first time. You'll learn to recognize the symptoms of random change, and to stick with it until finally something better turns up, whatever it may be. When you make a new plan, or re-prioritize, you may generate a lot of new conflicts. So you keep changing plans and re-prioritizing, pretty much at random, until the new conflicts go away, as eventually they will. Then you will slobber all over Ed thanking him for the wonderful things you have actually done for yourself. As Ed will tell you.

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Oded Maler (940329) --

Yes, it's possible to perceive PCT as a new religion and call down the wrath of the God of Feedback on its opponents. Every now and then the thought of following in L. Ron Hubbard's footsteps occurs to me -- the Church of PCT would be tax-exempt, for example, and PCT-based psychotherapy would not require a license, because it would be Pastoral Counselling. Furthermore, all the people who criticize PCT and its adherents would have to shut up, because it's not polite to criticize someone's religion. We could do any damned silly or offensive thing we liked, and nobody could oppose us because we could haul them into court for violating our civil rights. And we could make bundles of money by offering courses to ordain Holy Ministers of PCT, who would get their congregations to tithe. Yes, indeed, all kinds of attractions in becoming a religion.

The problem with treating anything as a religion -- even a religion -- is that it's too easy to interpret its teachings as an excuse for doing what you wanted to do anyway. Think of the people who punish deviants, saying "Vengeance is mine!", and leave off "...saith the Lord." The God of the Old Testament was actually saying "Leave that vengeance stuff to me, pal," but those in whose hearts the thirst for vengeance rages manage to find the interpretation that lets them slake it.

>"Spill your heated anger on the gentiles ..."

... is a welcome admonition to those who are inclined to violence anyway, and is ignored by those who aren't. What if you don't have any heated anger toward the gentiles -- are you supposed to whip some up so you can spill it? It seems to me that little incentive is needed for people like Arabs and Israelis to spill their heated anger at each other. They can both cite scriptural justifications for what they are doing for other reasons, and would do anyway.

Behind all the accumulated gimcrackery of the centuries, religion is simply a human enquiry into the basic problems of existence, particularly problems that science has chosen to ignore until quite recently. What is consciousness? Who is this Observer inside me, that I am? What is it to have purpose? How can we live together without pain? How can I make sense of this limited existence of mine and the world in which I have it? What are these longings inside me that draw me onward? Why is there beauty? Why is there sadness? Why is there goodness, why is there evil?

Scientific theoreticians turned their backs on these questions, so to find answers people turned to theological theoreticians. Who can blame them? I hope that PCT will not also turn its back on these questions.

Best to all, Bill P.

Date: Wed Mar 30, 1994 5:16 pm PST  
Subject: Behaviorists and PCT

<[Bill Leach 940330.10:57 EST(EDT)] >Rick Marken

Let me start with the fact that I don't feel that you are "attacking" Ed's work. Far be it for me to make such a determination anyway, you have been interacting with him for a many years and obviously he has Bill's respect and support.

I view your remarks as constructive criticism in the purest attempt to increase the faithfulness of Ed's work to the principles of PCT. However, it is in precisely this light that I make my comments. My "support" of a manner in which Ed discourses on PCT SHOULD be viewed with respect to it's own merits in light of PCT unless it is obvious that the statement was not intended relate to PCT.

I see my own personal interest in PCT as existing in to catagories. The first is just the fascination with the theory and related experiments. The second is with the practical application to personal behaviour and interpersonal relationships.

It is precisely in this second aspect that Ed's work is so useful to me. He is basically using the sorts of "goals" that individuals and counselors would normally use but is trying to present the activities required in a fashion consistant with PCT. I think that because this level of dealing with PCT is at so high a level of abstraction and also because this is the level that we use so "unconsciously", it is difficult to remain consistant in terms.

I think that a part of the problem in thinking about some of the ways of dealing with others using PCT to improve interpersonal relationships is that we often take situations to extremes for example purposes and of course ignore a great deal of real "baggage" that exists in any real situation.

We control perceptions of course. We have many "wants" that exist at numerous levels of priority and with rather complex interrelationships.

It would appear both from the theory and from experience that our highest level "wants" are both built-in (intrinsic) and indescribable. We attach "labels" to at least some of these wants such as "be happy" but they are weak assignments at best. It is likely that these wants are unchangable (at least volitionally).

It would seem that there are control systems that are related to these intrinsic wants established by reorganization as well as "hard wired" (like make the heart pump blood to maintain x blood pressure). It seems likely that for any given controlled perception related to an intrinsic want that there could be in existance several control models. In addition for any given intrinsic isn't it possible that there are several controlled perceptions that "make up" the net intrinsic error signal? Thus, it would be possible to have some level of intrinsic error that results in programatic control changes rather than reorganization and reorganization then would not occur unless intrinsic error increased (or possibly just persisted)?

Our high level but not intrinsic wants must be generated from the output of control systems that are controllng for intrinsic wants (setting the reference

signals). But what are the references that are set for things like "integrity" or "how to relate to wife"?

I agree that something like "how to relate to someone" is set by current intrinsic error condition, plus "world model" perception of the person, plus current perception of the same person. Indeed, the current perception is based upon current sensory input, recent stored perception and world model perception.

Ed's approach (and indeed that of many others) is that you can change the world model perception of a person and that a change there will result in a change in realtime perception of that same person.

As you mentioned before, we do have perceptions that we are not controlling. Much of what we see and hear are "stored" non-critically. However, controlled or not, and critically analyzed or not, these stored perceptions can and do affect current perceptions.

Now to try to "improve" an interpersonal relationship it is fine to talk about "changing wants", "goals", "priorities" and the like and it is quite another to have the decisions influence our "automatic" behaviour. By choosing to perceive another person in a fashion more to our liking (more consistant with our wants) we can cause the world model for that person to change.

In part this can be done by imagination, that is both remembering past perceptions with this person that were of low error and imagining future interactions that are low in error.

"Making a list" of perceptions that are pleasant with respect to the other person aids in this because the act of thinking about the "good" things that another does places higher priority on these occurrences than the less pleasant ones.

Additionally, adding a new task that creates real perceptions that are low in error will help and again it is likely that the intensity of focus for this new task will increase its' value in later comparison operations in the brain.

I am perfectly willing to hear about how else one might go about changing wants. It occurs to me that those of us that tend to "hang around" here are generally "logical sorts" of people. I know that I personally have a very high regard for "scientific method" and logical discourse. I can and apparently have in the past changed my behaviour exclusively through the use of logical analysis. However, I know through experience that even with a very high regard for logic, it is not enough for all behavioural matters.

While it appear to me that PCT has provided an explanation for the operation of the mind (among other things) that is logical it is clear from the theory and experience that people are not logical in operation (when viewed from the outside or even considered from the inside).

We all have the ability to "rationalize" beyond our wildest dreams (no matter how "logically" we think) and re-organization, it would seem, has the ability to cause just about any "behavioural anomaly". OTOH, it seems that most of us are also able to "limit" the changes that re-organization can wrought (at least in areas of overt behaviour).

-bill



Date: Wed Mar 30, 1994 5:16 pm PST  
Subject: New support for reorganization?

<[Bill Leach 940330.15:10 EST(EDT)] >NET

No sure how many have read about this (and I did not myself) but was told about it a couple of days ago...

It appears that some researchers have discovered a "new" brain cell that has an interesting behaviour. When "triggered", this new cell releases a gas that affects neurons in its immediate vicinity. I seem to remember that the gas was something rather a bit surprising... like maybe hydrogen-sulphide.

Sounds a great deal like this is physical evidence for a re-organizing system to me!

-bill

Date: Wed Mar 30, 1994 5:25 pm PST  
Subject: PCT Therapy

[From Rick Marken (940330.1540)] Bill Leach (940329.18:46 EST)

>Assuming "Bob's" relevant wants and perceptions are:

>W1) Happy  
>P1) Must have "nice marriage"

Again, perceptions are VARIABLES. Wants specify reference states of those variables. So a perception and the corresponding want must be on the same perceptual dimension. In your example, the perceptual variable might be called "quality of marriage" which ranges from "wonderful" to "don't ask". The want would be for a state of that variable that is somewhere in that range -- perhaps at Garrison Keillor's "pretty darn good".

The perceptions that you label "conflicts", by the way, are not conflicts; they are simply perceptions that are not in their wanted states. The reason why they are not in their wanted states could be due to conflict or it could be due to the fact that the person has not yet learned to control them.

>Seems likely that changing W1 is rather improbable (contrary to the >apparent nature of some of the folks that we all run into from time to time).

Want cannot simply be changed arbitrarily; we want what we want in order to produce perceptions that satisfy higher order wants. Nevertheless, changing wants is usually what is needed to solve conflicts. That's what therapy is about (from a PCT perspective) -- helping people reorganize their control systems so that they can set their wants more effectively. It's a lot easier to say that this is the therapeutic goal of PCT than it is to actually carry out that goal. I am not explaining HOW to solve one's problems; just what WHAT is involved (from a PCT perspective) in solving them.

>P1 and P2 are both internally generated perceptions and should be subject  
>to change

All perceptions are internally generated inasmuch as they are the result of computations carried out by perceptual neural networks (what are called "perceptual functions" in PCT). I don't believe that one changes these functions very often; what might be more common is that we develop NEW perceptual functions which compute new perceptions which take existing perceptions as their input. This is what Bill Powers (930430.0600 MST) suggested is what is going on with "quality time"; sounds reasonable to me.

>Now then the question is (I suppose), how do you "set" a new W4?

If you have to "decide" what to want you are probably in conflict; so the decision to want "this" rather than "that" state of a perception is made by the reorganizing system (in the PCT model) - - ie. randomly, by flipping a mental coin (possibly accompanied with a lot of imaginary justifications for why that was the "right" choice). If you are not in conflict, wants are set automatically so as to produce the intended perceptions for the systems that are setting the lower order wants: the "Marken spreadsheet" demo shows how this is done in a complex hierarchy of wants and perceptions, in near real time.

Best Rick

Date: Wed Mar 30, 1994 5:52 pm PST  
Subject: Article addition

[From Dag Forssell (940330 1630)]

My editor for Engineering Management Journal called back. Besides suggesting cuts of 2 1/2 pages from the beginning of my latest article, he suggested an addition. His question was:

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HOW IS THIS DIFFERENT?

Conventional psychology teaches us that the only thing we can legitimately study and deal with is peoples' behavior. When we are unhappy with the results of the performance of another, we ask: What are you doing? Why did you do that? Can't you do something better? We tell people: You can't do that; your behavior is unacceptable! Here is what I would do if I were you! This is the accepted method in that situation. If you say this..., the customer will do that... We focus on, reinforce, reward, train and modify behavior.

It does not make matters easier that the term behavior itself is poorly defined and confusing. Behavior refers to action, but is invariably defined by the result: harassing behavior, loving behavior, cooperative behavior. The questions above often lead to defensive excuses, conflict and resentment. Accidentally, they may also lead to a productive discussion of wants.

PCT psychology shows clearly that action is a by-product of wants, perceptions and circumstances. When we are unhappy with the results of the performance of another, it is best to ignore the action/behavior--the by-product or symptom--and ask instead about the wants and perceptions, which are the causes.

When you change your focus from behavior to wants and perceptions you compel your associate to think, to sort out internal conflict, and allow your associate to control well: to satisfy personal and company wants at the same time.

Old habits die hard. This change in focus will feel awkward for a time, but the payoff will be great.

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This half column is to be inserted after the segment: Mapping wants and perceptions, and before Summary.

I am hereby soliciting comments, corrections, suggestions ---

Best, Dag

Date: Wed Mar 30, 1994 7:21 pm PST  
Subject: Re: To Bill Leach on dynamic modeling

> Thanks for the excellent response message.

You're welcome. But flattering me can be dangerous. I already think too highly of myself. ;->

> Maybe it is I that is overly simplistic but just what is wrong with  
> Bill's standards for ANY 'real world' model?

I'm not sure I have any problem w/Bill's "standards". All I meant was that there are some things in the world that are not control systems. To study these things you need to use something other than the Power's negative feedback control system model.

> I don't see his view as being a problem with using other modeling  
> techniques as long as one recognizes that when models disagree with  
> reality, you can claim neither "reality being wrong" (and yes there are  
> those that do), nor that the model is necessarily "telling" you anything  
> useful short of "you don't know what is going on."

Yes, IF you have COMPLETE confidence in your measurement methods. That's one of the hallmarks of PCT and constructivism in general: you can only control what you can perceive. The rest is not unique to Bill, it's just Epistemology 101.

> I don't have a problem with statistical models BTW, but INSIST that one  
> recognize that such models potentially suffer from serious deficiencies  
> when it comes to prediction.

Statistical models produce statistical predictions. 'Nuff said.

Cliff Joslyn

Date: Wed Mar 30, 1994 7:56 pm PST  
Subject: Re: Misc

<[Bill Leach 940330.19:00 EST(EDT)] >[Bill Powers 930430.0600 MST]

Wow! Yet again! What a presentation! I hope Ed is following this. He should see some really "good stuff" in all of that last post.

-bill

Date: Wed Mar 30, 1994 8:03 pm PST  
Subject: Re: To Bill Leach on dynamic modeling

<[Bill Leach 940330.18:38 EST(EDT)] >Author : rclyde@AUGSBURG.EDU

I don't believe that Cliff was wishing for his demise and I don't know enough to decide my position but thanks for letting us know that he is still around. Does that mean that some of us should worry? ;-)

-bill

Date: Wed Mar 30, 1994 9:59 pm PST  
Subject: A new book

FROM CHUCK TUCKER 940330

One of the few friends and colleagues that I have in the socy department here at USC-Columbia recently published a book that he intends for introductory courses. The charm of his writing is that it is "down to earth" (much like Ed Ford's) w/o a bunch of jargon. This book is arranged around five "puzzles": 1) what is so puzzling about social life? 2) how do we know? 3) how does our world exist? 4) how can we get people to do what we want? and 5) in what shape is our social world and how did it get there? As you can see very common questions. I just got the book yesterday and started reading it and noticed a statement in the acknowledgements that might interest some of you (I was a bit shocked by it since I did not know he would write it):

Finally, Chuck Tucker and Bob Stewart have been my colleagues for more that 15 years. We have encouraged one another in our teaching and research on social life. I know that I would not have been able to write this book without our many discussions. In the past several years, Chuck has enlightened me on the work that he and others in the Control Systems Group, a scholarly association, are doing to explore the seemingly simple but profoundly significant idea that people direct themselves. I have drawn upon his insights and some of the work he has brought to my attention primarily in Chapter Four. (iv)

The book is: Paul Higgins. 1994. SOCIOLOGICAL WONDERMENT: THE PUZZLES OF SOCIAL LIFE. Los Angeles: Roxbury Publishing Company. (213) 653-1068 ISBN 0-935732-55-1 HM51.H54

Regards, Chuck

Date: Wed Mar 30, 1994 10:27 pm PST  
Subject: Re: To Bill Leach on dynamic modeling

Nothing wrong with thinking highly of yourself as long as you think highly of the general model too :-)

>To study these things you need to use something other than the Power's  
>negative feedback control system model.

Ah Ha! Reference problem here. I was not referring to the negative feedback model but rather the basic statement with regard to modeling in general.

>Statistical models produce statistical predictions. 'Nuff said.

Actually for me, yes. However, it is my opinion that herein is the root of my concerns. "Real" scientists are are very much aware of the limitations of their modeling techniques or at least the nature of such potential limitations and DO NOT pretend otherwise.

Statistical models have always bothered me, in physics as well have behavioural "sciences". It isn't that they do not or can not produce useful information but rather than too many people using the information impart too much meaning to it. That is, they go quite far beyond what the modeler would likely tell them are limiting conditions for validity. Additionally, the modeler should often be able to explain where causal connections should not or even can not be drawn from the model's results.

>can only control what you can perceive. The rest is not unique to  
>Bill, it's just Epistemology 101.

Yes, it is but I have never seen anyone put it quite so succinctly.

cheers! -bill

Date: Wed Mar 30, 1994 10:27 pm PST  
Subject: Re: Article addition

<[Bill Leach 940330.23:58 EST(EDT)] >[Dag Forssell (940330 1630)]

In light of recent discussions that is a particularly appropriate posting. Looks very good to me but then I'm easy :-)

-bill

Date: Wed Mar 30, 1994 10:36 pm PST  
Subject: Re: PCT Therapy

<[Bill Leach 940330.22:50 EST(EDT)] >[Rick Marken (940330.1540)]

Rick;

I am not doubt gonna hafta wrestle with this one a bit and appreciate your efforts at clearing the air. Trouble is, every explanation seems to raise even more questions than it answers... so what's new? :-)

I can see that terminology and reference points are a killer in a discussion such as this one.

As I am viewing it "must have a 'nice marriage'" is indeed a perception and not just A perception but several groups of them. It is also a want and a reference somewhere.

It is a perception in that the subject concludes via cognitive processing that "must have a 'nice marriage'". That is, some control system (set?) in the brain came up with this idea and the idea is a peception.

Of course the idea is a single perception but it is made up of many references that were generated in the same way (and therefore were themselves perceptions).

The brain may well create the link between this perception (happy marriage) and the higher, and as far as I am concerned, intrinsic want (be happy) and create or connect the control system that uses this perception as a reference. Poof! The magic has occurred and this former perception has somehow been turned into a reference level for some quantity of "nice marriage."

I think that "Introduction to Modern Pyschology" may have dealt a little with a major source of the real problem here. The perception "nice marriage" that "Bob" or anyone else discusses or even thinks about may come from control systems that might not have anything at all to do with controlling perception actually created as a result of interactions that take place with the environment.

When we talk from a counselor's or therapist's view EVERYTHING is perceptions. When the subject thinks about or talks about wants he isn't really talking about wants but rather his current perceptions concerning his wants.

This sort of thinking probably is one level of iteration too deep but I think that I had to "dip down there" once for my own sanity. :-)

Seriously though, this is undoubted a very real problem in dealing with ourselves as well as other people. We think that we know what we want but that is rarely true except in either very general terms or some isolated very specific terms (and those are probably questionable).

I find myself becoming more an more fascinated with the concepts of re-organization. I can't help but wonder an about a number of possibilities:

1. Might there be numerous degrees of re-organization? That is both the frequency and EXTENT limited by the magnitude of the intrinsic error signal?
2. Is it possible or even probable that re-organization is occurring at some degree continously?
3. Is this possibly the reason for psychosis?

Related, I think, is the idea that people can change some wants by cognitive effort. It seems that just "pondering" a situation does sometimes result in "new" insights or perceptions. I believe that at times these new perceptions (including things like consciously reordering priorities based upon perception of the nature

of existing conflicts) actually do result in a reduction in stress and a change in wants.

While I sure that I am "playing with ideas" that most of you have thought deeply about for years but I hope that I may be getting a little closer.

The "focus of attention" idea that Bill mentioned in a recent posting really triggered a "feeling of correctness" in me. I believe that several behaviouralists have made the statement that: Our ancestors faced a sabre toothed tiger a week and we experience the same effects at least four times before we finish breakfast.

The idea being that while we operate quite successfully "on automatic" most of the time, we need to be taught how and why we MUST do "mental housecleaning" regularly in the highly complex society that we live in if we don't want to be "re-organized out of existence."

The "why" is because when control fails, re-organization will "kick-in" and force changes to reduce error. Since it is at least pseudo-random if not absolutely random it might not only not "fix" the problem but may "break" other working control systems.

The "how" is probably a whole lot tougher but hopefully PCT itself gives one a chance to see what parts of "successful techniques" gleaned from the wisdom of experience (personal and historical both) are actually consistent with really correcting errors. This would allow throwing out the "medicine man's" worthless incantation while keeping the herb that actually works (or vice versa if it turns out it IS the incantation that does the work).

Keep it up... for myself, I think that in each round I gain just a little more understanding of what it actually is about methods that work that make the method work (and probably therefore when, where and why it might "fail").

-bill

P.S. Is this spreadsheet a "lotus" style spreadsheet? And if so, how do one acquire same?

Date: Thu Mar 31, 1994 5:22 am PST  
Subject: Re: A new book

<[Bill Leach 940331.07:59 EST(EDT)] >CHUCK TUCKER 940330

Sounds good to me Chuck. It is probable that such interaction as you have had has influence the rest of the book also.

Of course while the idea "...that people direct themselves" is "simple" and profound it is NOT the essence of PCT. The amazingly simple yet even more profound idea is what it is that people direct -- perception.

-bill

Date: Thu Mar 31, 1994 7:08 am PST

Subject: Re: To Bill Leach on dynamic modeling

><[Bill Leach 940330.18:38 EST(EDT)] >>Author : rclyde@AUGSBURG.EDU

>

>I don't believe that Cliff was wishing for his demise and I don't know  
>enough to decide my position but thanks for letting us know that he is  
>still around. Does that mean that some of us should worry? ;-)

>

>-bill

I suppose only if one might feel threatened!

RWC

Robert W. Clyde

Associate Professor

Augsburg College, Minneapolis, MN

612-330-1146

Date: Thu Mar 31, 1994 8:40 am PST

Subject: MORE ABOUT TIME - RKC

<Bob Clark (940331.1040 EST)>

Rick Marken (940329.0930) Subject: Time, Stress and Sainthood

>Perceptions that happen over time can be computed by functions that  
>don't explicitly include time.

Of course. But how do you explain the implicit inclusion of time to someone who is unable to perceive time directly? We are familiar with many devices and events where time is treated as an independent variable. Commonly time is included implicitly in the mathematical representation, but an explanation to someone unfamiliar with these situations needs some kind of separate discussion of time.

In physics, average speed is defined as the distance travelled along a path divided by the elapsed time. Average velocity is defined as the distance between the terminal points divided by the elapsed time.

Correspondingly, instantaneous speed is defined as the limit approached by average speed as the elapsed time is reduced to a minimum. Although we have instruments that display instantaneous speed (the car's speedometer), explaining its significance and usage require explicit discussion of time.

>I don't rule out the possibility that time, per se, can be  
>perceived. I just don't think it's necessary to perceive time in  
>order to be able to perceive and control variables that occur over  
>time. The reason I think so is because we can model control of  
>simple temporal variables (like rates and accelerations) without  
>including time explicitly in the perceptual computations.

Yes, such modelling is possible. But the person constructing, or explaining, his model, must use the concept of time explicitly in designing or explaining his device.



When it comes to questions of perceptions, it is a matter of personal subjective experience. While control implies perception, perception does not imply control. We build and use clocks. Surely they would be meaningless if we were unable to perceive time.

How do you tell the differences among past, present and future without perception of time?

How do you explain vision to one who has never seen?

Regards, Bob Clark

Date: Thu Mar 31, 1994 9:33 am PST  
Subject: Thinking highly; perceptions, goals, reorganization. misc

[From Bill Powers (940331.0800 MST)] Cliff Joslyn (940329)

Bill Leach said:  
> Thanks for the excellent response message.  
To which you replied:  
>You're welcome. But flattering me can be dangerous. I already  
>think too highly of myself.

Apply PCT to "I already think too highly of myself." If this is not just a way of trying to make other people's expectations less burdensome, it implies an error signal that is not being corrected. To correct it, you would have to perceive yourself as being less than you now perceive yourself to be, in whatever dimensions X you're thinking of. This, presumably, would mean being less than you now perceive yourself to be in those dimensions. Of course if one subsystem wishes you to be less X than you perceive yourself to be, that implies that another subsystem wishes you to be more X than you perceive yourself to be. Thus the chronic error (which you have now mentioned at least three times as I remember) implies a conflict. You are less X than one part of you would like to be, but more X than another part thinks you should be. The second error gets mentioned, but not the first. Interesting, no?

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RE: not all systems are control systems. You are quite correct. Social interactions among control systems constitute systems, for example, but not control systems. There is nothing in nature that constrains general systems to have the form of a control system.

-----  
Bill Leach (940330.2250) --

>As I am viewing it "must have a 'nice marriage'" is indeed a  
>perception and not just A perception but several groups of them.

"Must" anything is a reference level, not a perception. The perceived degree of niceness (from zero to max) of a marriage is a perception, but the desired degree of niceness is a reference level. Don't just assume that a person desires maximum niceness (all you have to do is imagine maximum possible niceness, and you'll see that on ANY dimension, maximum is too much). You can satisfy the reference level either by creating a marriage that is sufficiently nice in the terms you perceive as niceness, or by ignoring the actual data from the outside world and imagining something more to your liking. So you sit complacently tying

flies or whatever listening to the Little Woman bustling happily about in the kitchen or laundry, perceiving a happy marriage while she is hurling dishes into the dishwasher and thinking vile thoughts about that fat slob lolling at his ease in his hobby room.

-----  
>When the subject thinks about or talks about wants he isn't  
>really talking about wants but rather his current perceptions  
>concerning his wants.

That's what the imagination mode is for: the higher system sees the reference signal it is sending to the lower as if it had been achieved in perception.

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>I find myself becoming more and more fascinated with the  
>concepts of re-organization. I can't help but wonder about  
>a number of possibilities:

- > 1. Might there be numerous degrees of re-organization?  
That is both the frequency and EXTENT limited by the  
magnitude of the intrinsic error signal?
- > 2. Is it possible or even probable that re-organization is  
occurring at some degree continuously?
- > 3. Is this possibly the reason for psychosis?

(1). Actually, in the reorganization models I've experimented with, it's necessary to make the magnitude of the reorganizing effects depend on the amount of intrinsic error, as well as making the spacing of reorganizing events proportional to intrinsic error. The reason is that if you try to use just one fixed amount of change on every reorganization cycle, you have conflicting requirements. To get to zero error in a reasonable time from a state of large error, you want the individual changes to be large, but that creates too much random variation when you get near zero error. If you make the steps small in order to make the final solution stay near zero error, then it takes forever to get to it from an initially large error. Also, local minima have the greatest effects for small steps and the least for large steps. All in all, making the step size depend on the magnitude of error seems like a good idea.

(2). I think so. I've conjectured that reorganization is focussed in systems that are in the conscious state. If there is a minimum rate of reorganization, then we'd expect to see that any system in the conscious state shows signs of reorganization, even if it doesn't need it. In all my reorganization models, there is always some level of reorganization going on.

(3). I've thought that some kinds of psychosis (mania, for example) look like a reorganizing system that's stuck at maximum rate. According to an informant who suffered from mania, the main problem is being unable to shut off the continuous flow of new perceptions and goals long enough to actually try out any of them.

>Related, I think, is the idea that people can change some wants  
>by cognitive effort. It seems that just "pondering" a  
>situation does sometimes result in "new" insights or  
>perceptions.

You have to distinguish between will-power and insight. Insight is a genuine reorganization of perception, when suddenly you see the world in a new way. Will power, on the other hand, doesn't actually change a want: all it does is establish a new want opposed to the old one. This creates conflict, not insight. Of course the result is big errors (unless you start avoiding the situations that call up the conflict), and eventually there might be an insight that removes the conflict.

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Dag Forssell (940330.1630)--

RE: HOW IS THIS DIFFERENT?

Your editor was right: this is something that needs to be said. You said it very nicely.

Best to all, Bill P.

Date: Thu Mar 31, 1994 9:54 am PST  
Subject: MEMORY & IMAGERY - RKC

<Bob Clark (940331.1155 EST)> Leach (940323.07:41 EST) Powers (940325.0600)

The reports of your personal experiences with memory, visualization, and "total recall" were quite interesting. They demonstrate that "memory" is not a single entity. Recordings clearly can occur with respect to any perceptual modality, and at any perceptual level. And, likewise, the related recalls -- "replays" -- can be imagined.

Yet people seem to be quite varied in their ability to record and playback their experiences. I think many of the differences between people is an accidental result of specific incidents.

Unfortunately, the psychologists who have studied this subject seem to have assumed that "memory" is a single entity and that statistical studies of numbers of individuals will reveal the capabilities of the population. Rather, they should have studied the reasons for the differences between the individuals. A few seem to have done this.

My experience (late 1940's in New York) with the Bruno Furst Course in Memory and Concentration demonstrated several important characteristics of these abilities and the possibility of their enhancement. This course was fairly short on theory. Instead, methods and procedures were emphasized. The resulting skills of the students were very impressive.

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I offer a few of my conclusions about memory based on this course and later experiences:

You cannot remember that which has not been perceived.

"Paying attention" to the item to be recorded is essential.

Attention can be controlled -- but also can be distracted.

It is possible to learn to pay attention to perceptions that have previously been ignored.

Imaginary perceptions can be combined intentionally. This can be termed "association."

If two items are to be associated intentionally, there should be a one to one correspondence.

Dramatic, active, ridiculous combinations tend to be more reliable than logical or familiar combinations.

Recordings can have a "time label." An example of this is the student who crams for the exam -- and promptly "forgets" the material afterward.

These are only a few samples.

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There is a good deal more than this, but see if your library has a copy of the book used in the course: STOP FORGETTING, Bruno Furst. It is out of print, and I seem to have mislaid my copy.

Regards, Bob Clark

Date: Thu Mar 31, 1994 2:04 pm PST  
Subject: Article, BIOME, CSGintro

[From Dag Forssell (940332 1300)]

Last night I posted:

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HOW IS THIS DIFFERENT?

Conventional psychology teaches us that the only thing we can legitimately study and deal with is peoples' behavior. When we are unhappy with the results of the performance of another, we ask: What are you doing? Why did you do that? Can't you do something better? We tell people: You can't do that; your behavior is unacceptable! Here is what I would do if I were you! This is the accepted method in that situation. If you say this..., the customer will do that... We focus on, reinforce, reward, train and modify behavior.

It does not make matters easier that the term behavior itself is poorly defined and confusing. Behavior refers to action, but is invariably defined by the result: harassing behavior, loving behavior, cooperative behavior. The questions above often lead to defensive excuses, conflict and resentment. Accidentally, they may also lead to a productive discussion of wants.

PCT psychology shows clearly that action is a by-product of wants, perceptions and circumstances. When we are unhappy with the results of the performance of another, it is best to ignore the action/behavior--the by-product or symptom--and ask instead about the wants and perceptions, which are the causes.

When you change your focus from behavior to wants and perceptions you compel your associate to think, to sort out internal conflict, and allow your associate to control well: to satisfy personal and company wants at the same time.

Old habits die hard. This change in focus will feel awkward for a time, but the payoff will be great.

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I appreciate approving comments from Bill L., and Bill P., but even more that Ed composed and faxed me this counter-proposal. I want to share it with the net, so here goes:

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Dag, I just wrote this off the top of my head without much editing. It just seems to me that what you wrote really didn't address the concerns of the manager. I ended this with a very practical suggestion, one the manager could relate to. I tried to tie this whole thing to the idea that we don't control our people, so how can we deal with them to get them to become more effective..... Ed

How Is PCT different From Traditional Approaches in Psychology?

Traditional Psychology treats people as something to be manipulated. What can we do to get our people to be the way we want them to be? How can we motivate them? How can we get them to come on time, work harder, show more loyalty to the company, in short, to control their actions, their behaviors, or what they do? Strategies are continually being thought of to try and control employees to various ends.

PCT believes that, in the long run, this is impossible. We are designed to be self controlling, to set our own goals, create our own perceptions, to devise our own plans, to set our own limits, to establish our own values, to make our own choices based on our values and standards. And the last thing we want or will allow is for someone to control what we do.

What companies offer is a place for employees to satisfy many of their goals. To express themselves and find value through their work, to satisfy their economical need, and certainly, to some extent, to create a social outlet where they can interact and express themselves with others.

However, for employees to become a functioning member of an organization, they have to accept the general goals of the company, be willing to work with the established standards, and make choices consistent with the company's short and long term needs.

But rather than attempt to manipulate them, as the traditional psychology suggests, this approach stresses the need to teach responsible thinking, through various techniques. It recognizes that employees have to be taught how to work, how to manage, how to interact with others in the most efficient way by asking questions in such a way so as they begin to focus and think through what their goals are, the standards by which they achieve those goals, whether what they are doing is the most efficient way to get things done, whether through an evaluative process they judge their output or perception of a project as being in line with others, but always having them look at what they want, how they perceive things, and whether their actions are consistently getting them what they want.

This approach is one of asking, of developing responsible thinking, of simulating creative thought through the questions rather than manipulation. It is the continual respect for the employees' internal world and their own goals that is so critical. This approach recognizes employees are not controlled individually or as a group, and that they don't act as a group but as individual control systems within a group, all working, hopefully, toward the same purpose, the economical survival of a company.

It has to do with how managers perceive and then work with employees to help them take responsibility for their own potential. For example, in performance appraisals, rather than have a manager make a judgment about the internal working of another human being, I suggest each employee write their own performance appraisal. It be made up of two parts: the first part they should write what they have recently accomplished and give two specific examples to support their claim. The second part they should select an part of their job performance in which they would like to improve. They would then set a measurable goal, set forth a specific action plan, and a chart which would monitor their daily advance toward the completion of their goal. This they would discuss with their manager on a regular basis. This is quite different from a manager controlling an employee, but rather, based on PCT, an employs learning to control their own world more effectively with the manager seen as a teacher rather than a controlling agent.

-----  
So far Ed's counterproposal. Ed gives a much more human flavor than my first draft did. Here is an attempt to merge the two, maintaining my engineering bent and supporting it with Ed's humane bent. Ed's last paragraph gets into the subject of my next article, so I'll ignore it for now.

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HOW IS THIS DIFFERENT?

Conventional psychology teaches us that the only thing we can legitimately study and deal with is peoples' behavior. When we are unhappy with the results of the performance of another, we ask: What are you doing? Why did you do that? Can't you do something better? We tell people: You can't do that; your behavior is unacceptable! Here is what I would do if I were you... This is the accepted method in that situation. If you say this..., the customer will do that... We focus on, reinforce, reward, train and modify behavior.

The questions above often lead to defensive excuses, conflict and resentment. Accidentally, they may lead to a productive discussion of wants. It does not make matters easier that the term behavior itself is poorly defined and confusing. Behavior refers to action, but is invariably defined by the result: harassing behavior, loving behavior, cooperative behavior, leadership behavior...

It is widely said that conventional psychology is concerned with the prediction and control of behavior. This encourages managers to think of people as something to be manipulated. What can we do to get our people to be the way we want them to be? How can we motivate them? How can we get them to come on time, work harder, show more loyalty to the company, pay more attention? In short: How can we control their behavior?

PCT explains how we develop our own understanding, make our own choices based on our values and standards, and act freely--control our own perceptions. The last thing we want or will allow is for someone else to try to control our behavior.

We associate in organizations to satisfy many of our goals, express ourselves and find value through our work, satisfy our economic needs, and interact with others. We have to accept the general goals of our organization, be willing to work with agreed standards, and make choices consistent with the organization's short and long term needs.

PCT psychology shows clearly that action is a by-product of wants, perceptions and circumstances. When we are unhappy with the results of the performance of another, it is best to ignore the action/behavior--the by-product or symptom--and ask instead about the wants and perceptions, which are the causes.

A leadership approach based on PCT stresses the need to teach associates to control well through effective, responsible thinking. It recognizes that associates have to be taught to how to work, how to manage, how to interact with others and so forth by learning to think about their own wants and perceptions, not by memorizing action patterns. This approach stimulates creative thought through questions rather than manipulative coercion. Respect for the associates' internal world of wants and perceptions is critical. This approach recognizes that associates are not controlled individually or as a group, and that they don't act as a group but as individual living control systems within a group, all (hopefully) working toward the same purpose--the prosperity of their organization.

When you change your focus from behavior to wants and perceptions you compel your associate to think, to sort out internal conflict, and allow your associate to control well: to satisfy personal and company wants at the same time. You are seen as a teacher rather than as a controlling agent.

Old habits die hard. This change in focus may feel awkward for a time, but the payoff will be great.

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This last version is exactly twice as long. Is it twice as good? Is it easier to identify with (in terms of the old abuse)? Is it clear in terms of PCT?

Again, reactions and comments eagerly solicited.

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Bill Silvert,  
I took a look at your directories and think you have done a very good job of cleaning up. Noted that there is no directory for ARM1, breaking the pattern. Perhaps you want to add one.

Future readers of CSGintro thank you!

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Gary,  
I suggest you use the abbreviated intro to BIOME server I suggested in one of my posts to Bill Silvert. I trust you kept it.

Best to all, and thanks for comments on "How is this different"

Dag

Date: Thu Mar 31, 1994 3:08 pm PST

[From Bill Powers (940331.1430 MST)] Bob Clark (940331.1040)

>In physics, average speed is defined as the distance travelled  
>along a path divided by the elapsed time. Average velocity is  
>defined as the distance between the terminal points divided by  
>the elapsed time.

That's true, but how is elapsed time defined in physics? It's measured by other physical variables - position of a pendulum, firing of a relaxation oscillator, etc.. Time itself is not measurable. It's a word we use to refer to the fact that the world changes state. Those changes of state define time, not vice versa.

>But the person constructing, or explaining, his model, must use  
>the concept of time explicitly in designing or explaining his device.

In designing it, no; in explaining it, maybe. When we refer to a measure of time, we are referring to states of physical variables. The point Rick is raising is much subtler than the one you are addressing. You simply take the existence of time for granted because we symbolize it in equations. But there is no perception corresponding to time -- perceptions correspond only to things taking place. From the way things take place, we construct an abstraction we call time.

In a computer simulation, we can represent time as an explicit variable,  $t$ . The passage of time is indicated by incrementing  $t$  every time the program iterates. The only indication of the passage of time is in the changes in  $t$ . If the computer's clock speed is doubled or tripled (as by the speed button on the front of many computers),  $t$  will simply increment at double or triple the former rate. Nothing in the program will change; no time relationships will be computed differently. As far as the simulation is concerned, nothing had changed. The speedup of the "real" time is invisible.

In equations,  $t$  is simply a parameter. It does not refer to any observable thing. It could be eliminated by writing an equation for the behavior of a clock hand, and solving it for  $t$  as a function of the clock hand's angle. Substituting that value of clock hand angle for  $t$  would then express the behavior of one physical system in terms of the behavior of another one, and time would no longer appear in the equations. The concept of time is a convenience, not a necessity.

I find this idea fascinating, although I don't know what to make of it yet. I get a faint notion of what could be meant by saying that some cultures have no concept of time. We live in an eternal present; only the behavior of our perceptions, of one perception relative to another, could correspond to the abstraction we call time. We don't leave for work when a specific "time" has arrived. We leave when, in present time, we see a clock indicating a particular number. Or when the sun comes up, or when the sky gets light enough.

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>How do you tell the differences among past, present and future  
>without perception of time?

The present is easy: it is what we experience. The past is what we are now remembering; the future is what we are now imagining.



Best to all, Bill P.