A psychologist joins CSGnet.

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

From time to time a new participant appears on CSG-L. Each time a variety of basic questions are asked and answered. This thread shows such questions, answers and challenges in an unusually thorough and spirited debate between Jeff Vancouver and CSG regulars. You will find numerous comments on PCT and research and at the same time get a feel for the flavor of discourse on CSGnet.

The thread incorporates several thought provoking posts, such as PCT AND ITS CRITICS [Bill Powers (940922.1035 MDT)].

I think this thread reflects CSGnet well. All participants expose ideas and learn from the exchanges. This discussion took place in the past. I expect that ideas and opinions expressed here have continued to evolve. I know mine have.

I have done my best to include a complete sequence of questions and answers in this thread. Posts are not edited for content.

Dag Forssell March 17, 1995

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Date: Mon Jun 06, 1994 1:44 pm PST
Subj: Introduction

Hello CSG_net,

My name is Dr. Jeffrey B. Vancouver. I am an assistant professor in Industrial/ Organizational Psychology at New York University. My research and teaching are in the areas of motivation and group processes. However, my primary mission (reference signal) is promoting interdisciplinary collaboration. To that end, I have come to adopt the philosophy of von Bertalanffy. That is, seeking a higher-order paradigm that will allow scientists working in different areas to collaborate and learn from one another.

I have been enamored with PCT for some time (about 7 years) and discovered this net about 3 years ago. I have been lurking on it ever since. Initially, I did not participate because my exposure to PCT had been through people in my field (e.g., Carver & Scheier, 1981; Lord & Hanges, 1987).

These renditions piqued my interest, but left much unclear. I have since read Powers' book (1973), which I concur is profound, and his first collection of works. In addition, I have read most of the second half of Hershberger's collection; most of the American Behavioral Scientist special edition (1991); and the Richardson (1991) book on feedback thought. As well as a lot more in my field that is using the basic PCT model.

Even after having "digested" much of this I was reluctant to participate in this list as it is such a time sucker. Cziko's recent Penis post sums up that problem. I have often deleted or save large numbers of posting to read later (which I have done about 25% of the time). But summer is here, I recently submitted an article about this stuff for my field, and I feel ready to participate more interactively.

One of the reasons I want to jump in now is in response to the recent posting Dag Forssell made to the TQM, reengineering, and quality lists. [See BPR-L.THD] I should have jumped in earlier, for there is a problem I see with that post that will turn off many on those lists. This is a problem that permeates this list. It is an us against the world attitude.

There are several reasons for this attitude, not the least of which is the truth to it. BUT, I do not think it is bad as portrayed here. For instance, Forssell, as Marken did in 1991, begins by articulating PCT as a science of purposiveness. They go on to say that purposiveness is considered an evil in psychology. There is no doubt that it WAS. But contemporary psychology is embracing purposeful behavior in a big way. For instance, the most popular motivational theory in organizational psychology currently is Locke's goal-setting theory. That theory is based on many of the same self-regulatory ideas as PCT. There is no question that Locke and his school (along with Bandura) has not appreciated PCT, but they have ended up specifying models that are purposeful.

In fact, I see many contemporary theories and applications based on the same underlying principles of PCT. My argument is much like Richardson's (1991), which I was amazed to see on your reading list, particularly given the recent thread with the systems/cybernetic person whose name I cannot recall, that attempted to distance PCT from systems.

Much of the rhetoric from all the schools of thought remind me of a joke that would be funny if it were not so telling. If someone could tell me the origin or any teller I would appreciate it because all I have is a vague memory, but it goes something like this.

A man approaches another who was about to jump off a bridge to certain death. In an attempt to talk the jumper down the conversation turned to religion. "I am a protestant," said the jumper. "So am I," said the first man, "what denomination?" "Baptist." "So am I, orthodox or reformed?" "Reformed." So am I, Eastern or traditional?" "Eastern." "So am I, Tririllian or Sectarian?" "Tririllian." "I am sectarian, you heretic!" and pushes the jumper off the bridge.

The point, common in religion, is that those who are closest in beliefs are often at greatest odds. The PCT school suffers this same solipsism that does not serve the greater aims of its members. This was made public recently in an exchange with Marken, who apparently has a bad boy image on this net. When pointed out to him, it appears an error signal was generated that changed his behavior. I am assuming that the change in behavior reflects an reference signal in him for healthy debate. I also extrapolate this reference signal to others on the net, and healthy debate is needed.

The most recent example of this on the net is the Paul Revere thread. Bill C. seems to represent the broader psychological community in his description of uncertainty and decision-making. His interest in the focus of control and resources parallels my own. Further his understanding of the current psychological literature seems to reflect a broader understanding in psychology than many on the net. This is not to call people on the net stupid, just limited. We all suffer from this problem. But, the rejection of psychology out-of-hand is dangerous. Perhaps Mary P. is right when she says PCT and the DME have nothing to do with anything psychology has dealt with, but I doubt it. She uses the chemistry/oxygen paradigm shift as her analogy. I prefer the Newtonian paradigm shift. Euclidian geometry is not "wrong," just limited in scope. So too are many of the decision-making models that use subjective expected utility (SEU). But, they are still useful if one understands the scope - an understand PCT can give. For example Beach's Image theory (book by that title 1990) uses both old SEU and new control theory concepts (although he seems to make some of the mistake Miller, Galanter & Pribium made).

But the point is that PCT netters don't often walk the talk. They want others to see their way but reject anything not from them. This leads to misunderstandings and straw images of the each others theories.

Another example from Dag's post. He says that psychology (presumable cognitive) articulates a model of blind execution of internal plans. My reading of the literature is that the plans are a set of reference signals, just like PCT talks about. Further, an error signal that is larger that the plan expects will cause a focus of attention to that point in the hierarchy (see Vallacher & Wegner, 1987, psych review). There is some very interesting stuff coming from many corners of psychology.

Another unfortunate impression one has from the Dag post is that PCT solves the human element problem for TQM. Any quality/TQM/BPR_L list members looking for PCT to solve these problems will be disappointed. This is not because PCT is a poor model, but because it is a long way from solving the applied "human element" problems. I think that the PCT model is VERY helpful for looking at the issues. BUT, each individual in the applied setting is controlling their own unique set of reference signals, using their own perceptual functions, and their own behavior repertoires. Dealing with all this complexity, diversity, and interaction is not easy, period. The one response I have come across most for not adopting systems theory and PCT is that it has not fulfilled its promise. It would help if PCT stopped promising so much so soon.

One final thing, I have been teaching PCT to undergrads and grads, so you can increment the number of teachers teaching it. I hope to participate more fully in the future, but forgive me if I am not as responsive as others on the list. As the assistant in my title implies, my am trying to get tenure.

Jeff Vancouver

P.S. I just had a student ask if PCT deals with non-optimizing reference signals. I believe so, but cannot give a primary reference. This is one of Bandura's issues with PCT and we want to squelch it. Any suggestions?

Date: Mon Jun 06, 1994 8:04 pm PST
Subj: Re: Introduction

Tom Bourbon [940606.1631]

> Hello CSG_net,

Hello, Jeff.

> I have been enamored with PCT for some time (about 7 years) and discovered this net about 3 years ago. I have been lurking on it ever since.

Now there is a virtual person to go with the name I've seen every time I reviewed the list of subscribers. I hope you have been watching long enough to recognize my remarks and questions in this reply as friendly.

> Initially, I did not participate because my exposure to PCT had been through people in my field (e.g., Carver & Scheier, 1981; Lord & Hanges, 1987).

> These renditions piqued my interest, but left much unclear. I have since read Powers' book (1973), which I concur is profound, and his first collection of works. In addition, I have read most of the second half of Hershberger's collection; most of the American Behavioral Scientist special edition (1991); and the Richardson (1991) book on feedback thought. As well as a lot more in my field that is using the basic PCT model.

You seem to have read a large portion of the collected works on PCT, as well as some other sources with which we might not be familiar. Could you give some citations of work in your field (Industrial/Organizational Psychology) that uses the basic PCT model? . . .

> One of the reasons I want to jump in now is in response to the recent posting Dag Forssell made to the TQM, reengineering, and quality lists. I should have jumped in earlier, for there is a problem I see with that post that will turn off many on those lists. This is a problem that permeates this list. It is an us against the world attitude.

Remember, you are seeing us when we talk among ourselves -- _after_ we have put on our best faces and tried to communicate with "the world." When we try to publish work on PCT modeling, we usually go out of our way to avoid such an "attitude" -- not that it seems to help.

> There are several reasons for this attitude, not the least of which is the truth to it. BUT, I do not think it is bad as portrayed here.

Hmm. That doesn't seem to jibe with the contents of my file of reviews and rejections for PCT-related manuscripts.

> For instance, Forssell, as Marken did in 1991, begins by articulating PCT as a science of purposiveness. They go on to say that purposiveness is considered an evil in psychology. There is no doubt that it WAS. But contemporary psychology is embracing purposeful behavior in a big way. For instance, the most popular motivational theory in organizational psychology currently is Locke's goal-setting theory. That theory is based on many of the same self-regulatory ideas as PCT.

It is certainly the case that there are many putatively self-regulatory models in psychology these days, especially in organizational psychology, or so I believe from my limited acquaintance with org. psych. But therein turns part of the tale: The model in PCT is not a self-regulatory model and PCT is not about self-regulation.

> There is no question that Locke and his school (along with Bandura) has not appreciated PCT, but they have ended up specifying models that are purposeful.

Or so they say. I've never seen either of them demonstrate that their models will behave (purposefully or otherwise) in simulation. Instead, I've seen them assert that things work in a particular way, then they gather voluminous correlational data in which they look for associations (low, but significant, correlations) between measures they *assert* are related to the process of self-regulation. Their research strategy doesn't really produce the kinds of data we need in order to determine if their "models" work at all, much less if they work in the alleged manner. (See more on this below.)

I say this as a description of the state of affairs, not as a criticism. I become critical only when adherents of that style of research begin to assert that they understand control theory better than we do, and that they know our ways of using it and testing it are inadequate. You will rarely see those comments from the self-regulatory camp in print, but they are very common at the stage or reviewing and rejecting articles on PCT modeling. Maybe that's one reason we seem to come across as playing us-against-the- world -- you never see the other side of the argument in print.

> In fact, I see many contemporary theories and applications based on the same underlying principles of PCT.

Are they based on the idea that behavior is the control of perception and that most of what an observer sees when watching one who controls is irrelevant or unknown to the controller? Or that the control of perception is usually not the same as what is often called self-regulation? Or is it that they say (but do not test in simulation) that feedback (in general, or perhaps negative feedback, or perhaps both negative and positive feedback) is important. I do not ask these questions rhetorically or sarcastically; I am continually on the lookout for new material in which people really do use PCT, whether or not they call it by that name. . . .

> The point, common in religion, is that those who are closest in beliefs are often at greatest odds. The PCT school suffers this same solipsism that does not serve the greater aims of its members

> The most recent example of this on the net is the Paul Revere thread. Bill C. seems to represent the broader psychological community in his description of uncertainty and decision-making. His interest in the focus of control and resources parallels my own. Further his understanding of the current psychological literature seems to reflect a broader understanding in psychology than many on the net. This is not to call people on the net stupid, just limited.

Thanks. Some of our reviewers are not as kind as you! :-))

> We all suffer from this problem. But, the rejection of psychology out-of-hand is dangerous.

That would certainly be the case, were we to do it. Again, it is a pity there are ethical constraints on our simply publishing all of the reviews and rejections of our manuscripts on PCT modeling. Those documents might give you a better feel for who is the rejector and who the rejectee.

> Perhaps Mary P. is right when she says PCT and the DME have nothing to do with anything psychology has dealt with, but I doubt it.

But she was merely saying what many of my reviewers have said. Let me use some of my own experience as an example. You mentioned that you have read both Wayne Hershberger's book and the special PCT issue of American Behavioral Scientist. Perhaps you saw my two published articles on PCT modeling of social interactions; In part, both articles were about instances in which two people simultaneously perform a tracking task and the actions of one or both of them interfere with a variable controlled by the other. Those are the only things I have in print on social interaction, but I have a file of unpublished related manuscripts and data, some going on eight years old. Whenever I submitted that work to traditional journals, I always cited people whose work might be seen as "related," even when I knew that was not the case. I was careful to say that I knew some aspects of the work were unconventional -- I sampled continuous data from two people, I ran models in simulation to determine if the models would reproduce then predict later instances of performance by the two people, and so on. I presented the manuscript as an example of a different way to do social research, a way that was different from methods in the conventional literature, but that in no way as intended as a challenge to or rejection of traditional methods, and on and on. The result? Rejections in which people said such things as, "This is not like the research we are accustomed to seeing." "Why continuous variables? Surely the author(s) could have recast the experiment to provide discrete data." And so on. In every case, I was told that I was dealing with something different -- something they weren't interested in. So you see, Mary had it right.

. . .

> But the point is that PCT netters don't often walk the talk. They want others to see their way but reject anything not from them. This leads to misunderstandings and straw images of the each others theories.

We are often said to offer "straw images" of other people's models, but in our defense I offer the fact that the people who posit "models" of self-regulation or self-control typically do not provide anything resembling a working model for their ideas and they certainly do not test their ideas by requiring their "models" to behave in simulation. Absent any working models from those theorists, we often try to turn their words into working models, in order to test them in simulation. Perhaps the fact that those "straw models" so often fail in simulation is a sign of something other than a deliberate attempt by us to make other people look bad. After all, *anyone* -- anyone at all -- who objects to our "straw images" can, at any time, provide their own (non-straw) version of a model and demonstrate that it *does* behave the way they say it will. (Of course, when I have foolishly suggested that possibility, in manuscripts that I submitted, my suggestion has drawn comments that I was engaging in a cute, cheap ploy, intended to make my own presentation look better. And all the while I thought I was inviting people to shoot me down and make themselves look good. Silly me!)

> Another example from Dag's post. He says that psychology (presumable cognitive) articulates a model of blind execution of internal plans. My reading of the literature is that the plans are a set of reference signals, just like PCT talks about.

This is a crucial point, Jeff. Do the *writers* of that literature say that people act to produce and control their own perceptions, with their actions serving as unintended means to that end? (In the PCT model, we use reference signals to represent those kinds of intentions.) Or is it, as you literally say, that you *read* the literature that way -- perhaps reading into it something you *want to see* -- something you believe *ought to be there*?

> Further, an error signal that is larger that the plan expects will cause a focus of attention to that point in the hierarchy (see Vallacher & Wegner, 1987, psych review). There is some very interesting stuff coming from many corners of psychology.

I'm not sure what you mean by " an error signal that is larger than the plan expects." Could you say a little more about that idea?

> BUT, each individual in the applied setting is controlling their own unique set of reference signals, using their own perceptual functions, and their own behavior repertoires. Dealing with all this complexity, diversity, and interaction is not easy, period. The one response I have come across most for not adopting systems theory and PCT is that it has not fulfilled its promise. It would help if PCT stopped promising so much so soon.

PCT doesn't promise anything, but some of its adherents do. What should "we" do, instead? (And who are we, anyway?) :-)

> One final thing, I have been teaching PCT to undergrads and grads, so you can increment the number of teachers teaching it.

Great. You can take my place -- I recently stopped teaching and fell from the list!

> I hope to participate more fully in the future, but forgive me if I am not as responsive as others on the list.

Watch out; this net has a way of taking over your life!

> As the assistant in my title implies, my am trying to get tenure.

A brave person, indeed!

See you on the list.

Later, Tom

Date: Mon Jun 06, 1994 9:55 pm PST
Subj: Welcome, Jeff Vancouver

[From Bill Powers (940606.1820 MDT)]

Jeff Vancouver (940606, I presume) --

Welcome to the land of the speaking, Jeff. I'm glad you have decided to speak up, because you have some important observations to make.

I'll let Dag Forssell speak for himself. I can tell you, however, that there are often things going on behind the scenes that you wouldn't hear about. For example:

> But contemporary psychology is embracing purposeful behavior in a big way. For instance, the most popular motivational theory in organizational psychology currently is Locke's goal-setting theory. That theory is based on many of the same self- regulatory ideas as PCT. There is no question that Locke and his school (along with Bandura) has not appreciated PCT, but they have ended up specifying models that are purposeful.

What they have done is to admit that behavior is purposeful, which people have been claiming at least since McDougall in the 1920s, and they may even conclude that behavior is caused by the difference between the a goal and reality, but they (Locke and Bandura, for example) explicitly reject control theory as an explanation of these well-known phenomena. This is extremely puzzling, so puzzling that both Mary and I have written to Locke in an attempt to clear up some of his misconceptions about control theory (in response to an article by him violently attacking control theory). The problem is that Locke rejects control theory precisely because of his mistaken idea of what it is, and he refuses to change his understanding of it. Our correspondence with him has netted exactly zero by way of any improved understanding. He has picked a position and is not going to change it.

The appearance of George Richardson's book on our reading list is not so strange. If you read the book carefully, you will see that he puts PCT on a thread of its own, which is neither the cybernetics thread nor the systems thread. I was one of the prepublication reviewers of his book, and I gave it a thumbs up because he had a deep enough understanding of PCT to see that it differed in essential ways from the two mainstream ideas.

You have to have some experience with individuals from other disciplines to grasp just how ridiculous the rejection of PCT ideas can get. There is fierce academic competition going on out there, and the tactics used for trying to keep opposing ideas in abeyance are sometimes every bit as self-serving and underhanded as one might expect in the sleazier parts of the business world. You and I think of the ideal scientist as a person who might be dismayed at discovering that his ideas are refuted by some new approach, but never as a person who would deliberately try to suppress such ideas simply to maintain his reputation of rightness. Ideal scientists, alas, are few and far between, and they do not rise to positions of power and influence. Those who are at the top are, like any person endowed suspiciously well with the rewards of life, at the top because that is where they want to be and intend to stay.

You say

> In fact, I see many contemporary theories and applications based on the same underlying principles of PCT.

This I seriously doubt. You may find a lot of people talking about purposes and goals, but that is not theory; it is simply observation. I doubt very much that you will find _any_ other theory that contains an explanation of purposive and goal-directed behavior like that of PCT -- unless the explanation is in fact PCT, acknowledged or not. For example, Robin Vallacher writes a lot about very PCT-ish ideas, but that is not surprising. In about 1974 or 75, he read my book and invited me to give a seminar at ITT, where he was at the time. He got a thorough personal introduction to my brand of control theory, but you will have a hard time finding any acknowledgement of that in his writings. Even then, incidentally, I was saying that the focus of attention goes to where the largest error signals are, the only way I could think of to confine reorganization to the areas of the brain that actually needed reorganization.

Carver and Schier, who are a little better at acknowledgements, asked for my criticisms of their first book while it was being written, and I gave considerable time and effort to them, including a critique of one of the main points in their book about self- awareness -- which they ignored, although the last time I spoke to Carver (to invite him to a CSG meeting, which he begged off from), he mentioned that they gave up on that approach.

I have corresponded with literally hundreds of people about PCT, and have shared my ideas and explanations without stint when asked for them, holding nothing back. Whenever I have seen some piece of work that looked as if it were on the same track or close to it, I have written to the authors inviting their attention to PCT, explaining how it might be useful to them or apply in their work. The return on this effort has been miserable, although I have seen an increase in allusions to PCT-like ideas in the literature as the years have gone by, and have wondered just how much of that reflected my efforts. The least satisfactory responses have uniformly come from the most famous and admired people. Gerald Edelman, for example, was condescending and insulting in his reply. Roger Penrose ignored my letter completely. Jeremy Campbell never replied. Joseph Engleberger concluded that there was nothing in Perceptual Control Theory that was of any interest in his work with robotics. The list is long.

The biggest problem I see is that people simply don't realize the difference between observing that behavior is purposive and explaining how it can possibly be that. The conclusions of PCT get across, but rarely does anything of the theory come through. Even your student obviously doesn't realize that there is a theory beneath the trappings of PCT:

> P.S. I just had a student ask if PCT deals with non-optimizing reference signals.

The words "non-optimizing reference signal" don't strike me as meaning anything. A reference signal is simply a specification for the state of a perception: it is a signal that has a particular value, against which a perceptual signal is compared. There is nothing to optimize about it, as far as I can see, and a reference signal can certain DO no optimizing of anything.

I don't go in much for bashing the opposition, except when particularly frustrated and needing to blow off some steam among friends. I would not dream of using most of the tactics that have been used on me. I think that the face of PCT that you see on CSG-L is quite different from its public face. I do agree with you wholeheartedly about promising what we have never in fact delivered; I am as reluctant to emit junk mail as I am to receive it. But I have learned long ago that people do the best they can, and if my standards differ from theirs so be it. I just want to make sure that if a person uses the term PCT, it is in fact PCT that is being talked about. Even that isn't always easy to do.

Best Bill P.

Date: Mon Jun 06, 1994 10:53 pm PST
Subj: Re: Introduction

[From Rick Marken (940606.1800)] Jeff Vancouver (940606)

> Hello CSG_net

Hi Jeff!

> I have been enamored with PCT for some time (about 7 years)

Don't talk of love. Show me! ;-)

> There is no question that Locke and his school (along with Bandura) has not appreciated PCT, but they have ended up specifying models that are purposeful.

Two little nits. First, to my knowledge, Bandura and Locke have never recognized the phenomenon of purposeful behavior as control. Their idea of "purposeful behavior" is a lot like the idea of resting state of a dynamic variable-- no control involved at all. Second, they have never specified a model (in the PCT sense, ie. a model that actually behaves) of anything. Other than that, they have indeed done what you said.

> In fact, I see many contemporary theories and applications based on the same underlying principles of PCT.

This is fairly vague. One "principle" of PCT is that motor output is a function of perceptual input -- o = f(i) -- and there are many theories based on this principle. But in PCT this "principle" occurs in a closed negative feedback loop in which, ultimately, input is a function of an internal reference variable: behavior then is the control of a perceptual variable. Is there really any other contemporary theory (besides PCT) that says this? If so, then I would imagine that there would be a great deal of research being done on the types of perceptual variables that organisms control. Where's the research on controlled variables?

> healthy debate is needed.

What's a healthy debate? What was "unhealthy" about the debates that have been going on on the net?

> the rejection of psychology out-of-hand is dangerous.

Who has rejected psychology out of hand? I think some of us (Bill P., Tom B. and I) have rejected some individual tenets of psychology rather handily, though ;-)

> Perhaps Mary P. is right when she says PCT and the DME [Decision Making Entity] have nothing to do with anything psychology has dealt with, but I doubt it.

I think Mary has been suggesting that the DME probably has nothing to do with PCT. PCT, however, has everything to do with psychology.

> But the point is that PCT netters don't often walk the talk. They want others to see their way but reject anything not from them.

We explain the phenomena and model of control. We reject what is demonstrably false (information about the cause of variation in perception, control of contrasts in speech, social control, etc etc). We don't reject "anything" that does not come from "us"; we reject what's wrong. In fact, there is a great deal of work that comes out of conventional psychology on which we rely; especially the work on perception. Is there something, in particular, that you think we have unfairly rejected?

> This leads to misunderstandings and straw images of the each others theories.

PCT is tested by comparing the performance of working models to actual behavior. In order to compare alternative theories to PCT we often have to translate verbal descriptions into working models. When our implementations of other theories fail miserably, the proponents of these theories yell "straw man". We have asked the proponents of other theories to show us how to implement their theories correctly; that's usually the last time we hear from them -- as they walk away infuriated, still yelling "straw man" and mumbling about how we reject theories just because they are not ours. Do you have any suggestions about how to deal with this problem?

> [Dag] says that psychology (presumable cognitive) articulates a model of blind execution of internal plans. My reading of the literature is that the plans are a set of reference signals, just like PCT talks about.

Really? And these reference signals specify the required states of perceptual variables? Then why are there no studies of the perceptual variables that are controlled during the execution of these plans? Are the plans themselves a perceptual variable? If plans are controlled, then a perception of the plan would be the controlled variable, in PCT? Where are the studies of the control of the perception of a plan?

> Further, an error signal that is larger that the plan expects will cause a focus of attention to that point in the hierarchy (see Vallacher & Wegner, 1987, psych review).

How does a plan "expect" a certain level of error signal? Is the plan controlling the error signal? If so, then how does "focusing attention" move the error signal to its (possibly non-zero) reference level? Do they base their model on data showing control of error signals? What data is their model based on? How well does it account for the data?

> There is some very interesting stuff coming from many corners of psychology.

Yes. It looks very interesting. Let's discuss the Vallacher & Wegner model, by all means. But, before we get started, I've got to know: if their model produces no quantitative results, if it predicts average behavior over subjects or over trials, if it doesn't work at all, do I still have to like it and not criticize it in order to have a "healthy debate"? If so, then we can save a lot of time since I can give you my evaluation of their model right now -- excellent, most illuminating, marvelous ;-).

> The one response I have come across most for not adopting systems theory and PCT is that it has not fulfilled its promise.

PCT only promises the correct basic model of purposeful behavior. It works extraordinarily well in the limited circumstances where it has been tested. It makes clear, falsifiable predictions that, so far, have not been falsified. The conventional psychological models and data that fill the journals rarely make clear, falsifiable predictions about anything except statistical properties of behavior -- so they are never really rejected though they rarely work well. I'd say that PCT has fulfilled its promise in spades -- the promise of providing a strong FOUNDATION for the study of behavior. Apparently, few people want to build on that foundation because it's not yet the Taj Mahal. They would rather keep playing in the shack built on the shifting desert dunes. Nu? What can we do?

> P.S. I just had a student ask if PCT deals with non-optimizing reference signals. I believe so, but cannot give a primary reference.

What in the world is a non-optimizing reference signal? Optimization is a judgment an observer makes about the behavior of a control system; a control system just controls.

> This is one of Bandura's issues with PCT and we want to squelch it. Any suggestions?

Boy, Bandura goes right to the periphery of the issue, doesn't he? My suggestion for dealing with it? Admit it. Say "Yes, you're right Al. PCT does NOT deal with non-optimizing reference signals. You are one sharp cookie Al. If only those PCT guys knew what they were missing... er ... what are they missing, Al??";-)

Best Rick

Date: Tue Jun 07, 1994 1:48 am PST
Subj: Re: Introduction

<[Bill Leach 940606.23:41 EST(EDT)]
>Jeff Vancouver 10030 on Mon, 06 Jun 1994 10:52:58 -0400

Jeff;

Being both a "non-professional" and really a novice seeker of understanding of PCT, I'll comment to a "lurker".

First before being too critical, I'd like to remark that you likely could contribute a great deal what goes on here. You are quite obviously not one to "shoot to quickly from the hip" and can add a great deal of experience to the discussions that take place here.

I personally agree that there is something to the "Us against the world" attitude that is present here. Rick is, as most (including Rick) are willing to admit, the most direct about "attacking" that which appear "not to conform".

It is my opinion however, that it was this very insistence on purity that has helped me to understand PCT better than I might otherwise have done. Rick was often quite wrong about where I was erring or often even that I was erring BUT, the result of trying to explain or "defend" something that I said invariably resulted in my learning something new about PCT. Quite often the learning actually came from a posting of Bill Powers in his attempt to clear up a difference between Rick's perspective and my own but again, such a posting would likely NOT have occurred if Rick had not also been so insistent.

I agree that there is a 'danger' that some may be driven away by the "puritan" approach of many that post here frequently. I am not sure what to say about it though. I know, for example, that Dag is truly appreciative of serious consideration of his work. I feel that he is able to make excellent use of the comments that are made.

People such a Bill, Mary, Tom and Rick just about have to stick to the purest possible form of PCT. Even the HPCT discussion need to contain the reservations that Bill so often makes. These are the people that "are" PCT and they really do need to be careful if they are not to become like some many "popular" scientists. Bill's attitude about how PCT will stand ONLY FOR SO LONG AS REALITY ALLOWS is almost frightening to one accustomed to physical sciences but it really IS mandatory.

It is this very demand that Bill has demanded that makes PCT so vastly different from all other attempts at a behavioral science. All other behavioral science is too willing to dismiss variance as noise. PCT is unwilling to be so "loose". But that also means that PCT MUST remain very precise in terminology and in what IS and IS NOT actually a part of the theory.

The interest of many of us is in the practical application of PCT to "real life situations". Dag is especially one of those and of course Ed Ford is another. Personally, my interest is first to gain a real understanding of what PCT is and then how it can be used in "real world" situations.

I think it is the responsibility of those of us that have a "real world" concern with PCT to recognize that those that are the "bearer of the torch" CAN NOT permit "loose" use of PCT. Those "bearers" may well make errors in the understanding of the posting from such as myself but it is MY responsibility to attempt to clear up possible misunderstandings.

You have mentioned that a number of researchers have produced work where it appears that they have grasped PCT principles. Personally I would agree. Indeed (though I now need to study him again) the man that initiated the entire idea of "pop-psychology" (Dale Carnegie) in the classic book "How to win friends and influence people" was obviously espousing PCT principles. It is, on the other hand, probably very critical that he did not actually recognize the fundamental essence of what he was saying.

In the same manner, those that talk about "purposeful" behavior without realizing that humans are negative feedback control systems controlling perception ALL the time no matter what happens around them, are missing the main point (I think).

The point is (again I think) that the environment DOES NOT cause behavior -- ever!

Does the environment affect behavior (as perceived by others)? Of course but the only really important element is the subject's perceptions and the reference to which the perceptions are being controlled. This does not "simplify" anything (as you mentioned) but it does mean that often those trying to help or otherwise deal with others are making things more complicated by introducing issues that are not...

> Religion

This IS a fine point. The PCT "torch bearers" have to be very careful to avoid 'religion'. Several of Bill's postings have dealt with just that subject. He has frequently stated that he does not want a bunch of "supporters" that blindly believe the "high priests". On the Other Hand, heretics (yes Rick, I still believe that heretics can exist) must not go unanswered. PCT is in it's infancy and certainly there may well be many improvements and refinements. Martin's insistence upon evoking Information Theory could at sometime in the future prove to be necessary (at some time if or when experimental results fail to match reality).

I don't think that Bill (and others) are being "religious" when they insist that any "enhancements" stand the test of necessity.

> The point, common in religion, is that those who are closest in beliefs are often at greatest odds.

Again, I agree that this is true but...

I have often felt that Rick was wrong in his perception of what Dag (specifically) was saying. I really don't believe that it hurt either Dag or myself to try to come to an understanding with Rick. Indeed, it is likely that both Dag and I benefitted from the exchange.

> The PCT school suffers this same solipsism that does not serve the greater aims of its members. This was made public recently in an exchange with Marken, who apparently has a bad boy image on this net.

I have not had time to review the postings on the net while Rick was "silent" (I was also not on the net during that time) but my understanding was that Rick stayed away to see if the discussions were "freer" without him. I don't believe that they were but will defer to others since I have not reviewed the postings.

> This is not to call the people on the net stupid, just limited.

Here is a place where I again tend to agree with you except that I feel that my trying to understand the PCT "viewpoint" as espoused by Rick, Bill, Tom and others generally helps me more than trying to convince them that their viewpoint is limited.

I have several times myself been a bit frustrated when trying to integrate my limited understanding of PCT with my limited experience in human behavior. I believe that the frustration comes from failure to understand the significance of PCT and from failure to recognize that the theory itself is not able to explain may aspects of human behavior in a rigorous fashion (rigor as defined for PCT not psychology).

Thus, very soon after leaving the realm of direct experiment in PCT principles we are in an area where "opinions carry the weight". This is tough territory for the newcomer (such as myself). Bill and others have given a great deal of thought to many of the assertion that they make. Not only that, but they have had the experience of trying to "second guess" the theory in areas where experiments were eventually performed and "learned a thing or two".

> Another example from Dag's post. He says that psychology (presumable cognitive) articulates a model of blind execution of internal plans. My reading of the literature is that the plans are a set of reference signals, just like PCT talks about. Further, an error signal that is larger that the plan expects will cause a focus of attention to that point in the hierarchy (see Vallacher & Wegner, 1987, psych review). There is some very interesting stuff coming from many corners of psychology.

I suspect that this again misses the point. Anyone that really tries to observe human behavior will conclude that there is "purpose" in at least most behavior. What PCT says is not that there is "purpose" in behavior but that all behavior is the result of attempting to control perception. There is NO difference between the two assertion IF "purpose" is defined to mean "control of perception" but that is seldom the case. "Purpose" is usually taken to mean some sort of "higher" goal (that maybe the subject has even written on a piece of paper). Such a goal may or may not be related to behavior and this IS significant.

> TQM

Again, I agree and believe that Dag agrees too. However, the problem is that most TQM programs fail to consider how and why humans function the way that they do. It is not that PCT will provide any instant magic answers but rather that understanding PCT will keep one from wasting time trying to deal with matters and principles that have nothing to do with the problems that one is facing.

> promises

I have to consider B:CP as far a promises. I think that Bill did a good job of pointing out that PCT could help a great deal in understanding the behavior of living things. He has also stated that he personally doubts that anyone will ever model a human mind well enough to use it for exact prediction of individual behavior.

You have raised a number of issues that are a real importance to anyone interested in PCT. I certainly don't speak for the net and certainly caught my share of flack for fuzzy thinking and other errors. I really do believe that the "mechanics" PCT is not "where it is at" for those of us not directly involved in PCT research but rather in trying to really understand the implications of PCT.

I also understand your difficulty in keeping up with the net activity. I have not made a serious posting for close to two months and even this one is far more hurried than it deserves. Please do try to comment when you can, if you stir me to trying to think, I can just imagine what you must be doing to others here.

-bill

Date: Tue Jun 07, 1994 2:22 am PST
Subj: Afterthoughts on PCT versus other theories

[From Bill Powers (940607.0200 MDT)]

Trying to get adjusted for GMT, so far with the opposite results. I am some where in Australia at the moment.

Jeff Vancouver (940606) --

Afterthoughts from re-reading your post. You say

> However, my primary mission (reference signal) is promoting interdisciplinary collaboration. To that end, I have come to adopt the philosophy of von Bertalanffy. That is, seeking a higher-order paradigm that will allow scientists working in different areas to collaborate and learn from one another.

It's not that easy to put into words what your "primary" reference signal is. All you have to do is ask yourself, "Suppose I did succeed in promoting interdisciplinary collaboration. What would that get me?" This will show you that achieving interdisciplinary collaboration is only a means to satisfying an even higher-level reference condition -- and not just one single condition.

Actually, interdisciplinary collaboration is a lot easier to achieve than collaboration within a discipline like psychology. The stickiest situations arise when PCT comes up against people offering different theories about the _very same phenomenon_. Bandura and Locke reject control theory while describing the very phenomena that control theory is designed to explain. Even without PCT, you have cognitive scientists, Skinnerian behaviorists (there are plenty of them alive and kicking -- er -- responding) and personality theorists (like Lord and Hanges, Hyland, Bandura, and Locke) all operating completely separately and completely at odds with each other. Just consider the recent split of the whole field of psychology right down the middle, clinicians against self-proclaimed "scientists."

I find the usage of the term "theory" in various branches of the behavioral sciences rather odd. When Bandura says he has a "theory" that there are "proactive" behaviors, he seems to think that simply announcing this phenomenon amounts to offering a theory. But to me, it is only a description of something that we either can or can't observe. If the phenomenon is replicable, we have to accept it as real -- but that leaves the job of theory, as I have always understood the term, undone.

PCT is not simply a description of purposive behavior dressed up in a new vocabulary. We don't just substitute "reference signal" for "goal," "perceptual signal" for "stimulus," and "output" for "response." And when someone makes the translations in the opposite direction, the result is not an understanding of PCT, it is only switching words and continuing to apply them in the context of the same old model. In fact, a facile adoption of the terminology of PCT is an excellent way to avoid getting the point.

The point of PCT is to explain how it is that an organism can select some physical condition that does not currently exist and bring it into existence by acting on the environment. The first step in getting psychologists to understand the explanation is to get them to accept that organisms can in fact do this sort of thing. There have always been a few psychologists who accepted that fact, although usually with poor justifications. But for most of my career, at least, the vast majority of "behavioral scientists" didn't even accept that as a valid description of behavior. This meant that I couldn't even take the first step toward explaining PCT -- what good does it do to offer an explanation of a phenomenon that your listeners believe to be illusory? I have spent a large part of my career just trying to demonstrate that the phenomenon itself exists.

Now that more people are coming around to the view that behavior is purposive, goal-directed, intentional, etc., it is at least a little easier to start explaining what PCT is about. Or it should be. Unfortunately, too many people STILL think that when you have described a phenomenon, you have explained it. So when Bandura and Locke say that people pursue goals, and that goals cause behavior through a method of discrepancy reduction, they think they have explained goal-seeking behavior. This illusion could be shattered in an instant if you could just get them to focus on the question, "What is a goal, that it can have such an effect?" Or, "How is it that a discrepancy between a goal and the actuality can produce just those detailed motor activities that have consequences tending to reduce the discrepancy?"

PCT is sitting here with detailed answers to such questions, and has been sitting here for nigh unto 40 years, but to no avail: that sort of question doesn't seem to come up among the likes of Locke and Bandura. In explaining that behavior is goal-directed, they seem to think they have reached the foundations. I hope you can understand how frustrating that is to me. I want to say, "Good, now you understand the problem. How about listening to my solution?" But they seem to think that their description of the problem IS the solution.

In fields like AI, neuroscience, and the new incarnation of AI, Artificial Life, almost the opposite problem exists for PCT. Here we have explanations galore in terms of neural circuits and system designs, all good stuff, but practically no appreciation of the phenomena of ordinary purposive behavior. Thus many of these people are using their armory of explanatory tools to explain phenomena that don't occur in nature. Look at the neural net people. What behaviors are they trying to explain? Responses to stimuli! Unfortunately, when you approach the organism at the level of the functions of its components, as PCT does, you can construct gazillions of models that do _something_. But unless you can tie the models to the sorts of behaviors that organisms actually produce, the whole effort is just an exercise in imagination; mathematical Onanism.

PCT consists of two equally important parts: the definition of the problem (people seem to behave in very specific purposive ways) and a model that solves the problem (how they must be constructed in order to do that). It is necessary to take both parts of PCT seriously to understand PCT. In fact, you can't really understand either part of PCT without putting some serious effort into understanding the other part. A lot of our problems on the net have come from people with a fair understanding of one facet of PCT, but hardly any understanding of the other.

The PCT model, with its hierarchical control-loop structure and mathematical properties, helps us to recognize purposive control behavior when we see it; seeing examples of real behavior helps us to select possible architectures and discard others that are equally plausible on computational grounds alone. This interplay requires approaching PCT always from both sides, giving neither side a disproportionate emphasis. Observation keeps us honest; computation keeps us rational. Which one should we do without?

Best, Bill P.

Date: Tue Jun 07, 1994 12:01 pm PST
Subj: Comments from Mary

[Mary Powers 940607] Jeff Vancouver:

The reason I used the phlogiston/oxygen example was because the flip-flop from something emitted by a burning substance to something being added to it resembled to me the flip-flop from behavior being an outcome or consequence of external or internal forces to behavior simply being a means by which something else entirely is accomplished - the control of perception. "Rejection of other psychologies out-of-hand is dangerous" perhaps (in fact carries a considerable risk of career-blight), but it was not entered into lightly. I'd say that of the 40 or so years PCT has been in existence it took 15 or 20 to come, reluctantly, to the conclusion that PCT was revolutionary and would continue to meet for many years to come the kind of resistance and rejection that Thomas Kuhn described.

Some people scold us because they think we are knocking other psychologies unnecessarily and making life more difficult for ourselves than we have to. I agree that "those who are closest in beliefs are often at greatest odds". However, many of the people you cite (Carver, Lord) are further away from PCT than they (or you?) think. Truly, they are talking about phlogiston and we're talking about oxygen.

But Bill has answered your point about purpose, pointing out that admitting or asserting that it exists is not the same as having an explanation of how it works.

To add to Bill's litany of active lack of interest in control theory, I wrote to Lord and Hanges years ago, and received no reply. Some time later, Lord called Bill (about something to do with the Volitional Action book). While he was on the phone, I asked Bill to ask him why he never replied to me. The answer: " didn't know what to make of it". The scientific mind at work ;-)

[added later]

Before this is sent, I saw the posts from Bill, Rick, Tom and Bill L.

Do you feel like a quarterback sacked by the entire defensive line? That is certainly not the intention. The criticisms you made were very important to everybody who answered you - more hostile versions are very familiar (Tom Bourbon's famous rejection file, etc.)

I just want to clarify one point - theory versus practical applications. I'm wondering if you have gone beyond reading about PCT to getting the computer demos and simulations. I think they bring home the point about PCT being an explanatory theory, as opposed to descriptive or statistical. Going back to Kuhn, this suggests that pre-PCT psychology is pre-scientific. Lots of data, lots of ideas about how it ties together, but no theory in the sense that physics has theories (part of the difficulty here is the word theory, meaning a body of principles OR a guess or conjecture). If we criticize other psychological theories, it is from the point of view of the first kind looking at the second kind. Certainly as far as the basic model is concerned. We aren't so much psychology-bashing as theory-bashing.

Lots of therapists and educators and social workers and organizational developers do good work - no matter what theory they believe underlies what they do. The fashions come and go in these fields and don't make much difference. We like to think that the good ones are intuitive control theorists who probably don't need any help - and that the bad ones can learn to be better by being taught what the good ones figured out for themselves. Psychologists teach useful rules and techniques, but these often have no connection with one another, and are rationalized by competing and incompatible theories as to why they work. PCT is almost in another universe, demanding of, and at least sometimes providing, the "whys" a plausible theory of "how".

Mary P.

Date: Tue Jun 07, 1994 12:42 pm PST
Subj: Us against the world

[From Rick Marken (940607.1020)] Jeff Vancouver (940606)

> It is an us against the world attitude.

Tom Bourbon (940606.1631)--

> Remember, you are seeing us when we talk among ourselves -- _after_ we have put on our best faces and tried to communicate with "the world." When we try to publish work on PCT modeling, we usually go out of our way to avoid such an "attitude" -- not that it seems to help.

This is an excellent point. I think if you saw how Tom, Bill P. and I have been treated by reviewers and editors you would see who really has the "us against the world" attitude. CSG-L is the only place we get to talk about PCT honestly and candidly, without having to worry about whether what we say about the model is "palatable" to those who are in power (those who control access to the journals, grants, tenure).

We are only against cant and arm waving. If there is an alternative to the view that 1) behavior is control and 2) PCT is the model that explains that phenomenon then we are happy to consider it. So far, no viable alternative has been presented. Is that our fault? Was it Copernicus' fault that a heliocentric model ultimately was simpler and more accurate than the geocentric one?

Do the people who reject PCT have an "us against the world" attitude too?

Best Rick

Date: Mon Jun 13, 1994 9:25 am PST
Subj: Responding to welcomes - Jeff

Hello again,

I am pleased that my introduction got such a response. Being ignored would have been the ultimate insult. I did not feel unduly rebuffed, but, of course, I will respond back. My response centers around themes, not necessarily individuals. Specifically, I consider the following issues: self-regulation, modeling, some theorists in my field, and some minor issues:

Self-regulation:

Forgive my ignorance, but I think Tom Bourbon and Bill Powers think that the difference between self-regulation models and PCT is that PCT is about controlling perceptions and self-regulation is about controlling behavior. This is no doubt because the promoters of many self-regulation models _say_ they are about regulating behavior. However, if the model incorporates a test of the difference between a perceived state and a desired state, the result of which drives behavior, then the model is describing the control of perceptions. If the modeler says the model describes how individuals control behavior, they either don't realize their error or they are trying to reach a certain audience. But the model still describes the control of perceptions. D. Ford's Living Systems Framework is that type of model. I am still trying to get a handle on the consequences of the error for those models. So, to answer Marken's (940606.1800) post, yes there are others that describe the CSG loop.

Modelling:

A major theme in the responses to my introduction was the role of modelling as a test of PCT and the other models I mentioned (indirectly through proponents - e.g., Locke, Bandura). I think something that people on this net understand that many in psychology do not understand is that the ultimate goal of science is to create a comprehensive model of the phenomenon they study. (Don't misunderstand me here. That goal will never be achieved. Indeed, the science should be constantly refining, filling in gaps, and occasionally rejecting the model - if one is to believe Kuhn - which I do.), But psychology, burned by previous attempts at grand theories and comprised of individual, autonomous control systems is leery of such models. As I understand it, this is not unique to psychology. Other sciences have gone or are going through similar periods (the pre-paradigm or pre-paradigm shift period, Mary Powers 940607). As a result, grand theories are eschewed for mini-theories. Mini-theories, because of their reliance on exogenous variables, do not lend themselves to modeling. That is, they won't work because importance relationships and variables are left out of the model - usually feedback relationships. Nor do the architects of these theories understand how to model or what it can do for them. Further, these theories are empirical generalizations of relationships between latent variables, not structural models of underlying architecture (Powers, 1973). The question, that we might answer differently, is whether these mini-theories are of any scientific value. (Practical value is a related, but separate question). I think they are because I think there are gaps in PCT that have been addressed, certainly not resolved, by these mini-theories. More on these gaps later.

But the previous argument is mostly on an intellectually level. The reality of rejection letters and lack of collaborative spirit from the powers-that-be is painful, dispiriting, and financially and occupationally challenging. I apologize for raising any of those feelings. I have numerous colleagues with less than kind rejection letters based on the same reasons that many of you alluded to. On the other hand, I know others who have had little trouble on that score. Of course, their work is probably not completely in line with the "core." Most notably, they do not require a working model as a requirement for their work (there is also an emerging appreciation for which journals are "friendly" and which are not). Herein lies one of my concerns with the solipsism among the netters. Marken (940606) says "PCT is tested by comparing the performance of working models to actual behavior. In order to compare alternative theories to PCT we often have to translate verbal descriptions into working models." This requirement confounds the test of a theory with the theory. Marken is applying his criteria for a theory on the other's theory. I do not condemn the criteria, merely the blind application. If empirical data is always required, Einstein's GTR would never have lasted the 20 (?) years before it could be tested.

Now I realize that these mini-theories are not GTR. Indeed, many are very problematic. But I try to separate the chafe from the wheat. Carver & Scheier's self-awareness construct was clearly wanting, but they exposed many to control mechanisms that had not known them before. To quote Bill P. (940607) "Now that more people are coming around to the view that behavior is purposive, goal-directed, intentional, etc., it is at least a little easier to start explaining what PCT is about." No doubt there are errors in the views perpetuated by the non-PCT models, but it is a step. Further, theories like Carver & Scheier's deal with many social processes which PCT does not deal with as thoroughly. Identifying the discrepancies between these models and PCT, and developing empirical tests the results of which both parties can take as evidence one way or another is, I believe, a reasonable next step. And let me be perfectly clear, I think sometimes they have a point and PCT needs to incorporate it.

The problem with Marken's approach is that they don't buy/understand the data you use. Most don't understand modeling and/or don't trust its results. The rigorous requirements, central to your understanding of science, are seen as too rigorous for a science in it's infancy. You say it need not be that way and I TEND to agree, but one must lead, not force the transition. (Some will never change, leave them behind). Where I do not agree, perhaps because I was raised as one of them, is at the higher-level processes. Here I will talk about gaps. (I suspect I will learn of my ignorance, but that is one reason for being here.)

Gaps & theorists in my field

First, as Bill P. said in his intro to Living Control Systems, there is no content. What are the intrinsic signals and where do they come from? What are, if not the higher-order references signals, likely candidates and where do they come from? How are actions chosen and how does reorganization proceed? I see stabs at the first question on this net (biological needs) but another type of intrinsic signal is probably system efficiency variables. The recent discussion of uncertainty is one such variable. You are absolutely right to attempt to figure out how the organism could perceive uncertainty and reject it if it can't (but beware, just cause you cannot figure it out does not mean it doesn't exist). The point is that another perspective has inform the guess that uncertainty is a reference signal.

My reading of your literature and posts is that the identification of higher-order reference signals is truly a gap (although I fear I misunderstand the posts here). Many in psychology are seeking to study candidates for reference signal, including Little, Deci & Ryan, Emmons, Pervin (See edited books by Pervins, 1989, _Goal Concepts in Personality and Social Psychology_, and Ford & Ford's, 1987, _Human's as Self-Constructing Living Systems.). These are self-regulation theorists, and like I said above, I don't care what they think they are doing, I find what they are doing useful and interesting and I think some of you should too.

To the third question, dealing with reorganization (and other matters like stress and leadership) are people in my field. Proponents of PCT-like models (forgive them their transgressions), including Tsui & Ashford (e.g., Journal of Management, 1994), Edwards (e.g., Academy of Management Review, 1992), Cropanzano, James, & Cetera (Research in Organizational Behavior, 1993), Lord & Levy (Applied Psychology: An international review, 1994) and Manz (Academy of Management Review, 1986) to name a few. Other interesting thinkers include the Germany action theorists (Kuhl, Hechausen) and others (Vallacher & Wegner). Many or most of these authors or theories may have already been read and rejected. I do not agree with everything _any_ of them say. But herein lies the challenge: finding which loops conflict, proposing alternatives, and testing the them.

Marken (940606) says you have rejected social control. Well, I am not exactly sure what is meant by social control, but my impression from earlier threads (and B:CP) is that you rejected social influence. I find this very problematic. Marken (same post) says you have rejected "information about the cause of variation in perception." Again, I don't know exactly what Marken means by that phrase, but I get the impression from my readings that PCT eschews attributions and other beliefs (like self-efficacy) as relevant. This I find very problematic. Now I even find that the DME is not part of PCT. You mean that all conscience thought is irrelevant? This is VERY problematic. Reorganization is not simply random. A model that shows that it could be (e.g., E. coli, which some of you have contented) does not make it so. But I think I must be misunderstanding you here, too. Planning and thinking is part of PCT. Figuring out how that planning and thinking translates into choosing and doing is where contemporary psychology can help. PCT has been tested in only "limited circumstances" (Marken, 940606), where it has not been tested improvements can be articulated and empirical tests constructed (hopefully).

Finally, in reference to Vallacher and Wegner, apparently Bill P. thinks they are using PCT, just not acknowledging it. So Marken, when you say

> Yes. It looks very interesting. Let's discuss the Vallacher & Wegner model, by all means. But, before we get started, I've got to know: if their model produces no quantitative results, if it predicts average behavior over subjects or over trials, if it doesn't work at all, do I still have to like it and not criticize it in order to have a "healthy debate'? If so, then we can save a lot of time since I can give you my evaluation...

This is what I mean by unhealthy debate.

Some minor issues:

I was largely responding to Dag's post when I warned of solipsism. That post was meant for those outside the net. So my comments regarding presentation were legitimate. I did, however, appreciate the distinction between messages on the net and messages to the outside world. It is call impression management and the research in it has some interesting points for PCT to consider (although they do seem to miss the point that one can only attempt to manage the perceptions of impressions).

Some mentioned I should send my recent paper to them and others for comment. I appreciate that, copies are on their way. One comment: The paper argues for the adoption of a PCT-like model as a paradigm for organizational behavior. The impetus for the argument came from a recent article in my field calling for a paradigm. Just before I finished my article a third article appeared condemning the paradigm approach because it lead to elitism and gate-keeping. This is a problematic argument because we have elitism and gate-keeping now, as you know. I think that once PCT come to it's own (and it looks like I am betting my career that it will), the new gate-keepers will be you people. Now do you understand my fear regarding solipsism as it appears on the net.

Regarding the non-optimizing reference signal. I realize now that it was not a well-thought-out question. Nor is it worth considering further, I can deal with it. Thank you Bill P. for at least trying to address the question. Marken, your comment was very helpful, NOT! :-)

Regarding my "primary reference signal." (promoting interdisciplinary collaboration). Bill P. (940607.0200) took me too literally. Promoting interdisciplinary collaboration is an espoused, higher-order, professional, self-concept, reference signal (EHOPSCRS). Why I really do what I do is beyond me (although attempting to find out is important to me). My EHOPSCRS identifies a niche that I hope to convince others needs filling and that I can help fill. And yes Bill, cross-discipline collaboration may be easier than within, as my religion analogy was attempting to say. It may be harder as the modeling issue shows (social sciences does not use the methods of physical sciences).

I do need to get the demo's and simulations. I have Marken's Lotus program, which was when I found out about CSG_net (I also heard about it from Lord). I did not understand it then and I don't think it worked when I tried it since. (Rick, you might remember, I am the one who had Quattro Pro, it worked fine on that version, but I have since updated and I think that may be the problem).

Prologue:

There are some very ridiculous criticism to PCT out there. Clearly, the behavior we observe among the dominant coalition is based somewhat on the error they anticipate when their life's work is rendered as superfluous by PCT (e.g., Locke). Another is the mistaken implications associated with the word "control." I think its tendency to be overplayed and the connotation it evokes has caused many to misunderstand PCT, which has evoked a strong counter response from PCTers that is also misinterpreted (for example, I think I have mis-interpreted your social control posts). But the negative PR factor is a real problem. I cannot see the average citizen accepting the theory as they have accepted the general theory of relativity simply because of the name. Call this superficial, but I now refer to my work a part of living systems theory (of course, it is not core PCT, so you probably appreciate that - although I do cite PCT references often).

In conclusion, there are many roles to be played here. To evoke the religious analogy again (understanding that science seeks data more emphatically than religion, but it is so appropriate). The high priests will attempt to maintain the purity of their belief system, as well they should. The clergy will attempt to bring the message to the people, even if the message loses some purity by incorporating local beliefs. But eventually, some of those local beliefs bubble up to the high priests, who, seeing their merit, incorporate them into their the religion's belief system. Conflict and acrimony will mark the way, but such is the nature of social interaction. Shall we get on with it?

Later, Jeff

P.S. It was Cliff Joslyn who made many of the same arguments I am making, only he is in ST/Cyb and I am in psychology. Also, he is more literate than I in your methods. Cliff, if you are out there, I think we are kindred spirits.

Date: Mon Jun 13, 1994 1:27 pm PST
Subj: Re: Responding to Welcomes

[From Rick Marken (940613.1030)] Jeff Vancouver (940613)

> Forgive my ignorance, but I think Tom Bourbon and Bill Powers think that the difference between self-regulation models and PCT is that PCT is about controlling perceptions and self-regulation is about controlling behavior. This is no doubt because the promoters of many self-regulation models _say_ they are about regulating behavior.

Yes, partly. But, for me, the main problem is that their research shows no evidence that they understand "control of perception"; there is no testing for controlled variables -- or anything like it. Self-regulation promoters have described PCT-like models pretty well; but they don't evidence any understanding of these models in terms of how they do their research. The proof is simply in the pudding; self-regulation research is NOT PCT pudding.

If all you care about is whether the theory described by the self-regulation people looks (in diagrams) and sounds (in words) like PCT then I will concede that self- regulation theories are very similar to PCT. As I recall, the first two chapters in the Carver/Scheier "Self-regulation" book give an excellent description of PCT -- really good. It's only by reading the rest of the book -- the research "based on the theory" -- that you can see that they didn't "get it" at all. Actually, the Carver/Scheier book is a good example of how people can "talk the talk" of PCT with rather remarkable fidelity and still not be able to "walk the walk" -- ie. do PCT.

Again, I think the basic problem is that people can describe the theory pretty well; they are just not aware of the phenomenon that they theory explains; the phenomenon of control. I think people really have to experience the phenomenon up close and personal, in all its manifestations -- from controlling the position of your hand to controlling where you worship god - - to really "get" PCT. Anyone can give a good account of the model, but it's a pretty empty exercise unless you know what in the world the model is there for; and it's there for explaining something that's all around you all the time -- control.

> Marken is applying his criteria for a theory on the other's theory. I do not condemn the criteria, merely the blind application. If empirical data is always required, Einstein's GTR would never have lasted the 20 (?) years before it could be tested.

But SOME empirical data is required or you have no basis for evaluating a theory at all. I think Einstein's theory was constrained by one hell of a lot of data.

> The problem with Marken's approach is that they don't buy/understand the data you use.

What would you suggest that I do?

> The rigorous requirements, central to your understanding of science, are seen as too rigorous for a science in it's infancy.

I know. I've heard people say that research like that done by Tom, Bill and myself is trivial because we always get what we expect -- often to the third decimal place. A cognitive psychologist friend of mine (who thinks PCT is hogwash, by the way) thinks we could make a better impression if we would get results more like what psychologists are used to -- the kinds where you really need a statistical test to find out what happened. I'll leave it to Tom Bourbon to comment on this (if he's around and wants to), since the guy who said this is one of my dearest friends; we obviously don't talk about PCT much.

> These are self-regulation theorists [studying higher level variables], and like I said above, I don't care what they think they are doing, I find what they are doing useful and interesting and I think some of you should too.

Great. Tell us what they are doing and how they do it. It's hard for me to get access to all this stuff. I think the net would be a great place to discuss it. Believe me, if these people have some good hypotheses about higher level controlled variables and some good ways to test those hypotheses we will be interested -- VERY interested!

> Marken (940606) says you have rejected social control...I find this very problematic. Marken (same post) says you have rejected "information about the cause of variation in perception."... This I find very problematic. Now I even find that the DME is not part of PCT...This is VERY problematic.

Well, I think you misunderstand what is meant by each of these terms.

1. We reject "social control" in the sense that we reject the idea that there is an entity (called "society") that is a control system exerting control over groups of individuals. Individuals control and when this occurs in interaction with other individuals, this is "social control".

2. There is no information about the disturbance to a controlled perception in the variance of the controlled perception itself because this variance 1) is a simultaneous result of both disturbance(s) and output and 2) there are an indetermine number of variables that may be a disturbance to the perceptual variable. This is an important point because it rules out models of control that view the controlling system as one that calculates the appropriate "disturbance canceling" output based on perceptual input.

3. The DME is simply an open question; there are no data suggesting its necessity, beyond what can already be handled by mechanisms suggested in BCP. So it's not currently part of PCT but it certainly could become part of PCT were this demanded by the data.

> Reorganization is not simply random.

This may or may not be true. This is why data is important; if you want to convince me that this is true then you have to present me with data that support this contention. If the data is convincing, I will certainly accept this claim. The current model of the reorganizing system is random, not because data demand that that be the case, but for logical reasons; ie. how do you know how to change control parameters when things go wrong? There may indeed be biases in reorganization; but this is something to be determined by experiment, not reason.

> Finally, in reference to Vallacher and Wegner, apparently Bill P. thinks they are using PCT, just not acknowledging it. So Marken, when you say

>> Yes. It looks very interesting. Let's discuss the Vallacher & Wegner model, by all means. But, before we get started, I've got to know: if their model produces no quantitative results, if it predicts average behavior over subjects or over trials, if it doesn't work at all, do I still have to like it and not criticize it in order to have a "healthy debate'? If so, then we can save a lot of time since I can give you my evaluation...

> This is what I mean by unhealthy debate.

And it's what I mean by "no debate at all". Why don't we just discuss Vallacher & Wegner; if the debate get's unhealthy, we can consult a physician ;-)

> I do need to get the demo's and simulations. I have Marken's Lotus program, which was when I found out about CSG_net (I also heard about it from Lord). I did not understand it then and I don't think it worked when I tried it since.

Get Dag's demo disk!!! The spreadsheet model comes with it's own 1.2.3 compatible spreadsheet program. Everything on Dag's disk works. If you don't have this disk, GET IT. No wonder you think that conventional psychology still has something worthwhile to say ;-)

Get thee to Dag's Diskery! Best Rick

Date: Mon Jun 13, 1994 1:39 pm PST
Subj: Re: Responding to welcomes - Jeff

[Martin Taylor 940613 14:40]
>Jeff Vancouver, (apparently Mon, 13 Jun 1994 11:05:50 -0400)

A very nice posting, that should cause substantial disturbance some readers. There are a few things with which I might disagree, but they are lost in the wealth of comments with which I do agree.

One main disagreement:

> Now I even find that the DME is not part of PCT. You mean that all conscience thought is irrelevant?

To my mind, these two sentences are unrelated. The question does not follow from the statement at all, so far as I can see.

Consciousness is not considered, one way or the other, in what I have called "core," "classical," or "conventional" PCT. But several people, including Bill Powers, Bob Clark, and I, have speculated about it. I side with the Powers' in saying that the DME seems to require a mechanism that is essentially a duplicate control hierarchy, and therefore an indefinite recursion of explanation; for what is it that allows the DME to choose what decisions it is to make? A DDME? Mind you, Bob Clark has many times argued that this is a wrong understanding of the DME, but I have not yet properly understood his position.

For me, decisions seem to be a property of the program level (which I'm not so sure can be identified with the perception of conditional relationships, despite my acceptance of that idea when Marken posted it). If so, they are unrelated to consciousness except as a side effect of what I speculate consciousness "really ;-)" is--a state of possible switching among which perceptual signals are to be controlled. The perceptions that are, and the perceptions that may be, controlled are in consciousness. None else.

Anyway, to reject the DME is not to reject the notion of consciousness.

Another point:

> Reorganization is not simply random. A model that shows that it could be (e.g., E. coli, which some of you have contented) does not make it so. But I think I must be misunderstanding you here, too.

I think you are. Last year I presented a taxonomy of possible loci of learning in a control hierarchy. If I remember, there were 12 kinds. Some involve topologically continuous neighborhoods, in which gradient search is possible (and thus not necessarily random), and some involve spaces without topological continuity (and thus necessarily random). For example, in its simplest possible form, the perceptual-sensory connections in a control hierarchy are exactly those of a multilayer perceptron (perhaps a TDNN), and ANY learning algorithm that applies to an MLP or TDNN would apply to learning what to perceive. That's non-random, usually.

But the main point of control is CONTROL, and failure to control means that perceptual signals grow when they should shrink, and vice-versa. To change this is to change the sign of the loop gain, a discontinuous step. And to change the sign of any one link might well throw other loops out of control that use the same link. There's no way for the hierarchy to know which sets of link signs lead to stability with the present condition of the real outer world, and when the outer world changes, the appropriate sets of signs may change. That's part of the job of higher-level control systems--they have perceptual signals that include conditional aspects of the outer world (see for example J.G.Taylor's experiments in the 1950's and 60's on conditioning to inverting and other distorting spectacles--there's a spectacular (!) movie of Seymour Papert falling many times off a bicycle that illustrates that development of higher control systems). Changes have to be random, though the rough location in the network where they occur may well be non-random.

Some may think that PCT is a religion requiring high priests (or offering the opportunity for them to take their places). I don't think that's a useful view. It's a useful way of looking at the living world. It may even be true. But then, so may some of our "laws" of physics. Nobody will ever know. Meanwhile (i.e. forever), we use the best tools we have, as they are suited for the problems that interest us. For much of psychology, PCT is the best tool I know. Which is why I spend a lot of my limited effort trying to do what I can with it and for it.

Martin

Date: Tue Jun 14, 1994 3:32 pm PST
Subj: Re: Jeff, on reorganization

From Tom Bourbon [940614.1658]

Reply to some of the topics addressed by Jeff Vancouver: 13 June 1994

> I am pleased that my introduction got such a response. Being ignored would have been the ultimate insult.

There was never any danger of your being ignored here! Judging from the replies to your initial questions and remarks, your self-introduction seemed to be welcomed by everyone.

> I did not feel unduly rebuffed, but, of course, I will respond back. My response centers around themes, not necessarily individuals. Specifically, I consider the following issues: self- regulation, modeling, some theorists in my field, and some minor issues:

> Self-regulation:

> Forgive my ignorance, but I think Tom Bourbon and Bill Powers think that the difference between self-regulation models and PCT is that PCT is about controlling perceptions and self-regulation is about controlling behavior.

Yes, I do think that is a big difference (not the only one) between self-regulation models and PCT.

> This is no doubt because the promoters of many self-regulation models _say_ they are about regulating behavior.

Yes, that is one of the reasons I think their ideas are different from ours: they say as much.

> However, if the model incorporates a test of the difference between a perceived state and a desired state, the result of which drives behavior, then the model is describing the control of perceptions.

Not if a "modeler" says her or his model is about how people control their own behavior. If that is what a modeler says, then the odds are immense that sooner or later in the paper he or she will make serious mistakes when they say more about control. I do not allow myself to divorce an author's _diagram_ of a control system from what he or she _says_ about the model. I have learned that I must look beyond the often accurate depiction of a PCT model at the beginning of a paper on self-regulation, to see what the author(s) say in the middle of the paper and at the end. In a remarkable number of cases, a promising beginning collapses into a recounting of traditional control-of-behavior theories, with a bit of control theory terminology grafted on. (I do not say this is true of _all_ cases, and I reiterate my eagerness to see any examples to the contrary that you might provide.)

> If the modeler says the model describes how individuals control behavior, they either don't realize their error or they are trying to reach a certain audience.

Then in the first case, they do not understand the control of perception and they are not talking about it; in the second, they are giving their audience an incorrect interpretation of the control of perception that will be difficult or impossible to correct, later on.

> But the model still describes the control of perceptions.

I am not as generous as you on this count. In my perhaps prejudiced book, if a modeler doesn't _talk_ about behavior as the control of perception (whether the "modeler" does that as a strategy, or out of ignorance), then the modeler's model isn't about behavior as the control of perception.

> D. Ford's Living Systems Framework is that type of model. I am still trying to get a handle on the consequences of the error for those models. So, to answer Marken's (940606.1800) post, yes there are others that describe the CSG loop.

Do they call it the "CSG loop" -- the PCT loop? Do they say that behavior is the purposive control of perception and that the behaviors that control perception are themselves unintended and uncontrolled? Or are those things you (and I) _would like to see them say?_

> Modelling:

> A major theme in the responses to my introduction was the role of modelling as a test of PCT and the other models I mentioned (indirectly through proponents - e.g., Locke, Bandura). . . . But the previous argument is mostly on an intellectually level. The reality of rejection letters and lack of collaborative spirit from the powers-that-be is painful, dispiriting, and financially and occupationally challenging. I apologize for raising any of those feelings.

No need to apologize! It just happens that, whenever someone says PCT modelers are out of line to reject psychology, PST modelers are _very_ likely to reply that the rejection seems to run the other way.

> I have numerous colleagues with less than kind rejection letters based on the same reasons that many of you alluded to. On the other hand, I know others who have had little trouble on that score. Of course, their work is probably not completely in line with the "core." Most notably, they do not require a working model as a requirement for their work (there is also an emerging appreciation for which journals are "friendly" and which are not).

Fine. Not everyone _does_ modeling; but do the people to whom you refer know about the modeling that _is_ done? Do they try to incorporate it into their writing, perhaps as empirical verification of theoretical points they try to develop in their writing? Are they careful to see that what they say in their theorizing is consistent with what has been learned form the modeling?

> Herein lies one of my concerns with the solipsism among the netters. Marken (940606) says "PCT is tested by comparing the performance of working models to actual behavior. In order to compare alternative theories to PCT we often have to translate verbal descriptions into working models." This requirement confounds the test of a theory with the theory.

Say what? I really don't follow you here. How else are we to test whether the core causal assumptions in another theory (as best we can understand those assumptions, which are often left unanalyzed by advocates of the other theories) can really behave as their advocates claim they can?

> Marken is applying his criteria for a theory on the other's theory.

Of course he is. And so is Bill Powers. And so am I. If someone tells us theory X explains behavior Y, but does not tell us how or why that should be so, and especially if the someone in question does not demonstrate that the assertion is warranted, then are we merely to say, "OK. Whatever you say os right. Sorry to have asked for evidence that your theory does what you say it does?" And if the person goes further to say that PCT cannot do any of the things we say it does, are we to acquiesce, saying "Of course. We were wrong to apply our criteria to your theory. Go right ahead and apply your criteria to ours -- reject us by nothing more than assertion, rather than by a demonstration that PCT does not work." I'm sorry. Jeff, but that is not my understanding of how science us done -- or at least how it _ought to be done_. That's one of the (many) things that I like about doing PCT science -- I am _required_ to show that _every_ claim I make can be backed up in demonstration -- in modeling and simulation. No endless flights of puffery and bald-faced assertion allowed.

> I do not condemn the criteria, merely the blind application. If empirical data is always required, Einstein's GTR would never have lasted the 20 (?) years before it could be tested.

You lost me here. Shouldn't we expect people at least to show why they think the lineal cause-effect model lurking inside their theory does work, after all? Or shouldn't we expect them to tell us why control of behavior, rather than control of perception, is the best way to explain behavior? If they don't pass muster on basic questions like those two, there is no need for those of us trying to understand and model perceptual control to look at the rest of the other theory, no matter how sophisticated and inspiring it might seem on other counts.

> Now I realize that these mini-theories are not GTR. Indeed, many are very problematic. But I try to separate the chafe from the wheat. Carver & Scheier's self-awareness construct was clearly wanting, but they exposed many to control mechanisms that had not known them before.

Yes. And some of those so introduced have progressed beyond the Carver and Scheier stage. But most have not. Are the many in the latter class any better off for the experience, so far as their understanding of perceptual control? Is PCT any better off for the presence of people in that latter group on the review panels of journals?

> . . . No doubt there are errors in the views perpetuated by the non-PCT models, but it is a step.

Some steps along some paths are demonstrably wrong -- at least they are if the parties accept demonstrations as evidence.

> Further, theories like Carver & Scheier's deal with many social processes which PCT does not deal with as thoroughly. Identifying the discrepancies between these models and PCT, and developing empirical tests the results of which both parties can take as evidence one way or another is, I believe, a reasonable next step.

> And let me be perfectly clear, I think sometimes they have a point and PCT needs to incorporate it.

But up to now the efforts at "bridging" these gaps have run in one direction. Bill Powers told you if his experience when he tried, many times over many years, to "reach out" to people who allegedly already occupy the much prized "higher ground" of higher-level processes. With very few exceptions, they ignored him. That, or, like Bandura, they unleashed a special kind of wrath upon him in print. I have had similar experiences, on a much smaller scale, with people like Carver and Lord, who are often cited as examples of theorists who "really understand PCT," no matter what they actually say in print.

> The problem with Marken's approach is that they don't buy/understand the data you use. Most don't understand modeling and/or don't trust its results. The rigorous requirements, central to your understanding of science, are seen as too rigorous for a science in it's infancy.

What "science," in which "infancy?" Certainly not psychology, the experimental study of which is as old as science itself, in the West. The old dodge that "psychology is too new as a science to have any real laws or theories" won't wash.

And which problem with "Marken's approach?" Have you noticed any others on this net who espouse similar criteria and a similar approach? ;-))

Now that I have my "E. coli style" models or adaptive control working again, I'll say a few things about the section of your post on reorganization and reference signals, but I'll do that tomorrow.

Later, Tom

Date: Wed Jun 15, 1994 6:16 pm PST
Subj: Re: Responding to welcomes - Jeff

Tom Bourbon [940615.1322]

Back to your reply to our replies to your self-introduction. In particular, back to some of your comments related to what you called "gaps" and to reorganization.

> Gaps & theorists in my field

> . . . How are actions chosen and how does reorganization proceed? I see stabs at the first question on this net (biological needs) but another type of intrinsic signal is probably system efficiency variables. The recent discussion of uncertainty is one such variable. You are absolutely right to attempt to figure out how the organism could perceive uncertainty and reject it if it can't (but beware, just cause you cannot figure it out does not mean it doesn't exist). The point is that another perspective has inform the guess that uncertainty is a reference signal.

Concerning perceptions of "uncertainty." In light of recent discussions on the net, it would be inappropriate for _us_ (PCT modelers) to try to figure out how the organism could perceive uncertainty; to do that would be for us to presuppose that the organism _does_ perceive uncertainty and that we should test _our_ understanding of what _others_ mean when they say organisms perceive it. I believe the process should work differently. Those who say organisms perceive and control uncertainty bear the burden of performing The Test For The Controlled Variable, then showing us that, yes indeed, organisms control uncertainty. Were _we_ to do the testing, then we would be subject to the oft heard criticism recently leveled by Bill Leach (to whom I replied yesterday) that we are guilty of constructing straw versions of the ideas and models proposed by other people, then shooting them down unfairly. No more of that for me. From no on, I expect people who propose new and different "models" and "controlled variables" to do the dirty work of identifying, quantifying, testing, and modeling. Then if their models and variables fail, the credit falls where it belongs, not on us. And if the proposers are not willing to go to that trouble, they can go talk to someone else. (I've finally learned to stop testing other people's ideas.) Up to now, I have not seen "another perspective inform a guess that uncertainty is a reference signal," but I _have_ seen an _assertion_, by advocates of another position, that it is. I'm still waiting to see them perform "the test" and the modeling.

> My reading of your literature and posts is that the identification of higher- order reference signals is truly a gap (although I fear I misunderstand the posts here). Many in psychology are seeking to study candidates for reference signal, including Little, Deci & Ryan, Emmons, Pervin (See edited books by Pervins, 1989, _Goal Concepts in Personality and Social Psychology_, and Ford & Ford's, 1987, _Human's as Self-Constructing Living Systems.). These are self-regulation theorists, and like I said above, I don't care what they think they are doing, I find what they are doing useful and interesting and I think some of you should too.

But I _don't_ find their work useful and interesting, which in no way implies that _you_ should not or could not. I've tried to take it seriously, as an "applied" version of PCT, but I can't. For one thing, I don't see them studying "candidates for reference signals." Rather, I see them performing research in the tradition of lineal cause-effect, in order to test the null hypothesis that there is no relationship between two sets of scores, one of which (they assert) is a measure of "self efficacy" or "self acceptance" or some other "self" construct. I've never seen the self-regulation people talk about and test for the presence of controlled variables, a test that, if successful, might support an assumption that reference signals are at work. On the other hand, while reading their work I've _often_ found myself thinking, "If only they had tested for controlled variables. If only they had not abandoned the PCT model after the first few pages of the article." And so on.

> To the third question, dealing with reorganization (and other matters like stress and leadership) are people in my field. Proponents of PCT-like models (forgive them their transgressions), including Tsui & Ashford (e.g., Journal of Management, 1994), Edwards (e.g., Academy of Management Review, 1992), Cropanzano, James, & Cetera (Research in Organizational Behavior, 1993), Lord & Levy (Applied Psychology: An international review, 1994) and Manz (Academy of Management Review, 1986) to name a few. Other interesting thinkers include the Germany action theorists (Kuhl, Hechausen) and others (Vallacher & Wegner). Many or most of these authors or theories may have already been read and rejected. I do not agree with everything _any_ of them say. But herein lies the challenge: finding which loops conflict, proposing alternatives, and testing the them.

Purportedly, they and we are all talking about the phenomenon of control. Incontrovertibly, we have a model of control that works, but they do not. Who should look more closely at whose work? (I don't say this to assert a kind of superiority or pureness, but to show you how I think science _should_ work. I'm describing my system-level preferences and intentions for how science is done. I learned some time ago that my system-level ideas about science are out of whack. :-))

> Marken (940606) says you have rejected social control. Well, I am not exactly sure what is meant by social control, but my impression from earlier threads (and B:CP) is that you rejected social influence. I find this very problematic.

Indeed, that would be problematic; but no PCT modeler has ever rejected the idea that one person's actions can affect variables controlled by another and that, consequently, the results of one person's actions can act as disturbances the effects of which are canceled by the actions of a second person. We _have_ rejected the idea that a "society" is an agent -- a control system that acts purposively to control the actions of a given individual person. . . .

> You mean that all conscience thought is irrelevant? This is VERY problematic. Reorganization is not simply random. A model that shows that it could be (e.g., E. coli, which some of you have contented) does not make it so. But I think I must be misunderstanding you here, too. Planning and thinking is part of PCT. Figuring out how that planning and thinking translates into choosing and doing is where contemporary psychology can help. PCT has been tested in only "limited circumstances" (Marken, 940606), where it has not been tested improvements can be articulated and empirical tests constructed (hopefully).

Rick already addressed your concerns that over the idea that PCT (that is to say, PCT modelers) says conscious thought is irrelevant. I second his remarks to you, in which he said there was no need to worry about that point. I do want to say a few things about your remarks on reorganization.

You have _asserted_ that reorganization "is not simply random." Fine. You can assert anything you wish, but I believe you might have misread our discussions about reorganization. Never did any PCT modeler say that reorganization is simply random. I believe we have said that, for a human who is functioning more or less normally, when present strategies fail to produce the intended results, the person may engage in a formal program-level search for alternative strategies. The person, for example, might follow a widely-known (perhaps commercially promoted) technique to "brain storm" for new strategies, or might rely on techniques and procedures learned through personal experiences. (When I use words like "procedures, programs, and strategies," I refer to logic-level "actions" that occur when the person attempts to control perceptions at other levels; not to programmed actions.) These program-level attempts at control often include formal "if-then" branching decision trees.

Before program-level control is established (in general, or for a particular perception), or when that level of control fails, or when an instance of perceptual control simply is not programmatic, what do we do? And how do we account for changes in the control behavior of creatures that show no evidence of a program level? Those are some of the instances in which we have suggested that a random ("E. coli") process of reorganization might occur. Further, we have developed several ways to model such a process and have shown, in simulations of our models, that "ecoli reorganization" can work in a number of applications. From that start, we are willing to speculate (not assert as an established fact) that the mechanisms modeled in ecoli reorganization might serve as an explanation for reorganization in many other circumstances. As always, those speculations are subject to confirmation, or to rejection, whenever they are put to the test.

(In some of my recent modeling of various interactions between two control systems [which can mean between two people], I use an "ecoli reorganization procedure" in the model for one of the systems. The modeling is difficult to describe in a few words -- I'll demonstrate it in Wales next week and write about it in Martin Taylor's issue of IJMMS. I use the ecoli procedure to make one of the interacting models "adaptive," in that it randomly -- and I assume "unconsciously" -- changes its own gain, or its own reference signal, or the sign of its own feedback loop -- all as part of its "attempt" to control of its perceptions of the other system.)

We do not contend that ecoli reorganization explains all changes in control behavior. As you say, the uttering of such contentions would not in itself make them so; that is one reason we never utter them. But it is also true that ecoli reorganization _does_ work in the instances where we have shown it to work -- a fact that will not go away just because some people assert that it should. (Not that _you_ are making that assertion now, but assertions very much like it have come up in the past.)

> Some mentioned I should send my recent paper to them and others for comment. I appreciate that, copies are on their way. One comment: The paper argues for the adoption of a PCT-like model as a paradigm for organizational behavior.

If I was not on the original list to receive a copy, please add my name. I'd like to see it. :-) . . .

> In conclusion, there are many roles to be played here. To evoke the religious analogy again (understanding that science seeks data more emphatically than religion, but it is so appropriate). The high priests will attempt to maintain the purity of their belief system, as well they should. The clergy will attempt to bring the message to the people, even if the message loses some purity by incorporating local beliefs. But eventually, some of those local beliefs bubble up to the high priests, who, seeing their merit, incorporate them into their the religion's belief system.

Ouch! I know you are using the analogy to religion in the loose sense, but whenever I see it applied to PCT, I cringe. It too easily supports the idea that PCT modelers are "high priests" defending a belief-based "faith," with a social hierarchy trailing away beneath them -- a hierarchy of clergy, acolytes, unwashed believers and (I suppose) heathens, heretics, and other rotten types. I don't like that image at all! I'd rather see _everyone_ jump in over their heads and play the game of PCT science for all it's worth.

> Conflict and acrimony will mark the way, but such is the nature of social interaction. Shall we get on with it?

Come on in! I think there will be far less conflict and acrimony that you expect. :-)

Later, Tom

Date: Wed Jun 22, 1994 6:09 pm PST
Subj: self-regulation

[From Jeff Vancouver 940622)] Tom Bourbon 940614.1658

Suspecting that I am one of these self-regulation people, it is important that I understand what discrepancies that model creates for you all (or what discrepancies PCT creates for me). I should preface this with my understanding of the work of others. No one has it right. They are all wrong. You are wrong. I am wrong. Everybody is wrong. Given that, I usually try to find what is useful to me. Useful in filling a gap in my understanding or useful in provided a testable hypothesis I can get a pub from, etc.

Your (Bourbon 940615) lack in interest in uncertainty seems to be like my lack in interest in some of the issues of interest to some PCTers. We cannot be interested in all of it, it is too much (do you buy that). But I thought identifying reference signals was a primary concern. One of the first steps, if not the first, is to guess at a possible reference signal. Do you guess randomly? Using other's ideas might help to make an educated guess. That is what I thought the uncertainty guess was about.

Now, onto self-regulation. I get the impression that PCT likes to think of actions as unintended. I certainly think they can be. But I also think they can be intended as well. That does not mean intent equals success. Now one thing I had noticed among some self-reg people is an exclusive interest in intended behaviors. I see this as a boundary of their theories. Some, I think are aware that this is a boundary. Others are oblivious to the limits of their model. Attempting to point out the boundary gives me something to do, so I don't mind too much (I have not gotten rejection letters for PCT-like ideas yet). But are you, Tom, telling me that PCT does not included intended behaviors in its model? I know the net well enough to know that there are no behaviors beyond PCT's description, so it must be the intentions that disturb. Why do intentions disturb?

Do the people I mentioned and know use or know of PCT modeling? I cannot answer for all of them, but I think many do not. Gosh, I have been reading this stuff for a long time and can't distinguish what I know from the modeling from what I know from the verbal descriptions of the theory.

I referred to D. Ford's living systems theory (LST) as very close to PCT. You asked if he defines behavior as the control of perception. Hard to tell because he calls perception a kind of behavior. You ask if he calls his loop the PCT loop or the CSG loop. No, he does not seem to like your jargon (so it goes), but judging by the references, he gives Powers some credit (I am still reading it). In fact, M. Ford's book using LST to discuss motivation gives Powers (of course, he lumps in Lord and Carver & Scheier and other) a lot of credit (I did read that one). M. Ford's complaint is the jargon and the lack of coverage of emotion (they probably don't know that Powers' chapter on emotions was cut). When Bill gets back we will have to ask if the Ford's are one of those who ignored him.

You say (Bourbon 940615) that you have a model that works and those other do not. I would contend that you have a model that works in a limited context. What is going on at the program levels and above seems much more speculative. These others have equally speculative models. You simply don't appreciate their method of testing their models. I think that is largely because you have different aims than them. What remains a question is whether there is any overlap. Given that my aims are much more like theirs (establishing a career by showing something useful to the world) than yours (developing a model of human behavior), and I am paying some attention to your work, I must believe there is an overlap.

I guess my interest in this net is to learn more and to help others learn more. The learning is to improve the designs of our experiments and models. I am particularly interested in identifying the gaps in understanding, whether those gaps are within PCT or between PCT and other models (by this last I mean mostly to get the others up to speed. I am outlining a continuation of Bill's blunder's paper, so their problems with the control of behavior and other gaps interest me).

Later, Jeff

Date: Wed Jun 29, 1994 5:20 pm PST
Subj: Re: self-regulation

From Tom Bourbon [940629.1222] >[From Jeff Vancouver 940622)]

> Suspecting that I am one of these self-regulation people, it is important that I understand what discrepancies that model creates for you all (or what discrepancies PCT creates for me). I should preface this with my understanding of the work of others. No one has it right. They are all wrong. You are wrong. I am wrong. Everybody is wrong.

OK, I get it. We are all wrong. ;-)

Now we can get down to the business of examining the performance of PCT models -- and the performance of other kinds of models, if the necessary information happens to be available. (One problem with my reading of the self-regulation literature is that nowhere in it have I ever seen a working model for self-regulation -- and I'm afraid I've lost all patience with the idea that any of us should accept a lot of talk about something as constituting a "model.")

I'm more interested in some of the specific things you say about PCT modeling, later in your post, but first a few remarks about your initial ideas.

> Your (Bourbon 940615) lack in interest in uncertainty seems to be like my lack in interest in some of the issues of interest to some PCTers. We cannot be interested in all of it, it is too much (do you buy that).

Maybe. Let's see if we are thinking about the same thing here. I buy the idea that if I decide to say I am interested in information theory, where "uncertainty" has (or once upon a time had) a precise technical meaning, I had best pay attention to (show an interest in?) the things information theorists say about uncertainty. If I am not interested in (or informed about) uncertainty, as _they_ use the term and its associated measures, perhaps I should hold back from telling them they don't know what they are talking about, or from asserting that it's all a matter of preferences and personal interests. On the other hand, if an information theorist asserts that informatic measures of uncertainty are foundational to the workings of perceptual control systems, then I buy the idea that I might be able to say something to inform the discussion.

Is that the kind of thing you meant?

> But I thought identifying reference signals was a primary concern. One of the first steps, if not the first, is to guess at a possible reference signal. Do you guess randomly? Using other's ideas might help to make an educated guess. That is what I thought the uncertainty guess was about.

I'm sorry, Jeff. I don't follow you here. I thought this thread began with some assertions that uncertainty can be perceived directly, hence controlled. I didn't think it had to do with anything as mundane as, for example, my "uncertainty" about (simply not knowing about) which reference signal a control system might be controlling. In fact, during all of the discussions on csg-l about information and uncertainty as they might relate to PCT, I've been a little concerned that the advocates of information theoretic terms were using them in the common sense manner, rather than the technical one. If all we are talking about is "not knowing which", rather than about quantified bits of uncertainty, then the discussion takes on a different form.

> Now, onto self-regulation.

Good! The real meat!

> I get the impression that PCT likes to think of actions as unintended.

Well, aside from the fact the PCT is a theory and as such it doesn't think, ;-) I think I agree -- under the right circumstances, such as when actions are not intended -- which is the case _every time_ the _results_ of actions are intended. If the _results_ are intended, the actions _cannot_ be -- no ifs ands or buts about it. That's not a matter of theory or conjecture or preference; it's just the way control of perception works.

> I certainly think they can be.

We agree on that.

> But I also think they can be intended as well. That does not mean intent equals success.

Of course they can be intended. And of course intent does not _necessarily_ equal success.

Not too long ago I wrote a paper about how a person can control his or her perception of his or her own actions. The paper was:

Mimicry, repetition, and perceptual control.

It was in the famous journal, _Closed Loop_, Fall 1993, Vol. 3, No. 4, pp. 55-71.

One point of the paper was that people can act to feel the movements of their hands matching the seen movements of a target. I called that condition "mimicry." That's a kind of "control of movement", isn't it? A person can do that. So can a simple PCT model of the person.

For me, an interesting thing about the results is that when a person or PCT model makes perceptions of self-movements match a seen movement, any other variables affected by the self-movements go "out of control" and the results look like those produced by a purely reflexological S-R system. Control your perceptions of your own actions, as in mimicry, and you lose control of your perceptions of other consequences of your actions; control your perceptions of other consequences of your actions, and you lose control of your perceptions of your own actions. In _either_ case, the person and model act to control their own perceptions; it's all a matter of _which_ perception(s) they intend to control.

In the same paper I showed how a person and a PCT model can control their perceptions of their own actions in another way. People can act to make their momentary felt hand movements match a remembered pattern of movements. A simple PCT model can duplicate the results. I called that condition "repetition." Isn't it another kind of control of actions? Incidentally, the results look like those when a purely "cognitive," plan-driven or command-driven system controls its own actions as commanded outputs; perceptions of the actions are controlled and perceptions of the consequences of those actions go out of control. As I said above in the discussion of mimicry:

"Control your perceptions of your own actions . . . and you lose control of your perceptions of other consequences of your actions; control your perceptions of other consequences of your actions, and you lose control of your perceptions of your own actions. In _either_ case, the person and model act to control their own perceptions; it's all a matter of _which_ perception(s) they intend to control."

> Now one thing I had noticed among some self-reg people is an exclusive interest in intended behaviors.

Then we see the same things in their literature. I hope you see why we (PCT modelers) have so often said negative things about the self-regulation pseudo-models we have seen; none of those word-models can serve as a model for living systems, unless, of course, it does not matter that a "self-regulating" organism has no control whatsoever over anything other than its own actions. If the consequences of actions matter, then no system that controls only its own actions, by whatever means it achieves that control, can survive.

> I see this as a boundary of their theories. Some, I think are aware that this is a boundary. Others are oblivious to the limits of their model. Attempting to point out the boundary gives me something to do, so I don't mind too much (I have not gotten rejection letters for PCT-like ideas yet).

Don't worry. You will get them, and soon, if you take on that task!

> But are you, Tom, telling me that PCT does not included intended behaviors in its model?

I wasn't telling you that (see the discussions above, about mimicry and repetition); maybe something about my earlier wording or style led you away from what I was trying to say.

> I know the net well enough to know that there are no behaviors beyond PCT's description, so it must be the intentions that disturb. Why do intentions disturb?

I don't understand this, Jeff. Can you tell me more about what you mean here?

> Do the people I mentioned and know use or know of PCT modeling? I cannot answer for all of them, but I think many do not.

I haven't seen one of them do it. Heck, how could they; we can't publish in their journals. The editors and reviewers keep saying, "We've never seen anything like this before and we don't want to see it now." :-)

> I referred to D. Ford's living systems theory (LST) as very close to PCT. You asked if he defines behavior as the control of perception. Hard to tell because he calls perception a kind of behavior.

His use of the terms might (or might not) be a sign of some common ideas about what is controlled and what does the controlling. I'd need to look.

> You ask if he calls his loop the PCT loop or the CSG loop. No, he does not seem to like your jargon (so it goes), . . .

Jargon is _always_ a problem. That's one reason some modelers are so big on using models to make their ideas less ambiguous, and then to test for whether the relationships and interactions expressed in those ideas really produce the controlled results they (the modelers) believe they will. PCT is not about the words and the jargon; it is about an empirically observable (not conjectured) phenomenon of control by living systems, and about a working (not conjectured) generative model that literally (not hypothetically) duplicates many examples of control by living systems.

Does Ford use working (behaving, generative) models to make his jargon less ambiguous and to test his ideas about self-control? Or does he simply offer his own jargon, which he understandably likes more than ours?

> but judging by the references, he gives Powers some credit (I am still reading it).

Jeff, _many_ people "give Powers credit" in the form of citing him. Some even go further and pretend to summarize or interpret Powers's ideas. Most of those people cite him out of context or inappropriately (once in a great while they cite another PCTer or two), and many who pretend to interpret Powers or PCT do it terribly and in a misleading way. (I can cite examples of all of these ways of "giving credit," but not right now.)

> In fact, M. Ford's book using LST to discuss motivation gives Powers (of course, he lumps in Lord and Carver & Scheier and other) a lot of credit (I did read that one). M. Ford's complaint is the jargon . . ..

The fact that Ford lumps Powers with Lord, and Carver and Scheier makes me even more suspicious of Ford's grasp of PCT -- I mean PCT as the science of perceptual control, not "PCT" as the idea that, somehow or other, people control themselves. In what way(s) do you see Ford "giving Powers a lot of credit?" Is it by citing Powers a lot, or maybe by saying," I give Powers a lot of credit for doing ...?"

> You say (Bourbon 940615) that you have a model that works and those other do not. I would contend that you have a model that works in a limited context.

I'll stand by the range of applications of the PCT model and I'll bet that it compares quite favorably with the range of any other _working_ model of behavior you can find on the market today. I'll go even further and make a statement I do not believe is at all rash: There is no other generative model of behavior that can be applied successfully in as many settings as the PCT model. Period. Prove me wrong and I'll admit it publicly, right here on csg-l.

> What is going on at the program levels and above seems much more speculative.

Hmm. My presentation in Wales was titled, "Program-level control of a sequence of relationships." The task and the accompanying model got to the program level and the model included three different levels of perceptual control. Kent McClelland talked about (but did not model) social cooperation in terms of the control of perceptions at the levels of systems, principles, and programs. There have been a few demonstrations of control of perceptions at the systems level. The degree to which our remarks about higher levels can be called "speculative" is shrinking, but not nearly so fast as it would were more people to stop saying, "you people haven't done much at the higher levels," and join in to help do the work. ;-) (That's a hint, and a plea for help.)

> These others have equally speculative models.

I disagree. Their models are far more speculative than the model in PCT. Their "models" don't even work at the _lowest_ levels, so they _cannot_ work for the higher levels, which _must_ depend on the lower ones. That's one of the biggest problems with their non-models -- they don't behave at all.

> You simply don't appreciate their method of testing their models.

Agreed. I do not appreciate their method of testing models. I am trying to help develop a science of behavior that can explain the actions of individuals. The requirement that a "model" need only "sound plausible" is far too lax to suit me. That's all they require -- that, and perhaps some statistically significant differences between means of groups, or some statistically significant correlations, either of which is useless for explaining or predicting how a particular individual will act and what will be the consequences of those actions. I do not appreciate those methods of testing "models."

> I think that is largely because you have different aims than them.

You are right. I want to help study a phenomenon that is seen everywhere in nature, not just in statistical abstractions, and to develop a generative model that works as an explanation for that ubiquitous phenomenon.

> What remains a question is whether there is any overlap. Given that my aims are much more like theirs (establishing a career by showing something useful to the world) than yours (developing a model of human behavior), and I am paying some attention to your work, I must believe there is an overlap.

Hey, the fact that I may soon be out of work again shouldn't enter into this discussion. ;-) I was doing PCT even when I was a legitimate working person with tenure (if _any_ person with tenure can be called legitimate). I want to understand and explain one of the things living systems do all of the time and very well -- control. I'm especially interested in the hierarchy of control and in interactions between independent control systems. Who ever said I didn't want to make a living at the same time? It just isn't working out that way, that's all.

As for what is useful to the world, I'll go way out there and assert that most of what psychology claims to have learned about behavior is not only useless, but is also dangerous to the world. No joke. :-(

Later, Tom

Date: Thu Jun 30, 1994 8:52 am PST
Subj: rightness, reorganization, self-regulation and alerting

Jeff Vancouver [940629]

on the "rightness" of PCT:
[Bill Leach 940622.23:59, Martin Taylor 940622, & Tom Bourbon 940629]

I am claiming PCT is wrong in the sense of Kuhn's paradigms and Socrates' "all knowledge is tentative." Eventually, assuming it gains favor, PCT will be _replaced_ by another paradigm because it is no longer _adequate_. By adequate I mean it no longer points to ways of closing or identifies new gaps between our model and our observations of reality. By replaced I mean it will either be completely tossed or incorporated into the new paradigm. My guess is the latter, but this is crystal ball stuff.

In the meantime, many gaps are articulated by PCT and much work needs to be done. You see, science is about identifying and filling gaps. So when Bill when you say:

> Unlike ALL other psychological work, PCT is NEVER a matter of opinion.

You are missing the point. We seek opinion, so that we can shoot is down with data or logic (models being one form of logic). That is what we do. A perfect theory (no gaps) would be useless to scientists (although very useful to engineers) because it would give them nothing to do. I have no fear of this. The question is not whether PCT is right or wrong for it is surely the latter, but whether it is the best model out there at this time. I think it is. Of course one's definition of best depends on one's constituency. For the engineer/applied scientist, best means it can help in designing controls or at least predict behaviors or events (i.e., it is likely to be useful). For the scientist, best is gap identification and filling (even more specifically, it allows one to produce a publishable/fundable program of research, where fundable often relates to the applied scientists' criteria).

I felt vindicated when Martin (940623) said to Bill L.:

> I see PCT as a framework theory, like Quantum Electrodynamics in physics.

Of course, Martin is likely to get jumped on for that comment (probably already has - I suffer a severe lag on the net).

The point of this is that some things PCT (or any theory) cannot and even will not handle. There is no reason to get too worked up about this. It is part of the game. ALL KNOWLEDGE IS TENTATIVE.

on reorganization:
[Martin Taylor 921005 and 940622]]

Thanks. Your posts on reorganization confirmed my understanding of the possible reorganization process. I agree when you say "this aspect [how reorganization works] is not securely developed." I understand there is a big difference between theoretically possible and empirically tested. I also think there is a big difference between theoretically possible and modeled. By theoretically possible I mean that nothing in the theory precludes the possibility. It does not mean the theory requires all the types you suggest (although it does require some minimum types). But until all this is worked out (i.e., modelled and tested against the real world), we can assume with caution all 12 (and maybe more) as possible. That is what I wanted to hear. (Of course the negative side of all this potential is trivalization. This is the neoFreudian problem - anything is possible within the theory and thus nothing is predictable or much less controllable.)

To my question [Jeff 940622] "Is the process of deciding a course of action, creating a new or using a different reference signal?" you said:

> The tenor of this question suggests the control of output, whereas PCT is based on the idea that what is controlled is perception...

Yes, I get that (see more below). But you go on to say:

> "deciding a course of action" can be rephrased as "deciding reference levels for lower-level perceptions" and when it is phrased like that, the answer becomes self-evident. There is no "new of different" references signal, but the values of existing reference signals are affected.

Apparently it is not as self-evident as you suggest. First, by "different" I meant different _values_ of existing reference signals. If there are different values for a reference signal, some method must exist for select the value that is sent. That something is presumably the effector function. Changes to that function is one of your 12 types of reorganization. Second, "new" means new ECS. Can't a new ECS be created as part of a conscious process, as exemplified by Bandura's modeling concept? Surely this is a reasonable question? I think this is what Tom means be the term "mimicry" (Bourbon 940629).

on self-regulation: [same people]

I suspect I am wasting my fingers here, but I think the main problem is that many netters have their gain set too high on this behavior thing. I see it in Bill P. writing from early on, which I think accounts for it. Specifically, Bill was fighting the behavioralists. It was a very important fight back then. What seems missing is the understanding that the fight was won. PCT was maybe only a small platoon in the war, but the war is over.

That is, contemporary psychology assumes we are talking about perceptions, not behavior. I realized this after my reaction to most posts I received back was "of course." When contemporary psychology says we attempt to control behavior, that is simply a shorthand for saying we attempt to control our perception of behavior. Most know we cannot "know" our behavior, merely our perception of the results of it. Usually, there is little difference, so like physics often ignores friction, psychology often ignores biases and mis-perceptions because they are of minor importance. On the other hand, some concerns themselves with the question of when biases and mis-perceptions are likely to be important.

You can call this imprecise use of the language, but one can also call it prudent and expedient. The norm on the net is to call it imprecise, so I will try to respect that lest I get more "modify your behavior" messages.

But there is one more thing before I leave behavior and self- regulation. The meaning and role of intentions. Bill L. and Martin refers to reference signals as intentions. This may be what others on the net think of as intentions. When I say intentions, I am referring to contemporary psychology's use of the word, which is useful for PCT as well. That use is as the phenomological experience of wanting to engage in behavior. (A PCTer might say they wish to change the perception of one of their behaviors, but since it is a phenomenological phenomenon, it is perfectly legitimate to say "wanting to control behavior"). Usually these intentions are as a response to a goal the person "wants" to accomplish. In this sense both intentions and goals are not reference signals. They may be fair representations of references signals, or they may not. Our conscious experience of our goals and intentions is probably only an inference of those reference signals. Some in the self-reg school don't make this distinction, many do.

But, the question is, how does this conscious processing interact with the perceptual control hierarchy. Most interesting is how the phenomonalogical experience of a goal and a desire to behave in some way to achieve that goal translates into PCT reference signals. Because most of the time, conscious desires are translated into behaviors, which affect the environmental variables as intended (except when the environmental variable is your children ;-)), which affects your perception of the environmental variable. Further, your beliefs and attributions about all that process affect subsequent goals and intentions. I think these are all legitimate phenomenon for psychologists to study and should be of interest to some PCTer as well.

Indeed, when Martin says:

> ...I control a perception of typing this message as part of the control of my intrinsic variable "blood sugar" (supposing that to be an intrinsic variable). How? I see typing this as a way of perceiving myself to be better informed on the subject I write about, and as a way of helping other people to be better informed ...

Part of what self-regulation psychologists are trying to do is understanding why typing, etc., would control blood sugar.

Meanwhile, psychologists need to get a better handle on the underlying mechanism of behavior and PCT provides a very good model for doing that. Many, seeing the limitation of a purely conscious model are beginning to focus on consciously unintended behavior (that is, unintended perception control). They are missing the framework (psychologists are paradigm shy - having been burned in the past).

You all will probably hate me for this. But I am beginning to see the modeling and PCT v. self-reg debate as a form of the reductionist argument. To use the computer analogy. Many in my field (I/O psychology) are interested in the higher-level software. They want to create macros and engage in other resource saving techniques. Those in the more higher-level mental processes on also interested in the higher-level language, but are more into the code. Those in the lower-mental processes are more into assembly and machine language. Neuro and bio people are more into the electronics that make it work, etc. etc.

Now when I was studying computer science in college, I took the usual sequence of programming courses and learned how to program. But I did not feel I understood how the computer worked. Then I took the computer architecture course. It covered the link between the hardware and the software. Ahhh, now it was making sense. I could explain things to my level of satisfaction. I new that if I could understand it even better if I took some electrical engineering and physics courses, but I was satisfied with my level of understanding. I did not feel the need to know at those levels. Now, I program some in basic and I am learning visual basic. My programming courses have helped the most in my learning, but the architecture course has helped as well (maybe more than I know). Nonetheless, I can focus my learning efforts on that higher-order level (learning the programming language), and not the architectural level.

PCT is like that architecture course. My experience was very similar - now I have a much better feel for how it all works. Their is still some fuzzy areas, but some are not as critical to "writing efficient code" of the type I would write as others. A large part of the trick is to figure out what one needs to know to proceed efficiently. For me, I feel knowing something of the architecture is helpful. Many do not, they may be right, they may be wrong. The problem is that they cannot judge because they do not even know that a model of it exists. But I digress. The point is that all the players, from the physics to the engineers to the various levels of programmers can work somewhat independently - not completely independently nor completely interdependently. Finding the mix is the source of the conflict here. Perhaps we (or someone) should model it, because currently it is all a matter of opinion.

on alerting experiment [Martin Taylor 940623]

Martin, you and I are much closer on this than you appear to think.

First, by ECS I assume you mean Effector, Comparator, Sensor.

Second, I said

>> But the real question is the phenomenon you are trying to examine. Perhaps you have addressed this in previous posts, but what I think of when you say "alerting" is the allocation of attentional (or some other) resources to some part of the loop.

You said.

> Quite the opposite. Firstly, the alerting perception is specifically NOT part of the loop of any controlled perception, just as the beep is not part of the perception of the line-location that is to be controlled. Secondly, the whole notion of alerting is to avoid the requirement of allocating attentional resources.

So I am correct that this is largely an attentional resource allocation experiment. I never meant to imply the alerting loop was part of the line-location loop. Indeed, my original fear is that the alerting loop might take attentional resources away from the line-location loop(s). Your main idea is that the beep will free up attentional and visual resources, thus making control of the line-location more efficient (understanding control efficiency is key to selling the PCT model to my constituency).

Your hypothesis assumes the system would seek (have a reference signal) to increase efficiency (otherwise the beeps would be ignored). I think that is a perfectly reasonable assumption. Although specifying it would help (and modeling it would be even better)

Problems that still trouble me:

1) the unmodeled efficiency ECS (which may be intrinsic)

2) What resources exactly are involved (attentional, visual). The problem here is their lack of independence, must less our understanding the was attentional resources means.

3) The changing focus problem I mentioned in my previous post. I still worry that, in the name of efficiency, one only addresses line-location when the beep sounds. This way, visual resources can focus on correcting and not searching. Attentional resources can be devoted to sound/column identification. This process could be identified if, after the subject performs for awhile in the complex condition, complexity is lowered by slowing the target movements. If I am correct, no attempt will be made to correct a line-location until a beep is heard, even if the previous deviants was corrected. In other words, the subject will be slow to return to a search mode even though resources required for correcting could be spared.

I few points I did not understand.

> There is no argument IN PRINCIPLE about the availability of degrees of freedom in the working of the brain ... If my guess is right about attention, that what is attended are those perceptions to which or from which control is to be given or taken, then the output df limit the number of attentional df that can usefully be deployed.

I got lost. Whose principle? PCT's? There are definitely theories which postulate limited attentional processes. I buy this concept. And I think it is critical to your experiment. You are focusing on output df, but the critical constraint in your experiment might be the limit to attentional processes.

When you say:

> This is what I called the "search mode;" a perception that "should be" controlled cannot be effectively controlled because of data lack. Other perceptions are controlled in such a way as to remedy that lack...

You are discussing the other ECSs that may take various resources. This resources may by physical (with the 125 df) and/or they may be attentional (like worrying). Your hypotheses is that providing an alert in the environment will obviate the need to search thus free more resources to reduce discrepancies. I am saying it may also obviate the need to worry. In fact, I think it is more likely to be the latter rather than the former, since the beep will still require a search for the offending column, although less so as tones become associated with columns.

This post told about four hours to compose, how do you people do it?

Tom, I will get back to your specific questions later. Later Jeff

Date: Thu Jun 30, 1994 3:50 pm PST
Subj: uncertainty, & self-regulation

[From Jeff Vancouver 940630] Tom Bourbon [940614]

I will try to keep this brief.

on uncertainty:

I fall somewhere in between information theorists and "not knowing which", but I am winging it somewhat. The formal meaning of uncertainty in IT is not knowable by humans, therefore they are not controlling it and "informatic measures of uncertainty" are not likely to be of much use when considering control. We are much more likely to control the mundane sense of uncertainty. But the mundane sense is merely a poor measure of the IT sense because that is all the better we can sense it. But don't pay attention to me here. I don't really know. Uncertainty was just an example of a possible reference signal I thought people on the net were interested in and that that interest had come from another theory.

on mimicry:

It sounds like your subjects could not control all the variables at once. Why do you suppose that is?

on modeling and psychology:

"Useless and dangerous" - that is a limb. I must get back to it, watch out.

Later Jeff

Date: Thu Jun 30, 1994 3:56 pm PST
Subject: Back in service; a few comments

[From Bill Powers (940630.1330 MDT)]

Jeff Vancouver (940629) --

> We seek opinion, so that we can shoot it down with data or logic (models being one form of logic). That is what we do. A perfect theory (no gaps) would be useless to scientists (although very useful to engineers) because it would give them nothing to do. I have no fear of this.

Neither do I fear for science running out of something to do. On the other hand, I wouldn't avoid making a theory as good as possible just to keep engineers from taking over science. I think we have to work as if we're trying to perfect theories, especially by subjecting them to most direct challenges we can devise -- to do otherwise is simply to encourage sloppiness.

PCT is indeed wrong in the terms you state -- but the catch is that we don't know where it's wrong. It has survived all the experimental challenges we have thrown at it lately, although of course it got into its present shape by failing a lot of experimental challenges first. I don't worry about perfectibility anyway. If we think of a test that the theory can't handle, we'll just get a better theory out of it. I'm not too worried about a total catastrophic failure at this point. Even Newtonian physics still works perfectly well in its own little niche (most ordinary affairs, including space travel).

> That is, contemporary psychology assumes we are talking about perceptions, not behavior. I realized this after my reaction to most posts I received back was "of course." When contemporary psychology says we attempt to control behavior, that is simply a shorthand for saying we attempt to control our perception of behavior. Most know we cannot "know" our behavior, merely our perception of the results of it.

This is certainly news to me. What field do you refer to as "contemporary psychology?" I guess I thought that behaviorists, who are still around despite premature funeral announcements, and cognitive psychologists as well, still tried to account for the actions an organism performs, not the perceptions it controls. Have psychologists suddenly stopped arguing with each other?

Best to all, Bill P.

Date: Fri Jul 01, 1994 7:19 am PST
Subj: Re: uncertainty, & self-regulation

From Tom Bourbon [940701.0908] >[From Jeff 940630] >Tom [940614]

> I will try to keep this brief.

I'm disappointed. :-(You left out most of the things I really wanted to hear about. After reading your earlier post addressed to me [From Jeff Vancouver 940622)], I tried to reply to each of your questions and to ask some questions of my own. I was hoping for a thoughtful answer, not a brush off. ;-((

> on mimicry:

> It sounds like your subjects could not control all the variables at once. Why do you suppose that is?

That's it? Shucks.

I was trying to see what _you_ thought about the topic. Why _can't_ a person control actions, and the disturbed consequences of actions, both at the same time? Or do you think they should be able to control both at the same time? I'd like to see your thought on the topic -- then something about how the mimicry-repetition study relates to that idea. As I recall, I was replying to a post from you, in which you said the following:

==============================

> Now, onto self-regulation. I get the impression that PCT likes to think of actions as unintended. I certainly think they can be. But I also think they can be intended as well. That does not mean intent equals success. Now one thing I had noticed among some self-reg people is an exclusive interest in intended behaviors.

> But are you, Tom, telling me that PCT does not included intended behaviors in its model?

===================

So I took you seriously and drafted a reply.

And what about Ford, and my questions to you about whether the people you say are doing the same thing as PCT do any modeling? That's not an idle question. If they don't do modeling, and if they don't explicitly say they believe people control their own perceptions, then there is no direct evidence that they are doing "the same thing" as PCT. I had hoped to learn _your_ ideas about that. If you think they are saying and doing the same things as PCT, then what are _your_ criteria, what evidence do you see, for deciding that is so? I'm not challenging you or attacking you; I'm a simple inquiring mind and I want to know. :-)

> on modeling and psychology:

> "Useless and dangerous" - that is a limb. I must get back to it, watch out.

I'm watching! I can't wait! Don't disappoint me this time! ;-)

Later, Tom

Date: Thu Jul 07, 1994 10:20 am PST
Subj: On a limb

From Tom Bourbon [940707.1217]

Back in June, Jeff Vancouver replied to a post from me. He concluded his post with a remark that psychologists were trying to do things that are useful to the world. I made this reply to Jeff.

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From Tom Bourbon [940629.1222]

As for what is useful to the world, I'll go way out there and assert that most of what psychology claims to have learned about behavior is not only useless, but is also dangerous to the world. No joke. :-(

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To which Jeff replied:

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[From Jeff Vancouver 940630]

"Useless and dangerous" - that is a limb. I must get back to it, watch out.

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And I said:

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From Tom Bourbon [940701.0908]

I'm watching! I can't wait! Don't disappoint me this time! ;-)
==============

Jeff, I'm still waiting out here on the limb. Come on out -- but before you start sawing, be sure to check and see which of us is farther from the trunk! ;-)

Tom

Date: Thu Jul 07, 1994 3:22 pm PST
Subj: Re: uncertainty, & self-regulation

[from Jeff Vancouver 940707] Tom Bourbon [940701.0908]
and Tom Bourbon [940614] and [940707] and [Marken 940707] while I was writing this.

I am glad you are curious about what I have to say. I am sorry I am not fast enough or long enough in the tooth for you. Remember, I am trying to get tenure.

ME:
>> on mimicry:

>> It sounds like your subjects could not control all the variables at once. Why do you suppose that is?

Tom [940701]

> That's it? Shucks.

> I was trying to see what _you_ thought about the topic. Why _can't_ a person control actions, and the disturbed consequences of actions, both at the same time? Or do you think they should be able to control both at the same time? I'd like to see your thought on the topic -- then something about how the mimicry-repetition study relates to that idea.

My thoughts are that it depends on the resources and the interaction between the perceptions that are trying to be controlled. If by "disturbed consequences of action" you mean, for example, that I knock over a lamp while mimicking some movement with my hands, I will not be able to prevent the lamp from hitting the floor using my hands and still mimic. The degrees of freedom in my hands don't allow it. But if I use my foot, which is not involved in the mimicking, then maybe I can continue to mimic and correct the falling lamp simultaneously, if I can divide my attentional resources appropriately.

Tom again

> I was replying to a post from you, in which you said the following:

> Now, onto self-regulation. I get the impression that PCT likes to think of actions as unintended. I certainly think they can be. But I also think they can be intended as well. That does not mean intent equals success. Now one thing I had noticed among some self-reg people is an exclusive interest in intended behaviors.

> But are you, Tom, telling me that PCT does not included intended behaviors in its model?

>===================

> So I took you seriously and drafted a reply.

I think the issue about intended behaviors is my misspeaking about intended results. I posted about this on 940630.

> And what about Ford, and my questions to you about whether the people you say are doing the same thing as PCT do any modeling? That's not an idle question. If they don't do modeling, and if they don't explicitly say they believe people control their own perceptions, then there is no direct evidence that they are doing "the same thing" as PCT. I had hoped to learn _your_ ideas about that. If you think they are saying and doing the same things as PCT, then what are _your_ criteria, what evidence do you see, for deciding that is so? I'm not challenging you or attacking you; I'm a simple inquiring mind and I want to know. :-)

I don't know yet. I told you what he said about behavior and perceptions. Somehow Bill P. saw PCT light in the description Ford made about behavior (although I cannot find that post).

>> on modeling and psychology:

>> "Useless and dangerous" - that is a limb. I must get back to it, watch out.

> I'm watching! I can't wait! Don't disappoint me this time! ;-)

> Later, Tom

My child awaits. Priorities. A teaser before I go. The paper I mentioned in my introductory post has come back as revise and resubmit. Naturally many of the reviewers did not like the PCT flavor of it. I am trying to formulate questions for those of you on the net to help address the issues. But this will take awhile. I am trying to be thoughtful :-)

Later Jeff

Date: Thu Jul 07, 1994 5:04 pm PST
Subj: CONTROL

From Tom Bourbon [940707.1729]

Just to tweak up the interest level a bit, one reason I say most claims to knowledge by psychology are dangerous is that innocent people might be tempted to believe those claims -- some might even go so far as to appeal to psychological "knowledge" when they attempt to establish new social policies, laws, and the like.

Later, Tom

Date: Fri Jul 08, 1994 8:13 am PST
Subj: Tenure, Publication

[From Rick Marken (940708.0820)] Jeff Vancouver (940707)

> Remember, I am trying to get tenure.

PCT (REAL PCT, not the Carver, Scheier, Hyland etc variety) and tenure do not mix. It's true that Tom Bourbon and I were able to get tenure while doing PCT, but we had to masquerade as "real" psychologists in order to do this. And, besides, we are _amazing_. Do not try this at home ;-)

> The paper I mentioned in my introductory post has come back as revise and resubmit. Naturally many of the reviewers did not like the PCT flavor of it. I am trying to formulate questions for those of you on the net to help address the issues.

This could get pretty confusing for you; having papers rejected by journals because they have a PCT flavor and then getting lashed by loonies (like me) on the net who say your papers don't have nearly enough PCT flavor.

I suggest that you not assume that PCT is the reason why reviewers are rejecting your papers. If you are persistent (as I was) you will get some of your papers published in some journals. The fact of the matter is that journals are very selective -- they have to reject at least 80% of the papers submitted. The REAL criterion for acceptance and rejection is probably political, more than anything else. One of my advisors in grad school could publish nearly anything he wrote (all of which was pure horseshit) because he was 1) a known entity and 2) good buddies with the editors of all the big journals.

I think PCT papers are often rejected for what are basically political reasons; but some are surely rejected because they really could be better. It's very hard to tell what's going on. The fact that the review process is so subjective means that political clout will count for a great deal of what actually gets published. Since PCT people have zero political clout, its very tough to get papers published, regardless of the merits of those papers.

The only thing that works is persistence; keep trying. Getting published is like an e. coli walk up a chemical gradient; you don't know what to do to get up the gradient (to publication city) so you, basically, make random changes in the paper and see if it gets looked on more favorably on the next submission. If you persist, you MAY eventually get published; if you give up, of course, there is no chance.

I'm not sure that what one does to get published is really random; one does try to take the reviewers comments into account when revising. But doing so is no guarantee that the paper will be looked on more favorably. I don't know what it was about some of my papers that got them published right away while others took (literally) years or were never published. It's really a crap shoot. One important consideration, of course, is the journal to which you submit; there should be a strong match between the contents of the paper and what the readers of the journal care about -- at least at that moment. I had a lot of trouble with the "Degrees of freedom" paper, for example, until I sent it to Psychological Science; apparently they just happened to be interested in "motor control" and "new ideas" at the time, and it got in pretty easily. The paper on "Random walk chemotaxis", on the other hand, took years (and MANY different journals) until it was finally hidden away in the never-read Behavioral Neuroscience. The problem with that paper (I think) was that we (Bill P. and I) were too overt in pointing out how the experiment and modelling results were inconsistent with any notion of "reinforcement". Saying that there is no such thing as "reinforcement" is just not "politically correct" in psychology circles; we got the paper published, finally, by billing it as a study of how control systems can navigate without steering (and hide the "reinforcement" conclusion in an aside in the middle of the discussion).

So don't blame non-acceptance of your papers on PCT (even if there might be some truth to it). It is possible to publish real, live PCT papers; it's not easy and it requires persistence but, unless you are working with a politically powerful ally, this is true for non-PCT papers too.

Best Rick

Date: Mon Jul 11, 1994 12:38 pm PST
Subj: PCT as paradigm

[Jeff Vancouver 940711]

Okay, the rubber has hit the road and I am seeking your input. As I mentioned the paper I referred to in my introductory post has been invited for revision and resubmission. Basically, the paper argues that my field (organizational behavior) needs a paradigm and that living systems theory should be that paradigm. Living systems theory (LST) is very PCT-like (this post is about some of the differences as I see them). In fact, many of the reviewers think LST is simply PCT with a different name. Given the prominence of Powers in my paper, this assessment is not far off base. However, I am advocating some things I do not see PCTers advocating, so I assume you all appreciate my use of another name.

1. I have focused a great deal of attention to the feedforward process. As I understand it and explain in my paper, feedforward is the process of anticipating discrepancies via memory of effector and perceptual signals. I believe this is in line with PCT. However, I go on to say that plans are made and even choices to engage are made based on those anticipated discrepancies. That the anticipated discrepancies are used to assess potential environmental disturbances and head them off. This is consistent with Ashby, if not PCT. I guess my question is: why this is _not_ consistent with PCT? Let me anticipate the possible answers:

a) anticipated _disturbances_ are not possible, regardless of how imperfect they may be (this last phrase counters the argument that the anticipated disturbances are merely conjectures on the part of the individual, because actually knowing the disturbances is beyond the system);

b) we have not gotten around to modeling it yet because it is too difficult to model or other things have taken priority;

c) anticipated disturbances and their effects on the system have been model in PCT, see (cite).

2. I think the main counter-argument to the PCT way of doing science (i.e., modelling), is that social interaction, meaning, and other higher-level processes, do not lend themselves to precise, quantitative equations. This is what I mean in b) above. Also, this is the intrigue of neural nets and fuzzy logic for many. They are quantitative, but not precise. I too lend toward those representations as possibly necessary when modeling the higher-level processes. Where does PCT stand on the issue of precision in it's models?

3. The distinction between the perceptual hierarchy and an individuals concept of that hierarchy seems to be a critical issue that separates PCT from most of psychology. That is, most of psychology concerns itself with concepts like self-concept and beliefs, but not with the actual system concepts, principles, etc. that drive behavior. Some do, but many do not. If I understand the psychologist's position, self-concepts and beliefs are available to the conscious. If I understand PCT, reference signals are not directly available, but perceptions are. What I am not sure about is whether the perceptions that are available to the conscious are before or after filtering through the input function and do those perceptions from the model that the psychologist study?

When I warned to I was going to make this post, Marken [940708.0820] replied with a comical post. Attempting to understand the behavior of reviewers, editors, etc. he vacillated between random and political, with a little writing style and content thrown in for good measure. (Marken, can you simulate this oscillating behavior - a control system trying to predict the behavior of other control systems).

Ironically, I agree, that all the factors are involved (weights change to protect the innocent). But one specific thing I want to highlight. Marken said:

> This could get pretty confusing for you; having papers rejected by journals because they have a PCT flavor and then getting lashed by loonies (like me) on the net who say yours papers don't have nearly enough PCT flavor.

Building communication links between schools of thought that have not been able to communicate well before is exactly the niche I am carving for myself. It is not an easy task, but if it was, there would be no need.

Later Jeff

Date: Mon Jul 11, 1994 4:56 pm PST
Subj: Re: PCT as paradigm

From Tom Bourbon [940711.1655] >[Jeff Vancouver 940711]

> Okay, the rubber has hit the road and I am seeking your input. As I mentioned the paper I referred to in my introductory post has been invited for revision and resubmission. . . . However, I am advocating some things I do not see PCTers advocating, so I assume you all appreciate my use of another name.

You assumed right. :-)

You do have an interesting job in store, trying to satisfy those reviewers.

> 1. I have focused a great deal of attention to the feedforward process. As I understand it and explain in my paper, feedforward is the process of anticipating discrepancies via memory of effector and perceptual signals. I believe this is in line with PCT. However, I go on to say that plans are made and even choices to engage are made based on those anticipated discrepancies. That the anticipated discrepancies are used to assess potential environmental disturbances and head them off. This is consistent with Ashby, if not PCT. I guess my question is: why this is _not_ consistent with PCT? Let me anticipate the possible answers:

> a) anticipated _disturbances_ are not possible, regardless of how imperfect they may be (this last phrase counters the argument that the anticipated disturbances are merely conjectures on the part of the individual, because actually knowing the disturbances is beyond the system);

Do you mean _anticipation of disturbance_ is not possible? I think that's what you intended and will reply as though that's the case. If I'm wrong, disregard everything I say, which you might do anyway. ;-)

To _know_ disturbances in advance of their occurrence is not possible. To anticipate (imagine) them in advance of their occurrence is possible, but the anticipating occurs in the present and that is where all of the actions to ward them off also occur -- in the present. If that is the case, then actions I take _now_ to create the perceptions of preparedness that I intend to experience _now_ can be modeled as part of a present-time process of negative feedback control: no future event is involved, only present-time imagination and intention and perception and action. Carrying an umbrella when I leave the house after hearing a weather report that predicts rain is present-time perceptual control, not feedforward. The fact that it is not now raining seems to lead some people into thinking that my actions are directed forward into the future, when they are in fact happening right now.

Also, in hierarchical PCT systems, higher levels have longer time constants than lower ones. An observer who notices the actions of lower-level loops sometimes "sees" those loops "taking action in advance of environmental events:" a person leans forward before taking a step forward -- a clear case of feedforward, is it not? Or before taking a long trip a person plans an itinerary, decides what private "stuff" to pack and carry, and makes arrangements for the care and feeding of pets, plants and other dependent creatures -- a clear case of feedforward, is it not?

In either case, I believe the answer is, "It is not." What is easily overlooked in either case is the "bigger picture" of what the person is controlling. The body does not "lean" independently of the person "taking a step." Leaning and stepping are not two discrete, isolated events; one thing has happened and an observer has treated it as though it were two (or more) things.

Part of the problem for an external observer who watches the behavior of a hierarchical, high-gain, negative-feedback control system is that activity at the lower end of the hierarchy of perceptual control occurs on a time scale that is easy to see in a glance; when things look different to us in successive glances, we easily see different "things" happening, then give them different labels, then explain them by different mechanisms. When actions happen close in time to what the external observer identifies as the purpose of the actions, the observer often says they involve feedback control; when actions happen in advance of what the observer says is the purpose of the actions, at least some observers say they involve feedforward. All the while, hierarchical, high-gain, negative-feedback control is probably lurking in the background, ready to confuse the innocent observer.

> b) we have not gotten around to modeling it yet because it is too difficult to model or other things have taken priority;

See my reply to a).

> c) anticipated disturbances and their effects on the system have been model in PCT, see (cite).

See my reply to a).

> 2. I think the main counter-argument to the PCT way of doing science (i.e., modelling), is that social interaction, meaning, and other higher-level processes, do not lend themselves to precise, quantitative equations.

You have identified a frequent comment from reviewers and editors. To counter them, I think you (all of us who try to spread the news about PCT science) need to redirect their attention to the phenomenon of control. For example, if you can show (empirically, not theoretically or in a model) that a particular social interaction includes controlled variables and that the actions of each social actor affect variables controlled by other actors, then you establish the fact of control in a social setting. Once you establish the fact -- the phenomenon -- of control, the nature of the game changes, or at least it should. Now anyone who wishes to explain the observed social interaction must demonstrate that, at least in principle, the suggested explanation can explain the phenomenon of control. Any "explanation" that cannot in principle explain control _should be_ dismissed. (All of this is easier to say than to do -- if you discover a way to make our interactions with reviewers and editors work the way they _ should_ work, share the news immediately!)

> This is what I mean in b) above. Also, this is the intrigue of neural nets and fuzzy logic for many. They are quantitative, but not precise. I too lend toward those representations as possibly necessary when modeling the higher-level processes.

This is where some of us part company with you, not necessarily because you are wrong and we are right, but because we don't want to allow ourselves any way out. We want to see just how far we can get using nothing other than the PCT model. Our activity is driven by our belief that we see evidence of control in phenomena where others think they need to talk about "higher-level processes." Our way of working has a lot in common with that of scientists who refuse to allow "the hand of God" as part of their explanations of nature; the challenge they set for themselves is to see how far they can get without "giving up" and invoking principles or powers from outside their scientific model.

> Where does PCT stand on the issue of precision in it's models?

Precision is our guiding p-star.

Oops! Time to run. We are finishing the plans for our daughter's wedding on Saturday and I'm almost late for a fitting! I'll try to get back to the remainder of your post tomorrow.

Later, Tom

Date: Mon Jul 11, 1994 8:49 pm PST
Subj: Re: feedforward: planning perceptions

[From Bill Powers (940711.2115 MDT)] Jeff Vancouver (940711)

> As I understand it and explain in my paper, feedforward is the process of anticipating discrepancies via memory of effector and perceptual signals.

But if you anticipate discrepancies, all you have done up to that point is to extrapolate from the present and perceive a calculated future. What happens next? A perceived discrepancy, whether it be in present time, calculated, or imagined, does not tell you what action you will have to take to correct it. When the time comes to act, you will still have to deal with the world as it is at that instant.

What you're overlooking is that ALL behavior, at ALL levels, is control of perception. You can't plan actions; you can only plan the perceivable consequences of actions.

You can plan to stop at the newsstand on the way home to buy a paper. Superficially, that might seem like planning actions: stopping at the newsstand; buying a paper. But the actions that lead you to perceive yourself as being at the newsstand are not predictable. You may park your car in front of it, if there's a space empty, or five spaces away in either direction, or in the next block. You may have to walk if your wife just drove off with the car. Wherever you start, you will have to walk toward the door of the newsstand from the exact spot in which you find yourself and not from where you vaguely imagined you would be, past the people standing around and going in and out, not on the empty stage you imagined. You must actually carry out every last detail of every muscle contraction that is required to get you through the door and up to the counter. When you "buy a paper", you can't plan where the proprietor will be standing, or whether the newspapers are sold out, or whether the proprietor has change for a ten, or which hand the proprietor will reach with to receive your money, or where the newspaper will be -- on the counter, or handed to you. You can't plan the actions needed to buy a paper well enough even to end up inside the newsstand and not crashing through its window.

The best you can do is form a very incomplete picture of the general situation you hope to experience; being somewhere at the newsstand, walking away with a newspaper held in one hand or the other, then being back home with it. You don't plan the means of achieving any of these perceptual goals. You can't. The world is too unpredictable. All you can do is plan _goals_, and leave it up to your control systems to bring them about in real-time perception, dealing with the world as it actually is, in all its detail.

Best, Bill P.

Date: Mon Jul 11, 1994 9:34 pm PST
Subj: Replies to Jeff

[From Rick Marken (940711.2200)] Jeff Vancouver (940711) --

> 1. I have focused a great deal of attention to the feedforward process.

> I guess my question is: why this is _not_ consistent with PCT? Let me anticipate the possible answers:

> a) anticipated _disturbances_ are not possible,

You got it right off the bat -- though it would be more correct to say "the actions that will compensate for disturbances cannot (and need not) be anticipated". I see Tom B. and Bill P. explain this rather nicely.

> 2...Where does PCT stand on the issue of precision in it's models?

Precise is nice. But the main point of PCT is that behavior is CONTROL. Since conventional behavioral science data provides no precise evidence of the variables people control, PCT does not apply to this data of conventional behavioral science.

> 3. The distinction between the perceptual hierarchy and an individuals concept of that hierarchy seems to be a critical issue that separates PCT from most of psychology.

I think this is really irrelevant. The critical issue that separates PCT from most (all?) psychology is the issue of control. Conventional psychology is about the control of behavior; PCT is about the behavior called "control". Conventional psychology tries to determine the variables that control organisms; PCT tries to determine the variables that organisms control. Conventional psychology and PCT are not talking AGAINST one another; they are talking PAST one another.

> When I warned to I was going to make this post, Marken [940708.0820] replied with a comical post.

Thanks for thinking it was funny, but I was actually trying to give serious suggestions about how to get published. What was so funny about it? I was really trying to help -- and encourage you to be persistent in your efforts to publish. I rooting for you to get tenure; once you're safe and secure THEN you can go ahead and do PCT right.

I said:

> This could get pretty confusing for you; having papers rejected by journals because they have a PCT flavor and then getting lashed by loonies (like me) on the net who say yours papers don't have nearly enough PCT flavor.

Jeff says:

> Building communication links between schools of thought that have not been able to communicate well before is exactly the niche I am carving for myself. It is not an easy task, but if it was, there would be no need.

I'm glad that you want to build communication links but I also want to be sure that you're putting the right message over the wires. Carver and Scheier and their ilk have gotten conventional psychologists to listen to the message of PCT by providing the wrong message; this is called lying. I'm happy to communicate with other schools of thought; but if they will only listen when you tell them that PCT is something other than what it is then what communication has there been?

Bill Powers (for one) has been communicating to the other schools of thought quite clearly for many years; it's seems to me that the other schools of thought have not shown any serious interest in the message of PCT -- and for good reason; it would mean the end of psychology as they know it, a very unpleasant experience for people who have built careers on psychology as we know it. Revolutions are not fun -- just ask Galileo. Should Galileo have placed the earth at the center of the model solar system just because it would have gone down better with the "other schools of thought" of the day. I don't think so.

Best Rick

Date: Tue Jul 19, 1994 12:02 pm PST
Subj: free will, feedforward, misc.

[From Jeff Vancouver 940718]

Well, getting to my office once a week certainly puts me behind the times. I can only skim the posts. Several posts where RE: Replies to Paul and Jeff, but were actually only to Paul. Please be careful, the subject headings on one way I reduce information overload. Incidently, Paul, glad to see a kindred spirit on the net. Our kindred is not in our professions (I am a psychologist), but in our message (seek similarities with others, not differences).

Ironically, to seek that goal I am still seeking to understand some of the differences. Tom [940711.1655] thanks for the post regarding feedforward, it helped somewhat. But I am beginning to worry that Locke is right, or we are arguing semantics.

You said "Carrying an umbrella when I leave the house after hearing a weather report that predicts rain is present-time perceptual control, not feedforward." Does it matter if the weather report was given that morning or the night before? Surely you are not saying the perception of impending rain, compared to a reference signal would produce a "get umbrella" output. Instead, you are saying the weather report triggers a memory of the perception of walking in the rain which is compared with a desire not to get wet to produce the output "get an umbrella when you are going out." The use of a memory store, an not real time perceptions is what I mean by feedforward.

Bill P. [940711.2115] says "You can't plan actions; you can only plan the perceivable consequences of actions." and later "All you can do is plan _goals_, and leave it up to your control systems to bring them about in real-time perception, dealing with the world as it actually is, in all its detail."

This is exactly what I mean by feedforward. We do not propagate the planning too far down the hierarchy. Such detail would get us into the trouble of over specifying a situation that is too variable. In a private post from Charles Tucker, he seemed to be saying that Bill P. has a precise definition for feedforward that 1) requires the propagation and 2) is therefore not relevant. He suggests using the term planning. How does this sound?

P.S. That the plan are goals (reference signals), not actions is often, though not always missed by cognitive psychologists.

Back to Tom. You said you would address the rest of my questions after fixing your tux, but I never saw a followup. Did I miss something?

Marken [940711.2200]

on the feedforward question you say: "the actions that will compensate for disturbances cannot (and need not) be anticipated."

I say that we (complex systems) need not, cannot completely, but can grossly anticipate disturbances. The fundamental issue is the role of conscious processing in humans. I am trying to say that complex systems use anticipatory mechanisms to more effectively maintain their essential (intrinsic) and controlled (perceptual) variables. Further, I am saying that conscious processes are heavily involved in that process. What role does conscious processes have in PCT?

As I reread some of Locke and other psychologists I see the issue of free will raised as central to their problem with PCT. They often go too far - action is a function of conscious will. Do you take the other extreme - action is random? Locke seems to think you do. I think the answer is in between. Action is indeterminable, but related to the perceptions one is controlling. This moves the free will debate to the question of will over perceptions controlled. I have no idea where PCT stands here (I certainly have my own ideas).

I certainly agree with you Marken [940711.2200] "Conventional psychology and PCT ... are talking PAST one another." You say because they don't study control. Humor me, I say they do sometimes study it, just not always exactly like you. I say a difference is the emphasis on an individual's concept of their place in the world versus the actual hierarchy. The concept is a perverse rendition particularly because it rarely uses a PCT image. But, psychologist, me among them, say that concept is relevant to how they act _and_ perceive. I think some in PCT think that as well. Again I ask, where does PCT stand on the self-concept?

By the way, the humor in your tenure post was the vacillation on the causes of paper acceptance. Reread it, given the right frame of mind you too may find it funny. I do appreciate the thought, though.

I did have a strong objection to your calling Carver & Scheier liars. The may have been mistaken, but not intentionally so (at least, I would give them the benefit of the doubt). Using a word like "lying" is inappropriate. I don't buy that you are just talking among yourselves. As Dag just noted, you have a number of lurkers. This is a public net. Beside, _I_ expect civility.

On precision:

Your faith in the TEST is great. Yet, even Runkel said it is hard to determine controlled perceptions during reorganization. There may be other ways. PCTers need not pursue them, but can't they let others without saying it is useless?

On the E.coli model:

Thanks Dag, I got the simulations. I have run most. Unfortunately, the E.coli, where you "showed" reinforcement does not work confirms my straw model argument. Reinforcement is operationalized as "use the last turn that resulted in denser foodstuffs" according to your documentation. Why not remember the last vector. Why not remember the last set of vectors? I am not proficient in this type of modeling, but your model does not convince you have given reinforcement theory a fair shot. (I am not bring it up to argue for reinforcement theory, but to note the use straw models).

[Bill Leach 940707.23:28]

You say: "I don't think that anyone on the net has a problem with the idea that people can control more than one operation at a time as long as it is physically possible to do so. Though I don't see 'an issue' here between PCT and any other 'model'."

The issue is one of cognitive resources not physical. Some psychological models are concerned with the limitation of our cognitive processes/attentional resources. This is why I want to understand the role of cognitive processes in PCT. BTW, the distinction between cognitive and conscious not always made in these models, but I don't want to revisit that either, unless it is relevant to the question at hand.

in conclusion:

I have printed a number of the posts between Paul and the PCTers. I will try to read them before my next visit to my office. I like the model. It gives a good reference for discussion. For example, is D what psychologists try to study? Here's a juice one, if numerous individuals F4's produce Hs that represents F in nearly identical ways and given that F3 is constant across people can I model (in Bandura's sense) someone's D to affect H, which I think is affecting F? The point of the question is that many of the components in the model can be ignored without much cost to predictive ability. I am going out to shoot myself now :-)

Later Jeff

Date: Wed Jul 20, 1994 1:36 am PST
Subj: Jeff

[From Rick Marken (940719.1400)] Jeff Vancouver (940718)

> (seek similarities with others, not differences).

Along with Mary Powers (940717) I think it is a mistake (well-intentioned, but a mistake nevertheless) to assume that you can move others (and yourself) toward PCT gradually, by seeking similarities with what they (and you) already believe. As Mary says:

> There really are no baby steps to take between behavior as outcome, consequence or result, and behavior as the control of perception

Jeff says:

> I certainly agree with you Marken [940711.2200] "Conventional psychology and PCT ... are talking PAST one another." You say because they don't study control. Humor me, I say they do sometimes study it, just not always exactly like you.

Humor me back, Jeff. Give me one example of a conventional psychological study in which control is the object of study.

> I say a difference is the emphasis on an individual's concept of their place in the world versus the actual hierarchy.

This has nothing to do with control as it is defined in PCT. I wrote a long post on the nature of control; perhaps we can start from there. Once you have read and understood that post, please give me an example of a conventional psychological study of control, even if that study is not exactly like one of ours.

> Again I ask, where does PCT stand on the self-concept?

I believe the term typically refers to a high level perceptual variable or set of variables. I think I control many different perceptions that have to do with my "self" -- simple perceptions like where I am at any particular time and complex perceptions like perceptions of myself following certain principles and system concepts. I think there is room for what has been called the "self concept" at all levels of the perceptual control hierarchy.

> I did have a strong objection to your calling Carver & Scheier liars.

Sorry. I re-read my post where I said this and I see that I expressed myself poorly. I didn't mean to say that C&S are liars. I meant that I would be a liar if I presented PCT as Carver and Scheier do. C&S are NOT liars because they have no idea what control is or how PCT explains it; they are not being duplicitous when they make PCT seem compatible with what conventional psychologists are already doing because they have no idea how PCT differs from what conventional psychologists are doing. I, however, do know what control is and how PCT explains it. I also know how PCT differs from what conventional psychologists are doing (so do Tom B., Bill P. and Mary P.). Therefore, if I made PCT seem compatible with what conventional psychologists are doing, I would be a liar. I'm not ;-)

> Your faith in the TEST is great. Yet, even Runkel said it is hard to determine controlled perceptions during reorganization. There may be other ways. PCTers need not pursue them, but can't they let others without saying it is useless?

You'll have to expand on this one a bit. What is "faith in the Test"? The Test is based on the definition of a controlled variable; it works -- amazingly well (ever do the "Mindreading" demo?). Where's the faith part? And what does reorganization have to do with it; if you are reorganizing -- and not in control of any particular variable -- then there IS NO variable under control and the Test will reveal that fact. The phrase "it is hard to determine controlled perceptions during reorganization" makes no sense; controlled perceptions are controlled perceptions. It doesn't matter at all whether they are controlled during reorganization or during a thunderstorm in Houston; if they are there, the Test will pick them up; no question. So I'm sure that Phil Runkel never said anything like that because Phil makes a lot of sense.

> Unfortunately, the E.coli, where you "showed" reinforcement does not work confirms my straw model argument. Reinforcement is operationalized as "use the last turn that resulted in denser foodstuffs" according to your documentation. Why not remember the last vector. Why not remember the last set of vectors?

Why not try that model? I'm REALLY tired of hearing this stuff about "straw men" theories and then getting verbal descriptions of how it could "really" be done -- and never being shown that it CAN really be done that way. Talk about faith! How stupid do you think Bill Powers and I are, anyway? We tried every model we could think of that was consistent with reinforcement principles as we understand them; none worked. Knowing that reinforcement theorists would say that we only tried "straw men" versions of their theory, we asked - - begged, pleaded with - - reinforcement theorists to show us how to do it RIGHT; we asked to see how ANY version of their theory could produce the operant behavior in the E. coli study. All we get from reinforcement theorists is the same line you deliver in the next sentence:

> your model does not convince you have given reinforcement theory a fair shot.

What would convince you? What model would be fair? If we come up with the model, it's a straw man (as long as it doesn't work, and none of our "straw men", so far, have worked); but the reinforcement theorists won't show us how to do it "correctly". They just say (like you) that reinforcement theory CAN explain our results. Pretty cute. Apparently, reinforcement theory cannot be rejected. It's proponents do not feel like it's necessary to do anything more about our data than be unconvinced and say that we are not fair. Well, excuuuuuuse me for daring to challenge the wisdom of reinforcement theory.

> (I am not bring it up t argue for reinforcement theory, but to note the use straw models).

What are you arguing for then, Jeff, the unrejectability of a theory? You say you are not arguing FOR reinforcement theory but you are also saying (not showing, SAYING) that the theory CAN explain the operant behavior in the E. coli situation, where the consequences of actions are random. I think you're missing a bet, here, Jeff. If I were you, I'd be arguing FOR reinforcement theory; then I couldn't possibly be wrong.

Best Rick

Date: Wed Jul 20, 1994 9:12 am PST
Subj: Reinforcement theory vs E. coli

[From Bill Powers (940720.0745 MDT)] Jeff Vancouver (940718)

> Unfortunately, the E.coli, where you "showed" reinforcement does not work confirms my straw model argument. Reinforcement is operationalized as "use the last turn that resulted in denser foodstuffs" according to your documentation. Why not remember the last vector. Why not remember the last set of vectors?

Because none of these interpretations results in a working model, either. We also tried "previous time rate of change of concentration" and "average rate of change of concentration over past n episodes" and "difference in average concentration between episodes before and after a tumble." The basic problem is this: the next direction that will result from a tumble is always selected at random. So no matter what criterion you use as the basis for tumbling, it can't be used to select a more favorable direction of the next tumble. There is no control over direction, no way to predict what the next direction will be.

The point of this experiment was not to show that "reinforcement doesn't work." It was to show that the organism can move in the right direction even when there can be no basis for differentially reinforcing a "correct response" on the basis of past consequences of responses. Thus the explanation for the behavior we see has to be something other than reinforcement theory. The paper offered a PCT model that _does_ work, the model demonstrating its own correctness by producing behavior very much like that of E. coli. In this model, E. coli senses the time rate of change of concentration of the attractant (due to its swimming speed and direction in the chemical gradient), and varies the delay before the next tumble according to whether the _current_ sensed rate of change is above or below a reference setting. The result is that the organism tumbles sooner when swimming the wrong way and delays the next tumble while swimming the right way. Previous consequences of tumbles don't enter into the model at all.

There is no consequence of the _previous_ direction of swimming that has any systematic relationship to the _next_ direction of swimming, because tumbles are truly random. Since reinforcement theory depends on _past_ consequences of behavior to shape _future_ behavior, all attempts to producing a working reinforcement model run into the snag that the tumbles result in new directions unrelated to the old direction. When all behaviors are randomly rewarded and punished, reinforcement theory must predict either random behavior or superstitious behavior, neither of which will result in the observed behavior of E. coli.

I should mention that the model is based on direct observation of the behavior of E. coli tethered in a flow of liquid in which the concentration of perfused attractants could be varied experimentally. The spacing of tumble episodes was found in this way to be proportional to the time rate of change of concentration relative to some particular rate of change (which we took as evidence of a reference setting). The relevant work is reported in Koshland, D. (1980); _Bacterial Chemotaxis as a Model Behavioral System_, New York: Raven Press.

The referees who rejected the paper could not reconcile the fact that the tumbles resulted in random new directions of swimming with the fact that E. coli found its way very efficiently up the gradient. Most of them simply refused to believe that the new directions were random (Koshland cited actual measurements showing a uniform and random distribution in space). The alternative explanations offered by the referees all depended on NON-randomness of tumbling, on the existence of discriminative stimuli which they made up out of thin air (and wouldn't have helped anyway), or on geometric arguments that simply ignored the fact of randomness. Their faith was being sorely tested, but they kept to it in the usual way: by changing the facts or making up facts. They rejected our paper because according to their beliefs neither E. coli nor our working model could possibly have behaved they way they did. In fact, their reactions were a clear-cut proof that reinforcement theory could not handle this phenomenon (although that didn't help with getting the paper published).

The other factor that all the referees shared was that their arguments were purely verbal; not one of them offered a refutation in the form of a working model that actually behaved as they claimed such a model could behave. Not one of them found any flaw in the construction of our working model. Not one of them commented on the striking similarity between the plots of the behavior of our model and the behavior of E. coli. I don't think that any of them actually understood the concept of a simulation as a way of demonstrating the predictions of a theory. Like most psychologists, they simply assumed that a working model built to fit their explanations would actually work as they claimed it would. In this case, it would not.

Best, Bill P.

Date: Thu Jul 21, 1994 3:25 pm PST
Subj: Reinforcement theory and free will

[from Jeff Vancouver 940721]

First I want to apologize for my inability to read all the posts on the net. I just skimmed it for references to my posts. I found two [Marken 940719.1400 and Powers 940720.0745]. To which I respond below. Meanwhile I am continuing to develop a strategy to respond to my reviewers. Sometimes this takes my questions to different levels, as you will see below.

[Marken 940719.1400]

Study of control by Psychologists:

Campion, M. A., & Lord, R. G. (1982). A control systems conceptualization of the goal-setting and changing process. Organizational Behavior and Human Processes, 30, 265-287.

Kernan, M. C., & Lord, R. G. (1990). Effects of valence, expectancies, and goal-performance discrepancies in single and multiple goal environments. Journal of Applied Psychology. 75, 194-203.

Hollenbeck, J.R. (1989). Control theory and the perception of work environments: The effects of focus of attention on affective and behavioral reactions to work. Organizational Behavior and Human Decision Processes, 43, 406-430.

Can you give me the reference for your "long post on the nature of control?" Most of your posts seem to fit this criteria :-)

By the way, I was reading your Degrees of Freedom paper. You cite neural networks as promising for modeling perceptions. Have you changed your mind or did I misunderstand something you said?

On reinforcement [both Marken & Powers]:

I want to take this to a higher level, but I need to respond to Marken & Powers. You are correct that I cannot give a counter model. I do not how to model! I sympathize with your inability to get your detractors to work with you on such a model. That is the most one can ask. Your efforts are impressive. I no long think that you "straw maned" reinforcement theory. Instead, I think your description of E.coli _as you model it_ can be interpreted as reinforcement theory. I should reread Powers response to the behaviorists in earlier writings. But let me quote Bill P's post (940720.0745):

"In this model, E. coli senses the time rate of change of concentration of the attractant..., and varies the delay before the next tumble according to whether the _current_ sensed rate of change is above of below a reference setting."

Forgive me, but cannot that be restated "E. coli senses the time rate of change of concentration [stimuli] of the attractant [reinforcer]..., and varies the delay [a response] before the next tumble according to whether the _current_ sensed rate of change is above of below a reference setting."

That is, the stimuli "causes" a behavior, where the behavior is length of delay!

If this argument, or some form of it, has been rebutted in published work, just cite it - no need to repeat here. Unlike Paul, I have amassed most of it over the years. Unfortunately, it is beginning to fade from my mind.

Here is the lasted bottom line:

The problem of "reinforcement theory" in your models is important (to me) because of Locke's claim PCT is neobehaviorism. If I understand it, the usual response is that the model includes internal [the real crux of control] as well as external variables (e.g., behavior). Since behaviorism did not acknowledge any of these internal variables, PCT is not behavioristic. This always worked for me.

But as I try to reconstruct my own counter arguments (the editor wants me to take the debate further), I am beginning to see a point that I cannot get around. If the values of reference signals (higher-order output signals) are arrived at randomly, which is the central process for reorganization, then your model does seem - on one level - difficult to distinguish from neobehaviorism (by which I think they mean S-O-R). Stimulus (S) leads to response (R) with the organism's (O) perception as a mediating variable and discrepancy (of perception from reference signal) and the ultimate reinforcer. (Ironically, Bandura was one of those who popularized S-O-R).

Adding the O takes a lot of wind out of the PCT "is not neobehaviorism" argument I had used in my mind. Maybe I have been too generous in thinking neobehaviorism includes O. If it does, than I can accept that, argue that PCT is neobehaviorism, so what? It still serves functions goal theory and social cognitive theory do not. But I suspect S-O-R is not acceptable to PCT either. Plus the argument takes us to interesting new dimensions.

It is Marken's insistence that behavior is out-of-control that seems to support the neobehaviorism argument. As I understand it, controlling a perception variable occurs in two ways. Once the appropriate lower-order reference signals have been discovered, the loop simple sends those signals. Although this might result in different behaviors (due to different circumstance each lower-order loop must deal with to meet their sent reference signals), the flavor of the response to a stimuli (which was translated to a perception that sends the focal loop's perception off its reference signal), it the same. That is, the response is reoccurring.

I anticipate red flags going off here. PCTers will say, "but the behavior is different because the circumstances are different. Only that special case where the circumstances are exactly the same, or close enough for your statistic analysis, will a reinforcement model work. And what is the _flavor_ crap?"

This _flavor_ crap is the other way Locke might mean neobehaviorism. Regardless of Locke or Bandura, however, I can describe a concept called "flavor of a response" which means set lower-order reference signals the same way. The result is the R in S-O-R is now flavor of response. It is completely internal because lower-order reference signals are internal. (Perhaps Locke and Bandura would not accept this meaning for R and I can stop here. But this argument has been made by Bill P. and I want to take it further. When one takes it further, it gets very interesting.)

If one can accept the new definition of R, then I can move to the next way of controlling -- that is, before a flavor of a response is developed. This is the reorganization process. During this process varying reference signals are sent to various lower order loops in a random, but localized fashion. As these random changes begin to reduce the discrepancy, the set of reference signals for the lower-order loops begins to be defined. That is, the probability that a lower-order reference signal will have a certain value increases as the discrepancy is reduced (reinforced). Recognize behaviorism?

Now, will a simple adjustment counter the argument? If the set of reference signals is developed discontinuously (either it eliminates the discrepancy or it does not. If not, try completely different configuration), than the phrase cannot be probabilistic (it becomes: a set of lower-order reference signals will be adapted if the discrepancy is made zero). But this is not much of a change and becomes problematic for meeting complex perceptions. Also, the concept of "localized" reorganization would need to be examined.

No, the real problem is the method of reference signal selection. Let us not consider hardwired, which some consider, but has not been modeled. Hardwired will not work for controlling most complex perceptions anyway, so lets only concern ourselves with the only other process considered by PCT advocates - random. This is the only central difference between PCT and the Locke and Bandura self-regulation models where the Locke & Bandura arguments make some sense to me. (Another difference is that they talk of controlling behavior, which is clearly wrong on their part, so we need not discuss it further with regards to Locke & Bandura).

Here is where we get really philosophical. Locke & Bandura are arguing that reference signal selection is our source of free will. We consciously choose our goals (reference signals). I think they would concede that this does not happen all the time, but at least some of the time. They claim conscious choice translates to free will, because it is not completely predictable or determinable. I don't buy the free will argument for a second (although lack of predictability and determinability is much easier to accept). The free will argument requires that humans are fundamentally different from the rest of nature (Sappington, 1990?, Psych Bull). A conclusion I cannot accept. (Ironically, Sappington uses Bill P. and Bandura to argue for free will, but it seems Sappington was assuming that reference signal selection was ultimately non-random.)

What I do accept is that conscious processes are often involved in selecting the (set of) reference signals. I am a soft determinist, which means consciousness enters the process of behavior selection (which strictly speaking is better said reference signal selection), but that conscious choices are determined by some other factors. This is what I like about Bandura's model, the other factors are abstractly described (e.g., self efficacy). Of course, as Bill P. pointed out (1991, Amer Psych), the specifications are somewhat problematic if one were to try to model them. I agree. What I don't know if these _types_ of factors are considered in PCT? Specifically, Bill P. says "beliefs about one's actual effectiveness in achieving a given goal [Bandura's self-efficacy]" is a perception (1991). By that do you mean self-efficacy is just another controlled variable? If is it a controlled variable than 1) what is "F" in your model and 2) what is F2? If F is an internal variable related to the focal goal and if F2 affects references signals related to the loop (goal/task) under consideration then we have just resolved a major conflict among the models.

One more problem and I will have resolved two of my biggest conflicts with PCT. If a function type for F2 includes using an external address for a reference signal, then outside influences are available for constructing a hierarchical control system. Is it possible?

The messy models in psychology begin to describe many of these influences. Together with the structure of PCT and the rigor in your methodology, I think psychology can make great strides.

Postscript: I think one can persuade others that PCT has something to offer. The exact function is debatable, but I have moved closer and closer to PCT as I have come to understand more and more of it. Bill P. claimed most people take 2 years. Ed Ford and Dag report making converts slowly. This data, anecdotal though it is, indicate the process is not completely discontinuous, although certain leaps my occur on the way. I suspect that Locke and Bandura will only be convinced by PCT when they perceive rewards are contingent upon accepting it, which will probably not happen in what remains of their careers. But I will first seek to convince myself, then the reviewers and editor. If I am not convinced, I will take what I am convinced about and make clear the distinctions between PCT and my view. I would appreciate Bill P.'s sanction on the manuscript to assure our respective views are properly represented.

A parallel question is can I convince PCTers that psychology has something to offer PCT? The argument is parallel. Some are already convinced. Others will never be. But, unfortunately, in my system, that loop has little gain. PCTers won't grant me tenure.

Later Jeff

Date: Thu Jul 21, 1994 4:47 pm PST
Subj: out on a limb, part 2

[From Jeff Vancouver 940721.1808]

I hesitate to post this given that my previous post is my main concern, but I forgot to ask Marken where "the Blind men and the elephant" was published.

But while here, I wanted to rebute the psychology is useless and dangerous notion. In my field we develop tests of cognitive ability and other predictors of job performance. Focusing on job samples, which rarely have adverse impact (where scores differ depending on race or sex), these tests can predict job performance at around .30 to .60, depending on the job mostly. The alternative, doing nothing, would correlate .00 with job performance, or the job interview (before we improved it) .11. The differences might seem trivial to those who look for correlations in the upper 90s, but the difference can save a company hundreds of thousands of dollars (we have data on this). From the individuals stand point it will improve the fit between their skills and their job, which usually make the individual happier and more secure. Without the work of psychologists, organizations would be less productive and individuals would wander from job to job looking for a good match.

There is work in psychology that is dangerous and other work that is useless.

My colleague next door does it (just kidding). But there is also useful and helpful work. May we be the ones who decide which is which.

Later Jeff

Date: Thu Jul 21, 1994 11:17 pm PST
Subj: Re: out on a limb, part 2

<[Bill Leach 940722.00:26 EST(EDT)]
>[From Jeff Vancouver 940721.1808]

> But while here, I wanted to rebute the psychology is useless and dangerous notion. ...

> There is work in psychology that is dangerous and other work that is useless. My colleague next door does it (just kidding). But there is also useful and helpful work.

Sounds like your work may indeed be useful. You work with data that does not explain (or try to explain) how people function but rather with data that is statistical and correctly so. There is no doubt the occasional error where your data is not correct for a particular individual but like the "mortality tables" your data "is the best we can do.

PCT is probably not at a point where is could be reliably or economically applied to such a task (even if there were enough PCTers to try to do so).

The quality of your data would however benefit from a deep understanding of PCT. A serious problem with a great deal of the data collected in the behavioral sciences is that critical information needed to relate the data to people based upon how people actually function is not taken.

-bill

Date: Thu Jul 21, 1994 11:30 pm PST
Subj: Re: Reinforcement theory and free will

<[Bill Leach 940721.23:14 EST(EDT)] >[Jeff Vancouver 940721]

I recognize, Jeff, that you must primarily pay attention to the writings of the PCT researchers but I would like to comment nonetheless:

> Forgive me, but cannot that be restated "E. coli senses the time rate of change of concentration [stimuli] of the attractant [reinforcer]..., and varies the delay [a response] before the next tumble according to whether the _current_ sensed rate of change is above of below a reference setting."

> That is, the stimuli "causes" a behavior, where the behavior is length of delay!

This would be ok except that there is no know way to postulate a "sense of time" for the E.coli. Indeed, even for humans, it seems that elapsed time is available only through indirect perceptions. In addition, it is probably possible to prove that the trigger is a concentration issue and not a time issue (though I don't know that such proof has been attempted).

> But as I try to reconstruct my own counter arguments (the editor wants me to take the debate further), ... If the values of reference signals (higher-order output signals) are arrived at randomly, which is the central process for reorganization, then your model does seem - on one level - difficult to distinguish from neobehaviorism. ...

I think that there are several different "opinions" concerning reorganization. One is that it is a "catastrophic" phenomenon that only "kicks in" when intrinsic errors exist that are either of a "large" magnitude, or maybe exist for an extended period of time.

The second triggering condition also fosters the idea that reorganization might be a nearly continuous process. There is also the possibility that reorganization is not only local but that the magnitude of its effect is related to the magnitude of the error.

As you are no doubt aware, observation of "learning" is strong evidence in support of both the reorganization concept and its' "random" nature.

A serious problem that you face is that control theory itself is not well understood in the first place. Even people with extensive control systems experience are often astonished at just how much power a closed loop control system can exhibit. Indeed, the long time PCTers will, in truth, admit that they did not really realize the capability of just this one paradigm AND are still discovering new abilities that such systems display.

> It is Marken's insistence that behavior is out-of-control that seems to support the neobehaviorism argument.

"Out-of-control" might be a poor choice of words (in some sense). Behavior is not controlled is correct. The sense of this is that behavior is driven by the presence of an error between perception and reference. The behavior that results is a function of physical laws including the physical capability of the person and the physics of the portion of the environment being acted upon. The "person" is "causing" the behavior in that all behavioral is a direct result of something that the person does but what is important is that the something is control perception where control has the same meaning as it does in the field of control theory.

A loose definition of the term control allows one to say that we control our own behavior but such a statement is using an entirely different meaning of the term control from the precise meaning used in control theory and PCT.

> lower-order reference signals the same way. The result is the R in S-O-R is now flavor of response. It is completely internal because lower-order reference signals are internal.

This is fine except that it does not explain control action. A stimulus which directly affects a controlled perception will look like a form of "stimulus-response" to someone unaware of the control system nature of living beings. The problem occurs when an applied stimulus only partially affects (or does not affect at all) any controlled perception of the subject. Then you get a "66%" of the subject, etc. type report with the researcher ignoring the other 34% percent that are also very real subjects too.

> If one can accept the new definition of R, then I can move to the next way of controlling -- that is, before a flavor of a response is developed. This is the reorganization process. During this process varying reference signals are sent to various lower order loops in a random, but localized fashion. As these random changes begin to reduce the discrepancy, the set of reference signals for the lower-order loops begins to be defined. That is, the probability that a lower-order reference signal will have a certain value increases as the discrepancy is reduced (reinforced). Recognize behaviorism?

I have to admit that at first, this one really "hit me" as a problem but upon thinking for a bit...

What if there is no stimulus? What if there is a stimulus but the behavior that develops is eventually completely unrelated to the original stimulus? That challenge seems to ignore purpose (or the idea that the references are set up by the organism and not the environment.

> Now, will a simple adjustment counter the argument? If the set of reference signals is developed discontinuously (either it eliminates the discrepancy or it does not. If not, try completely different configuration), than [then?] the phrase cannot be probabilistic (it becomes: a set of lower-order reference signals will be adapted if the discrepancy is [not?] made zero).

I'm not sure I am following what you are trying to say there well enough to even comment.

> But this is not much of a change and becomes problematic for meeting complex perceptions. Also, the concept of "localized" reorganization would need to be examined.

"Becomes problematic" is an opinion that is not well supported by the facts.

I think that you will find that the PCTer agree that "localized" reorganization needs to be examined.

> No, the real problem is the method of reference signal selection. Let us not consider hardwired, which some consider, but has not been modeled. Hardwired will not work for controlling most complex perceptions anyway, so lets only concern ourselves with the only other process considered by PCT advocates - random.

A difficulty here is again the assumption that "hardwired" will not work. There is no evidence that the highest level perceptions to which all others are ultimately related may indeed NOT be "hardwired" (at least over the lifetime of a specific instance of an organism). I believe that the problem with reorganization is that not enough is actually known about control systems in general and random changes to effect control.

> The free will argument requires that humans are fundamentally different from the rest of nature (Sappington, 1990?, Psych Bull).

> A conclusion I cannot accept. (Ironically, Sappington uses Bill P. and Bandura to argue for free will, but it seems Sappington was assuming that reference signal selection was ultimately non-random.)

If by deterministic you mean that given sufficient knowledge about a specific organism and the environment for that organism one could predict exactly what that organism will do, then that is likely true. However, when you count the apparent randomness of physics at the particle interaction level such predictions are not likely. New evidence that electron orbital energy releases are random has recently been obtained (as if there was a great deal of doubt to start with).

> One more problem and I will have resolved two of my biggest conflicts with PCT. If a function type for F2 includes using an external address for a reference signal, then outside influences are available for constructing a hierarchical control system. Is it possible?

If I understand what you are saying there, then the answer is no.

-bill

Date: Fri Jul 22, 1994 1:03 am PST
Subj: Re: free will, feedforward, misc.

From Tom Bourbon [940721.1803]

Still catching up after the wedding. >[Jeff Vancouver 940718]

> Ironically, to seek that goal I am still seeking to understand some of the differences. Tom [940711.1655] thanks for the post regarding feedforward, it helped somewhat. But I am beginning to worry that Locke is right, or we are arguing semantics.

I'm afraid I don't catch your allusion to Locke. I know his work, but /i don't catch what you mean when you say you "worry that Locke is right?" Right about what? Why does whatever he said worry you?

> You said "Carrying an umbrella when I leave the house after hearing a weather report that predicts rain is present-time perceptual control, not feedforward." Does it matter if the weather report was given that morning or the night before?

Not a bit, except that the accuracy of weather reports drops off exponentially with time! Carrying the umbrella is present-time behavior, no matter how long ago I heard the weather report.

> Surely you are not saying the perception of impending rain, compared to a reference signal would produce a "get umbrella" output.

I'm not? Maybe I am; maybe I'm not. I _am_ curious about why you wouldn't want me to say that -- why my saying that would perturb you. Can you let me know?

> Instead, you are saying the weather report triggers a memory of the perception of walking in the rain which is compared with a desire not to get wet to produce the output "get an umbrella when you are going out."

But aren't you saying here that, if there is a discrepancy when present-time perceptions are compared with present-time reference signals, then there will be a change in actions? That was what I was saying -- it's all in the present. The past does not exist now; the future does not exist now; now exists now.

> The use of a memory store, an not real time perceptions is what I mean by feedforward.

Oops. Where did feedforward come from? "Memories" are present-time "things," aren't they? And so are perceptions, and comparisons, and discrepancies (error signals) and actions. Maybe it would help me understand you better if you were to give me your definition of "feedforward."

> Bill P. [940711.2115] says "You can't plan actions; you can only plan the perceivable consequences of actions." and later "All you can do is plan _goals_, and leave it up to your control systems to bring them about in real-time perception, dealing with the world as it actually is, in all its detail."

> This is exactly what I mean by feedforward.

This example doesn't help me very much, as a clue about your definition of feedforward. I see Bill talking about present-time "planning" of the expected results of unplanned actions. I plan to experience some dinner soon -- I've had nothing to eat since breakfast, which was long ago. I "plan" to experience myself logging off, locking the office, taking the elevator down, waiting for my wife to stop by on her way home, and so on. All of these imaginings are right now; all of my actions (which I cannot plan in advance) will be happening "now;" all of my perceptions will be controlled "now."

> We do not propagate the planning too far down the hierarchy.

I don't think we propagate a plan for _actions_ at all.

> Such detail would get us into the trouble of over specifying a situation that is too variable.

The variability is precisely the problem. ;-)

> In a private post from Charles Tucker, he seemed to be saying that Bill P. has a precise definition for feedforward that 1) requires the propagation and 2) is therefore not relevant. He suggests using the term planning. How does this sound?

Planning perceptions, yes. Planning actions, no.

> P.S. That the plan are goals (reference signals), not actions is often, though not always missed by cognitive psychologists.

I'll drink to that! Or maybe I'll eat a few bites of dinner to that.

> Back to Tom. You said you would address the rest of my questions after fixing your tux, but I never saw a followup. Did I miss something?

I'm still looking for them. I'm not yet caught up on the past week of mail.

> Marken [940711.2200]

> on the feedforward question you say: "the actions that will compensate for disturbances cannot (and need not) be anticipated."

> I say that we (complex systems) need not, cannot completely, but can grossly anticipate disturbances.

Jeff, _why_ do you say that? Can you give some examples we could model, quantitatively? How would we test for whether a person is doing what you say here?

> As I reread some of Locke and other psychologists I see the issue of free will raised as central to their problem with PCT. They often go too far - action is a function of conscious will. Do you take the other extreme - action is random?

Did you ever see anyone (PCT modelers) say action is random? (You might have -- I'm just asking.) If so, what were the conditions under which they said actions would be random -- or at least _appear to be_ random, to an observer?

> Locke seems to think you do.

Can you quote Locke on that? I have a small sampling of his writing and I'd like to see if I have that quote, or if I can locate it for my file of mistaken ideas about PCT.

> I think the answer is in between. Action is indeterminable, but related to the perceptions one is controlling.

You are onto an important theme here. Indeterminable from whose perspective? That is the crux of the matter.

> I certainly agree with you Marken [940711.2200] "Conventional psychology and PCT ... are talking PAST one another." You say because they don't study control. Humor me, I say they do sometimes study it, just not always exactly like you.

Again, all you need to do to convince us on this point is show us, chapter and verse, where they themselves say they study the phenomenon in which individuals control their own perceptions and, incidentally, control variables in their environments. I think Rick is saying there isn't evidence that psychologists study that phenomenon.

> I say a difference is the emphasis on an individual's concept of their place in the world versus the actual hierarchy. The concept is a perverse rendition particularly because it rarely uses a PCT image. But, psychologist, me among them, say that concept is relevant to how they act _and_ perceive. I think some in PCT think that as well. Again I ask, where does PCT stand on the self-concept?

Rick and Mary have already answered you on this point. I concur.

Time for dinner! Now, what was that plan? First twitch the extensors attached to that bone over there ... . ;-))

Later, Tom

Date: Fri, 22 Jul 1994 11:08:58 -0600
Subj: Misc replies RE PCT

[From Bill Powers (940722.0800 MDT)]

Jeff Vancouver (940721) --

In studies of control by psychologists, the main missing ingredient is an understanding of what happens when outputs affect inputs at the same time that inputs are affecting outputs. You are perfectly correct in seeing that a control system is just an S-R system with a reference signal that introduces a bias. In fact, you could probably find S-R studies in which the reference signal was recognized, as the "effective zero" of the stimulus. Way back when, I was convinced that this was the wedge that would get control theory into psychology; in fact, it turned out to be a "wedge" more in the modern usage of a sticking place.

There are two reasons for this wedge. One is that psychology is heavily biased toward seeing stimuli and responses as discrete events: first the stimulus goes "ping" and then the response goes "pop." I think that one reason for doing this (and designing experiments this way) was precisely to keep responses from interfering with the administration of stimuli. If you could get the stimulus over with quickly enough, the response couldn't modify it before you were through manipulating it. That meant that the stimulus could be considered an independent variable and the response a dependent variable, as required by the statistical analyses (and the theories) that were used. Also, for those who did recognize the closed loop, this meant that they could treat the loop as a sequence of events: S-R-S-R- and so on.

The second reason is that psychologists knew nothing about control theory and therefore didn't understand that closed loops of causality would have emergent properties that were not obvious from the sequential event-oriented viewpoint. As rumors from engineering began to spread into psychology, in the 1950s, psychologists picked up a few ideas, such as the fact that control systems tended to be slow and unstable, and would run away if they were too sensitive. This smattering of ignorance convinced them that feedback phenomena couldn't be very important in behavior, because organisms could act quickly and stably, and didn't show runaway behavior.

PCT is based on the actual properties of sensitive closed-loop systems. Most psychological attempts to analyze goal-seeking system are based on incorrect rules of thumb, or completely misunderstandings of how such systems work.

> I think your description of E.coli _as you model it_ can be interpreted as reinforcement theory.

> ... "E. coli senses the time rate of change of concentration [stimuli] of the attractant [reinforcer]..., and varies the delay [a response] before the next tumble according to whether the _current_ sensed rate of change is above or below a reference setting."

The problem with saying it this way is that it isn't reinforcement theory. What is happening is that the _current_ time rate of change is affecting the _current_ delay, thus varying the time of the _next_ tumble. According to Skinner, reinforcements are consequences of _past_ behaviors, and when they occur, they tend to increase the probability that the same behavior will occur again in the future.

When the next tumble occurs, the result is a new time-rate-of-change of attractant. That is the consequence of the _current_ amount of delay being generated -- as you say, "the response". Unfortunately, the next time-rate-of-change is completely unrelated to the current one. Whether the current response be a short delay or a long one, the chances that after the next tumble the concentration of attractant will be increasing are equal to the chances that it will be decreasing. Long and short delays are not differentially rewarded by the results of the next tumble.

There is in fact no strategy based on past rewarding outcomes that can be used as a way of selecting a better future response. Each segment of E. coli's travel is an entity unto itself; whatever results in systematic progress up the gradient must happen during each segment independently of all others before or after.

I don't object to the words of reinforcement theory. If you want to call input variables "stimuli" and "reinforcers", and outputs "responses," that's OK with me. What I object to is the _organization_ of reinforcement theory, which claims that past rewards determine future behaviors: that behavior is controlled (meaning determined) by its consequences, to put the thesis exactly in Skinner's words.

> The problem of "reinforcement theory" in your models is important (to me) because of Locke's claim PCT is neobehaviorism.

How about quoting us some quotes? I can see that this might give your editor some problems. From what I've seen of Locke, he attributes characteristics to control theory that just aren't true; maybe we can find some specific statement to refute. One of Locke's complaints about control theory is that it isn't based on any experimental data! I guess Locke carries a lot of weight in your field.

> Maybe I have been too generous in thinking neobehaviorism includes O. If it does, than I can accept that, argue that PCT is neobehaviorism, so what? It still serves functions goal theory and social cognitive theory do not.

Attaboy. It doesn't matter what you call it. Except, of course, to your editor.

> As I understand it, controlling a perception variable occurs in two ways. Once the appropriate lower-order reference signals have been discovered, the loop simply sends those signals.

Actually, the higher level of control is continuously monitoring its own controlled variable, and continuously varying the reference signal of the lower system as a means of controlling the higher variable. The reference signals are not just sent as blind outputs to the lower system. The result of sending them is always being reflected in the state of the higher perception, so control is continuous. This is true at all levels. The higher system can't just decide on a good output and stick with it, because there are always many influences tending to alter its perception. It has to vary the output that becomes the lower-level reference signal according to the current state of error in the higher system.

> No, the real problem is the method of reference signal selection. Let us not consider hardwired, which some consider, but has not been modeled. Hardwired will not work for controlling most complex perceptions anyway, so lets only concern ourselves with the only other process considered by PCT advocates - random.

Careful, here. In PCT, control systems are what you might call soft-wired. That is, reorganization can slowly and randomly change the wiring, but on the time-scale of ordinary behavior the system is, for all practical purposes, hard-wired. We handle control of complex perceptions by dividing them into orthogonal sets, each of which varies in magnitude only, in one dimension only. We see a separate control system for each possible dimension of variation (that is under control). What is normally treated as a single complex perception then becomes a collection of perceptions, each representing one dimension of the complex perception.

This seems very wasteful, but actually you end up with extremely simple control systems for any one dimension, simple enough that they could be implemented with a few neurons. The other way of approaching it looks more compact, but requires an enormous number of computations, so I don't think you end up saving any neural capacity.

Reorganization involves a random component, but the operation of the control systems in the main hierarchy doesn't.

> Locke & Bandura are arguing that reference signal selection is our source of free will. We consciously choose our goals (reference signals).

Well, that's a feeble step toward a hierarchical model, isn't it? After they have pushed this idea of internal goal-selection for a few years, maybe one of them will wonder _why_ a particular goal is chosen -- what does it do for the person to freely chose that goal instead of another? Then they might realize that there is another level of goals and perceptions, which are achieved by selecting the lower level of goals.

Free will isn't as simple a concept as Locke and Bandura make it out to be. I'm perfectly free to move my hands any way I please, until I am using them to steer a car. Then I must vary my hand position as required by physical laws to keep the car where I have freely chosen it to be on the road. If I wish to avoid running into a culvert, on the other hand, then I must choose positions for the car on the road that do not intersect the culvert, and if I choose to leave one road and turn onto another, I have to choose positions for the car that will achieve that goal -- a very limited set of positions, in comparison to what free choice might allow.

So every goal is chosen as a means of achieving a higher goal, and as soon as that is recognized, the lower-level goal can no longer be chosen freely. It has to be chosen so that achieving it will achieve the higher goal. Is there a highest-level goal? Where does it come from? Questions to be answered experimentally, not by philosophy.

> Specifically, Bill P. says "beliefs about one's actual effectiveness in achieving a given goal [Bandura's self-efficacy]" is a perception (1991). By that do you mean self-efficacy is just another controlled variable? If is it a controlled variable than 1) what is "F" in your model ...

F is the set of lower-level perceptions on which you base your perception of self-efficacy. They might consist of such perceptions as one's own degree of skill, other's opinions about one's effectiveness, memories of successes and failures, and so forth. You can control the perception of self-efficacy toward a high level by developing more lower-level skills, by persuading others to admire you, or whatever means will alter the perceptions on which you based the perception of self-efficacy. You can also control for a low-degree of self-efficacy: you can make clumsy mistakes, antagonize others, refuse to learn any skills, and so forth (depending on what perceptions add up to self-efficacy for you). Why would you choose a low level of perceived self-efficacy? Perhaps to avoid being given responsibilities -- after all, nobody gives the hard important work to a klutz. ----------------------------------- >If a function type for F2 includes using an external address for >a reference signal, then outside influences are available for >constructing a hierarchical control system. Is it possible?

Uh-uh. Reference signals in the hierarchy are strictly the outputs of higher-level systems. There is no way the environment can directly set any reference signal inside the person. You can set up circumstances in which a person might well choose to set a given reference signal in a given way, but that is always up to the person, not the environment. You can be told "Ride this horse to lose the race or I will kill your daughter." After weighing your goals regarding losing races and losing your daughter, you might decide to ride the race to lose. But you might also weigh other goals, and kill the person who is threatening you on the spot. Or you might decide that you can always make another daughter, but you have only one reputation as Dick Francis to lose. Higher considerations always come into play, and they always come into play inside the person actually doing the controlling. All the outside world can do is present circumstances and connections. It can't force a person to choose any setting for any reference signal.

> I would appreciate Bill P.'s sanction on the manuscript to assure our respective views are properly represented.

At your service.

> In my field we develop tests of cognitive ability and other predictors of job performance. Focusing on job samples, which rarely have adverse impact (where scores differ depending on race or sex), these tests can predict job performance at around .30 to .60, depending on the job mostly. The alternative, doing nothing, would correlate .00 with job performance, or the job interview (before we improved it) .11. The differences might seem trivial to those who look for correlations in the upper 90s, but the difference can save a company hundreds of thousands of dollars (we have data on this).

Now this gets ticklish, because if I don't just say that your field is wonderful and useful, you will get all prickly and start thinking up defenses, and we won't get anywhere. So try to remain calm.

Why do you suppose it is that companies and other large organizations are willing to put out serious money to get potential employees tested, while job applicants fear these tests and avoid them wherever possible? The answer lies in the correlations you cite above. Obviously, if a company uses screening tests, even with as low a correlation as 0.3, it will in the long run avoid hiring quite so many unsuitable people. That can save it a lot of money and grief.

But now consider the testing procedure from the standpoint of the person taking the test. If the correlations are as high as 0.6, this means that the "coefficient of uselessness" (sqrt(1-r^2), more traditionally called the coefficient of alienation) is 0.8. If I remember Gary Cziko's explanation correctly, this means that you would do 80% as well in predicting performance simply by taking the mean of the group performance. If the correlation is as low as 0.3, the coefficient is 0.95. In any event, this means that many people who test low actually belong in the high group and vice versa.

From the individual's point of view, this means that there is a very high probability of being misjudged -- either being accepted for a job at which you will fail, or being rejected from employment which you could easily handle.

Where the large company can even out the statistics by using the test on hundreds of people per year, the individual applicant gets only one chance every five or ten years. Furthermore, the payoff matrix for the company is weighted oppositely to that for the individual applicant. If the company makes an occasional mistake, it loses little, and occasionally gets even more than it bargained for. The individual, however, is faced with the alternative between making a good living and a poor living (or none at all). A misjudgment is far more serious for the individual than for the company. The usual justification for using these tests is that "over the long run" they are quite reliable. But for the individual, there is no "long run."

I realize that I am taking on a multi-billion-dollar industry here, and have about as much chance of reforming it as the proverbial snowflake of surviving in hell. But am I not speaking the truth? The harm done by psychological testing in industry is not to its beneficiaries, the companies who commission such testing. It is to those who are tested.

You say " From the individuals stand point it will improve the fit between their skills and their job, which usually make the individual happier and more secure." But that is a myth. Over the long run, what you say is true from the company's standpoint -- but it is false for a very large proportion of the individuals, particularly if you include all the individuals tested, not just those selected.

Best to all, Bill P.

Date: Fri, 22 Jul 1994 13:01:10 CST
Subj: Re: out on a limb, part 2

From Tom Bourbon [940722.1105]

>[From Jeff Vancouver 940721.1808]

> I hesitate to post this given that my previous post is my main concern, but I forgot to ask Marken where "the Blind men and the elephant" was published.

It is in our ghetto journal, _Closed Loop_, 1993, vol. 3, no. 1 -- the same issue that contains "Models and their worlds," by Bill Powers and me. Copies are available from Mary Powers and I have several extras.

> But while here, I wanted to rebute the psychology is useless and dangerous notion.

Jeff, I'll start my reply by going to the conclusion of your post.

> There is work in psychology that is dangerous and other work that is useless. My colleague next door does it (just kidding). But there is also useful and helpful work. May we be the ones who decide which is which.

I've stopped using the label in most settings, but I am also one of "we;" I am -- gulp -- a psychologist.

I am still a card-carrying member of the American Psychological Association, the American Psychological Society (a charter member), and the Psychonomic Society (than which there is no purer group of experimentalists ;-). I taught psychology courses, undergrad and graduate, for over 28 years. I speak from _inside_ psychology.

What I am about to say is not "merely" a PCT issue, but an issue that should concern all behavioral (life, medical, cognitive, etc) scientists, no matter their theoretical stripes.

> In my field we develop tests of cognitive ability and other predictors of job performance. Focusing on job samples, which rarely have adverse impact (where scores differ depending on race or sex),

Does this mean scores that lead to wrong predictions, but do not differ according to the race or sex of the test taker, rarely have "adverse impact?"

> these tests can predict job performance at around .30 to .60, depending on the job mostly. The alternative, doing nothing, would correlate .00 with job performance, or the job interview (before we improved it) .11.

In my reply, I am assuming that the scores you report are correlation coefficients, calculated from scores on the tests compared with measures of performance on the job. If my assumption is wrong, never mind what I say next. :-) I know, Jeff, that you already know many of the things I am about to say, but there is a diverse audience looking over our shoulders and I want to be sure the same vocabulary is available to everyone. And please don't think I am trying to impugn your motives or your character.

A common interpretation of a correlation coefficient (r) is in terms of the percentage of the variance it "accounts for" in the relationship between the two variables (for example, between test scores and measures of job performance). A common estimate of the variance accounted for is the value of r-squared. In your example, r-squared ranges from .3^2 = .09, 0r 9%, to .6^2 = .36 or 36%. Now it is certainly true that either of those correlations and variances accounted for is greater than zero. But how well do they work, and does the use of the tests really have no adverse impact on people? From whose perspective is the effectiveness or adverse impact of the tests decided?

Concerning the effectiveness of the tests, there are other informative ways to interpret the relationships "captured" by correlation coefficients. One of those ways is to calculate k, the "coefficient of alienation," which is a coefficient of inefficiency or failure of the correlation as a predictor. The calculation is simple: k = square root(1 - r-squared).

In your example, for r = .3, k = .954; for r = .6, k = .800.

What do those numbers mean? First, if you did not know the correlation between test scores and job performance, but you did know the means of both sets of scores, then given a particular person's score on the test, your best estimate of the person's job performance would be the mean score on the scale of job performance. This is always the relationship between predictor and predicted scores, _if_ you do not know the correlation, or if the correlation is zero. (When r = 0, k = 1.00 -- maximal alienation or "uselessness" as a predictor.) Any non-zero correlation should reduce the coefficient of alienation, indicating that the correlation improves your ability to predict performance from the test. But the gain in predictive ability is low, until the correlation coefficient is very large.

For example, your correlation of .3 leaves the chance of a failure in your prediction of job performance 95.4% as great as it was when you did not have the test. And for r = .6, the chance of failed predictions is 80% as great as before the test.

I grant, right up front, that even a 4.6% success rate is non-zero and that it might appear to be of some use to an employer, but what about the people to whom the test is applied and whose lives are thereby affected? The race, gender, age, height, and sock size of those individual persons are irrelevant; the fact is that many more people will be harmed by the application of such a test than will be helped, unless, of course, the people we are talking about are the employer or people on the employer's "team."

> The differences might seem trivial to those who look for correlations in the upper 90s,

For now, let's just say they are demonstrably small and they are very poor, as predictors.

> but the difference can save a company hundreds of thousands of dollars (we have data on this).

I grant you that.

> From the individuals stand point it will improve the fit between their skills and their job, which usually make the individual happier and more secure.

From the stand point of _which_ individuals? Certainly not from that of the 95.4% to 80% of people who are misclassified by such a test. The misclassifications will include both the very large numbers of people who are denied a job and the ones who get the "wrong" job.

> Without the work of psychologists, organizations would be less productive and individuals would wander from job to job looking for a good match.

I think both of those claims can be challenged, especially the one that seems to portray humanity as lost and wandering in the wilderness, but for the intervention of psychologists. Most of the psychologists I have known seemed pretty badly lost, themselves. ;-)

I am _not_ saying the quest for efficiency and fairness is wrong. I _am_ saying that the psychological "knowledge" brought to that quest is poor and that its application is dangerous, if and when the application is to individuals.

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><[Bill Leach 940722.00:26 EST(EDT)]
>>[From Jeff Vancouver 940721.1808]

>> But while here, I wanted to rebute the psychology is useless and dangerous notion. ...

>> There is work in psychology that is dangerous and other work that is useless. My colleague next door does it (just kidding). But there is also useful and helpful work.

> Sounds like your work may indeed be useful. You work with data that does not explain (or try to explain) how people function but rather with data that is statistical and correctly so.

Ah, Bill, the tests _do_ allegedly say something about how people function; individuals are the ones who are given jobs, or denied jobs, on the psychologist's assumption that a single person's test score says something about how that particular person would function in a specific job. The test may be _administered_ to people en masse, and scored in bulk on a computerized system, but its application and effects come down one misclassified person at a time.

> There is no doubt the occasional error where your data is not correct for a particular individual

Occasional error? From 80% tp 95.4% errors don't rank as "occasional" in my book. As a psychologist, I am ashamed that we use such lousy and dangerous predictors. But then, as a psychologist, I'm a pretty lousy representative of the field.

> but like the "mortality tables" your data "is the best we can do.

And "the best we can do" is not good enough, in my private opinion.

By the way, the mortality tables do represent descriptive statistics of high quality -- we have pretty good data, from large numbers of cases, on the proportions of people in various age groups who die during a given period of time and on whether those proportions are stable or changing. We have nothing near that quality with regard to the validity of screening tests.

> PCT is probably not at a point where is could be reliably or economically applied to such a task (even if there were enough PCTers to try to do so).

As I said up top, this is not a PCT issue, but an issue of the adequacy of data and predictions in behavioral science. I'll crawl even farther out on my limb: The predictive power of psychological assessment "instruments" will never improve very much, so long as the causal model behind psychological research and test construction is lineal and so long as psychologists continue to mis-apply statistical procedures, making statements and predictions about _individuals_, when they have used statistical procedures that (when used properly) only allow you to speak about groups.

Later, Tom

Date: Sat, 23 Jul 1994 08:56:00 -0600
Subj: More on disturbances

[From Bill Powers (940723.1650 MDT)]

Jeff Vancouver (940721.1808)--

> But while here, I wanted to rebute the psychology is useless and dangerous notion. ...

I am quite taken by your "creative typo." The word "rebute" combines the functions of "refute" and "rebuke"; it means, obviously, "to prove beyond doubt that your argument is faulty and at the same time chastise you for having presented it."

By the way, to "deny" a statement is to say that it is wrong. To "refute" a statement is to _prove_ that it is wrong. . . . . .

Best, Bill P.

Date: Wed, 27 Jul 1994 16:28:10 CST
Subj: Re: limbs, papers, models

From Tom Bourbon [940802.1418]

>[from Jeff Vancouver 940728]
>Tom Bourbon [940725.1200]

> Briefly, 1) organizations are going to select (discriminate) regardless of psychologists providing them tests (they will do something because they must)

Fine, so let them do it. Just don't expect me to participate in the misapplication of poor psychological data in a manner that unjustifiably harms the people who are tested and discriminated against. People who do such things do so because they intend to do so, not because they must. If psychologists are satisfied to earn a fat fee by helping employers in that discriminative task, power to them. For me, I've taken the PCT poverty vow.

> 2) prior to providing those tests organizations tended to discriminate unfairly (the popular notion of discriminating) and poorly, that is, organizations used methods that predicted performance very poorly.

And now they discriminate fairly? Using "instruments" that are wrong in from 80% to 95% of the cases? Sorry, but I don't buy into that. The tests harm many more innocent people than they help.

> 4) now _some_ organization use method that predict performance much better (particular when used together) and thus save the organizations large amounts of money

Much better? In your original post on this thread, you said the correlation between interviews and job performance was about .1 (any data on that?) and that the correlation between psychological tests and job performance ranges from .3 to .6 -- correlations that yield the percentages of incorrect predictions I mentioned above. Even I ;-) can see that the proportion of failed predictions went from .995 (99.5% of them were wrong) when only the interview was used, to .80 or .954 with the tests. I can see the difference, but I can also say, as a psychologist expressing a personal opinion, the difference isn't something I would be proud of.

Much better? Could you describe your criteria for making that statement?

As for saving the organizations money, if they say so, I accept it. After all, we are talking about _their_ bottom lines.

You say that when tests are used in combination, the results are even more impressive. But if the tests are independent, as the tester would want them to be, then the results are multiplicative. If you use two independent tests, each of which correlates .3 with job performance, then each of them "explains" .09 of the variance -- when used alone. When you use them together, they do not explain .09 + .09 = .18; but .09 * .09 = .008. Use them together and you are worse off than with either used alone, and neither was very good when used alone.

> 5) individuals who are not selected by these tests are often better off because they would have been fired eventually or not done well, which is usually frustrating and debilitating.

Hmm. That's _very_ interesting. Let me try to get this straight, because you seem to be alluding to a breakthrough in predictions that is of major proportions. At a correlation of .3, a test would misclassify 95.4% of the takers with regard to their performance on the job for which they applied. Yet, you are saying many of the people were in fact _correctly_ identified and further that those who were rejected would indeed have done sufficiently poorly they would have been fired. Can you tell us about how someone would decide whether any particular person who was rejected would have > been one of the sure-fire fired failures? We could probably make a fortune by applying your technique. ;-)

> 6) the general public is often better off (we do not want airplane pilots that cannot fly very well, which we might not be able to tell except under adverse - or in this case - simulated adverse conditions).

Agreed, and I'm damned pleased the pilots who took me to the meeting and back were good at their profession. But you were talking about something else -- tests that lead to wrong conclusions in from 80% to 95.4% of their administrations. Pilots aren't selected that way.

> 7) individuals can use the results of tests to clue them into deficiency and competencies - and often do.

Sorry, I don't follow you here. Can you help me?

> bottom line: tests give use more information than no tests. We must use that information responsibly (and we have associations that attempt to see that we do).

Once more, I do not deny that there is a difference between 99.5% errors and 95.4% errors. Speaking for myself, I think the only way to use such information responsibly is to warn the public and do all we can to eliminate the present abuse of innocent test takers.

> But psychologists help develop the tests and the methods for using the information gained from them responsibly. (e.g., we have always advised against using the MMPI for selection purposes - it was to designed to aid in diagnosis).

Yes, psychologists often do try to prevent applications of their tests outside the settings for which they were designed. I respect (some of) those efforts.

However, I'm afraid my concerns also extend to applications in the original, intended settings. Poor correlations are poor correlations, no matter where they occur; abuses that arise from the application of poor correlations are abuses, no matter where they occur.

> I just picked up Runkel's casting nets book. He acknowledges the uses of the method of frequencies. This is what I have described above.

I'm glad to hear you got Phil's book. I would recommend it to _everyone._ However, I don't think the tests you described illustrate the method described by Phil. In fact, I believe Phil would identify most uses of psychological assessment as _inappropriate_ applications of the method of relative frequencies. When it is used properly, the method of relative frequencies tells you that certain proportions of people are found in certain categories. As important as that result can be sometimes, that is all the method of frequencies tells you. It leaves you in a situation where you can make _absolutely_ _no_ statement about any particular individual. Any application of group data (even of properly collected group data) to specific individuals is unjustified.

> The method of specimens (and PCT) is we our profession needs to better understand humans and thus construct better tests (instruments is the better word, but too long).

Yes! On this, I believe Phil Runkel would agree, as well. And the _only_ way to design better tests for predicting what a given individual will do is to study people one at a time and, paradoxically, thereby learn something about how all of them "tick." Phil called that kind of research the method of specimens. PCT is an example of a science that studies individuals, as specimens of the species, or more generally as specimens of life.

Let us know what else you think of Runkel's book.

>Tom Bourbon [940725.1633]

> See my address above for sending the models paper. Appreciate it.

Great. A copy of "Models and their worlds" will be in the mail tomorrow. If you read it, that will make a total of five or six people in all the world. ;-)

> I am still waiting to hear your reply to the rest of my post. I do not, nor does Bandura or Locke, interpret the S-O-R symbol as requiring lineal causality (although I see why it is easily interpreted that way).

But it _is_ lineal, Jeff. It includes two assumed end points, with causality moving from the beginning to the conclusion. It doesn't matter a whit that they put something between the beginning and the conclusion -- causality still works in one direction with two end points. The same can be said of _every_ information processing "model" that speaks of Input-Processing-Output. Every such model is a variation on the same lineal theme -- and that theme is inadequate as an explanation of the behavior of living things.

> Bandura spends much time in his recent work to the reciprocal determinism idea (cyclical causality), which I use frequently (but have a problem with the looseness of words - given I know PCT)

Ah, but the fact you know PCT should make Bandura's cute little word games all the more unacceptable to you -- well, I can't defend that kind of prescription for you, but it certainly applies to me. The phenomenon of control is not an example of "cyclical causality," as Bandura defines that term, but it is an example of a continuous, simultaneous relationship between an organism and some particular part(s) of its environment. If Bandura knows the difference, then he would serve science better were he to speak clearly and draw the distinctions crisply. But I believe there is ample evidence he does not know the difference; what he believes, he says.

It is one thing to believe there is something "reciprocal" about the relationship between person and environment; it is quite another to understand how such a relationship can come about and persist. Up to now, from all I have seen, Bandura hasn't a clue about how it can happen. In fact, Bandura has made a special point of rejecting, out of hand, both (1) descriptions of the phenomenon of control and (2) the PCT model. He is clueless.

> But Bandura and Locke's models are flow charts (Powers 940507.1420), not system diagrams. That is why they cannot model their theories (and why PCT is fundamentally better than their theories).

Agreed, on the first and final points, but not quite agreed on one in the middle. They use flow charts by design -- by intention -- not out of necessity. They have no intention of modeling their ideas. They verify their ideas by assertion, by citations of data that are lousy but are statistically significant, and by appeal to authority, not by demonstrating that the ideas work. It is by their own design that they do not model their theories.

> However, there are practical application of their flow charts that PCT is not capable of making. Like, if performance is low, check self-efficacy, if it is low, try to increase it, performance often improves (which makes EVERYONE happier).

Again, Jeff, I believe I understand why someone, a psychologist for example, would want to know about or talk about such things, but the constructs are just too inexact for me. They define "self-efficacy" operationally -- in terms of test scores that correlate with -- with what? And why do they accept correlations that, while statistically significant, suffer error rates as high as those for the pre-employment screenings you described earlier? I have seen no evidence from them that "self-efficacy" exists, as they define it, much less that it can act to cause behavior.

They use poor data and untested theories as justifications for their statements about "big" topics. The fact that PCT modelers often refrain from speaking about many of those topics should not be taken as evidence that those who do speak, speak from a base of scientific knowledge.

> What I want to know is how does a belief like self-efficacy plug into PCT? (My previous post began to talk about that).

I think Rick gave a good answer to this question.

> One final question (for now).

Wow! You have really fired off a salvo of questions! I have been at this longer than I should have been and must run to the lab for a while. I promise to come back to the final questions. Note the plural -- you didn't stop with just one! :-))

Later, Tom

Date: Tue, 2 Aug 1994 17:20:50 -0600
Subj: Re: free will (from Mary)

[from Mary Powers 9408.02]

>[Jeff Vancouver 9407.18]

> As I reread some of Locke and other psychologists I see the issue of free will raised as central to their problem with PCT. They often go too far - action is a function of conscious will. Do you take the other extreme - action is random? Locke seems to think you do. I think the answer is in between. Action is indeterminable, but related to the perceptions one is controlling. This moves the free will debate to the question of will over perceptions to be controlled.

I really don't know what Locke and other psychologists are saying about free will, but if they insist that it is about producing actions and that it is always conscious then they will indeed have a problem with PCT.

The PCT alternative to the conscious production of action is not to assert that actions are random. Actions must be free to vary in order to control perceptions in a changing environment (and we've discussed the fact that the environment is always changing because factors like muscle fatigue mean that no actions can ever be exactly the same). But freedom to vary does not mean random. Actions, although free to vary, are limited by the environmental constraints which one perceives - I always leave a room by the door because I haven't figured out how to walk through walls, nor would a door in the ceiling be much use to me. In any case, I can usually get out of the room, which is my intention, even if the environment prevents me from doing it in certain ways, and certain actions are more likely to be used than others, like walking and twisting the door handle.

We leave rooms all the time without conscious intent. We may become conscious of the problem if the door is locked, or if there is a fire in the hall and leaving by the window seems like a better idea. We intend to be out of the room, and do what we must to have that perception.

As for choosing which perceptions are to be controlled: eventually as you go up the hierarchy you come to very important perceptions like "the self" which Locke and others seem to believe are conscious. But we act to control perceptions all the time which, if you trace the reference signals up, do maintain a desired state of the self without there ever being consciousness that that is the goal. I may be leaving the room to go to the bathroom, but I do not have to be, and therefore am not, conscious of maintaining my self as someone who does not wet her pants in the room where the conference is meeting.

Another angle on free will as coming from a conscious, self level is the idea that consciousness reaches the highest level there is. I prefer the idea that it does not: that concepts like creativity, intuition, and having a higher power, are a few of the ways of referring to properties of the human organism which are not accessible to consciousness, and which are the source of reference levels which, when maintained at certain values, are experienced as the self. But they do not come from the self.

I think that Locke and others see the self as not only conscious, but also fixed at the highest level they can think of, which seems to be the logic level - and of course as performing all these huge calculations all the time to produce outputs. Thus they see choosing, or intending, or having free will, as being conscious and as being expressed as calculating those outputs. This concept has to be a product of where they get to when they introspect as to how they themselves function - informed, of course, by a particular model of how the brain must work.

I believe that "having free will" is simply how it feels to control one's perceptions. You don't feel you have it when the environmental constraints are too severe, and you don't feel you have it when you have reference signals that conflict with one another - when you can't control one set of perceptions because, awaredly or not, you also want the opposite thing, and you are stuck in a position of being unable to achieve either. But these are special cases.

Mary P.

Date: Wed, 3 Aug 1994 00:32:50 -0400
Subj: Re: limbs, papers, models

<[Bill Leach 940802.23:36 EST(EDT)] >Tom Bourbon [940802.1418]

Tom;

I really dislike taking stabs at you after your previous post to me but a perception of an error must not be ignored...

> want them to be, then the results are multiplicative. If you use two independent tests, each of which correlates .3 with job performance, then each of them "explains" .09 of the variance -- when used alone. When you use them together, they do not explain .09 + .09 = .18; but .09 * .09 =

> .008. Use them together and you are worse off than with either used alone, and neither was very good when used alone.

Seems to me that this is a matter of how the "results" are viewed. If "employ" results from any one test are interpreted to mean "hire this person", the calculation is additive (assuming that the test really are 'independent').

I'll leave it to Jeff to explain how the results of multiple tests are handled.

-bill

Date: Wed, 3 Aug 1994 12:11:32 -0600
Subj: free will;

[From Bill Powers (940803.0930 MDT)]

Bill Leach (940802.23:53 EDT) --

(horning in)

> Mary, are you postulating that the 'highest level' references establish in a deterministic way the goals of the immediate level down...

Higher level reference signals determine only the higher level perceptual signals. The reference signals actually sent to lower systems depend on what disturbances are acting. To achieve a higher goal you must VARY lower goals, not set them to fixed states.

Suppose you have the freely-chosen goal of holding a steering wheel centered. This means that you must set reference signals for applied force to WHATEVER LEVEL IS NEEDED to cancel any externally-originating forces that also affect the position of the steering wheel. If the external forces vary, you must vary the goal-setting for applied force in exactly the opposite way, provided you still want to keep the steering wheel centered. So the environment determines what forces you will have to produce if you want to achieve the goal of a certain steering wheel position.

Now, suppose you decide (freely) that instead of wanting to keep the steering wheel centered, you want to keep the car on the road. This goal now requires that you VARY the position of the steering wheel in any way required to counteract crosswinds, tilts in the road, and curves in the road. Now the physical properties of the world determine from moment to moment the angle of the steering wheel that you must set as a subgoal in order to achieve the higher goal of keeping the car on the road.

You can continue this: you must vary the position on the road in order to turn off onto another road; you must vary your choice of roads in order to get to your destination despite detour signs; you must vary your destination if you want to close a deal with another person who has moved to a different town; you must vary which person you close the deal with if you want to get the best deal; and so forth. Each goal that seems freely chosen at the moment becomes determined by external circumstances when you take the next higher goal into consideration.

Even at the highest level, whatever it may be, you must VARY that highest goal if you want to remain alive in the manner that best suits a human being -- which is determined by your intrinsic reference signals, which are inherited. The only thing you are truly free to do arbitrarily is to be human -- and only the species, not you as an individual, can do that, or choose to cease doing that.

So we have to conclude that free will does not consist of freedom to choose goals arbitrarily. Arbitrary choice of goals would disable part or all of the hierarchy of control above the level of the choice.

Where, then, could truly free will come into play? Obviously it must come into play where it will not disrupt any higher control systems (which will automatically resist the disruption). This suggests that it will come into play where there are no higher control systems -- in branches of the hierarchy that remain unfinished. But reorganization will automatically come into play there, too, if intrinsic reference levels are violated. So the range of free will is not completely infinite, given the goal of staying alive.

However, for every control problem there are multiple solutions which are equivalent in terms of their effects on intrinsic state. A person can resolve certain problems either by becoming a mathematician or by becoming a truck driver. Either way, the person will survive adequately to prevent intrinsic error. So we can suspect that what is called free will comes into play under "don't-care" conditions. Where there are multiple routes to a successful organization, one can choose any route freely, without constraint because of any knowable consequences at higher or lower levels.

It is sometimes claimed that free will is evident in the fact that we can volitionally produce any arbitrary action we please at any time, for no reason. This is tantamount to saying that anything can be reorganized. But that idea considers the volitional act only at the microsecond of its execution. All acts have consequences, and we are not free to like or dislike any particular consequence chosen at random. If a volitional act, an act of reorganization, has consequences that cause errors in existing control systems, or in the reorganizing system, there will soon be a need for another volitional act, compensating for or undoing the effects of the original one. In other words, if we do not like the consequences of a volitional act, the only way we can correct the problem is to nullify the volitional act: we have no choice, if we want to go on experiencing what we like rather than what we dislike.

At bottom, what we like and what we dislike are built into us: pain hurts, pleasure feels good. What we experience as pain and pleasure are consequences of actions of control systems which exist beyond our ability to be conscious of them: all we know is that certain consequences seem in themselves to have a quality that is given, beyond analysis. What we call feeling "good" or feeling "bad" is the criterion against which we judge all experiences. Solving a difficult set of equations feels good; failing feels bad. That is our built-in nature speaking to us, through intrinsic error signals or diminution thereof.

So where does this leave "free will?" I think it leaves these words in the same class as "intelligence" or "traits" or "phlogiston." These are words that sound or once sounded, and were used, as though they must have some profound meaning. This impression proved ultimately to be mostly an illusion: there was no experience to go with the words.

Your objection to Tom's remarks is valid:

Tom: >When you use them together, they do not explain .09 + .09 = >.18; but
.09 * .09 = .008.

Variances are like probable errors squared. They add.

What Tom is saying applies to the truth-value of assertions. I suppose there is a way of converting from "percent variance explained" to "probability that the hypothesis is true." If you have two hypotheses, each with a probability of truth of 0.6, then the probability that both are true at the same time is 0.36. So if the hypotheses are each "probably true", any conclusion that is drawn from assuming they are both true is "probably false." This is what dooms a psychology built on low-probability statistics to remaining a simple science, if it can be called a science at all.

Of course any hypothesis that explains only 9% of the variance is "probably false" to begin with.

Best to all, Bill P.

Date: Thu, 4 Aug 1994 16:04:13 CST

Subj: Re: free will

Tom Bourbon [940804.1519]

>[From Bill Powers (940803.0930 MDT)]
. . .
>Bill Leach (940802.23:53 EDT) --
. . .

> Your objection to Tom's remarks is valid:

> Tom:

>> When you use them together, they do not explain .09 + .09 = .18; but .09 * .09 = .008.

> Variances are like probable errors squared. They add.

I was talking about proportions (or, with a slight change, percentages), not variances. The correlations originally described by Jeff Vancouver (between pre-employment tests and job performance for the few people who were hired after the employers' psychologists finished with them) ranged from r = .3 to r = .6. For a Pearson-r correlation, the "proportion of variance explained or accounted for" is r-squared; the percentage of variance "explained" is (100 * r-squared). For r = .03, the proportion of variance explained is = 09. For r = .6, "proportion of variance explained" = .36. Neither test is very good, as is shown by their respective coefficients of alienation, k, where k = squareroot(1 - r-squared). For r = .3, k = .954; for r = .6, k = .80.

Pretty soon, this turns into number salad, but something devastatingly important for traditional behavioral (medical, cognitive, etc) research lurks among the olives and lettuce leaves. Scientists often use correlations to justify their attempts to predict one kind of performance (on the job, for example) from some measure of another kind of performance (scores on a paper-and-pencil test, for example). If you do not know the correlation between the two sets of scores, or if the correlation is zero, the best you can do, given that a person makes any possible score on the test, is predict the mean value from the scale of performance on the job. When you do that, the proportion (percentage) of errors in your predictions will be the greatest possible. Any non-zero correlation between the two sets of scores reduces the proportion (percentage) of errors in your prediction of job performance from test scores. For any given correlation between the two sets of scores, the proportion of reduction in errors of prediction is given by (1 - k); the percentage reduction is (100 * (1 - k)). For r = 1.0, k = 0, and the proportion of reduction in errors is (1 - 0) = 1.0; for r = 0, k = 1.0 and the proportion of reduction in errors is (1 - 1) = 0.

For the correlations reported by Jeff, r = .03, k = .954, and proportion of reduction in error of predictions = 1 - .954 = .006; for r = .6, k = .8, and proportion of reduction in error of prediction = 1 - .8 = .2. Here, we are talking about error-laden predictions of who will do well, or poorly, on a job. We are talking about psychologists using inadequate predictors, then defending those predictors as "good" and "beneficial to humanity." Nasty business, that. Harmful to more people than it helps.

Number salad. Oil and vinegar, anyone?

> What Tom is saying applies to the truth-value of assertions. I suppose there is a way of converting from "percent variance explained" to "probability that the hypothesis is true." If you have two hypotheses, each with a probability of truth of 0.6, then the probability that both are true at the same time is 0.36. So if the hypotheses are each "probably true", any conclusion that is drawn from assuming they are both true is "probably false." This is what dooms a psychology built on low-probability statistics to remaining a simple science, if it can be called a science at all.

Doom and gloom, and not a minute too soon.

> Of course any hypothesis that explains only 9% of the variance is "probably false" to begin with.

And that's the truth. ;-))

Later, Tom

Date: Thu, 4 Aug 1994 17:44:29 -0400
Subj: free will

[Jeff Vancouver 940804]

Mary, Bill P, & Bill L.

I think that covers it. My e-mail is down, so I am not fully functional here (as if I ever was).

What I am trying to understand with my free will discussion is these fuzzy concepts that Bill P. used like "decide" and "want." If ultimately the organism is attempting to maintain its intrinsic signals, then how can an organism decide to die? That is, decide to commit suicide?

This is just the pen-ultimate example. For me, this is not a problem in PCT because, in fact, the intrinsic signals refer to much more specific things than "life". Nonetheless, the idea that the organism can override all the intrinsic signals that are going off during a hunger strike is difficult to explain.

What I am looking for (P) is mechanisms that 1) allow beliefs to influence the choice of a reference signal, and 2) mechanisms of ignoring or toning down error signals (particularly from intrinsic and higher-order loops). For the first mechanism, I need more than simply to view beliefs as a kind of perception (Marken). That is, where in the hierarchy is a belief type loop, and how does it affect output functions. For it seems that the decision mechanism is at the output function. An error signal goes to an output function that has more than one set of reference signals available to it (e.g., "I could hit this guy or just smile and pretend to ignore him"). This condition evokes a thinking mode (switching output and input gates), the results of which determine the choice and give us the experience of some level of self efficacy and valuing. Free will, if it exists, which I doubt, is at the choice point. At least the experience of free will (e.g., "I choose not to be the type that hits people, no matter how offensive they are.")

The all-else equal/doesn't matter condition strikes me as very unlikely. There are too many loops involved. Every choice one makes will adversely affect higher-order perceptions, at least for an adult. Hence (2).

In terms of (2) - toning down an error signal, or preventing one from happening - may relate to the lag of the loops. Is this experienced as "I just did not think of that at the time?" But what is the experience "I swallowed my pride and just walked away"?

I am trying to reconcile subjective experience, which influences Locke and Bandura, with PCT. The subjective experience must be just as explainable as behavior. It is just a matter of translating PCT to explain this experience (and the causal appearance of this experience on behavior).

I guess I will address the testing stuff later, except to thank Bill P. and L. for correcting Tom.

Later Jeff

Date: Fri, 5 Aug 1994 17:19:00 CST
Subj: Re: free will

Tom Bourbon [940805.1702]

>[Jeff Vancouver 940804]
. . .

> I guess I will address the testing stuff later, except to thank Bill P. and L. for correcting Tom.

I'm eagerly watching for your next comments about the goodness and usefulness of psychological testing when it is used as a crutch to justify major decisions about the lives of people who are required to take the tests.

Jeff, you must be referring to the post in which Bill P. corrected what he mistakenly thought were statements by me about the additive or mulitplicative properties of variances. As I indicated in Tom Bourbon [940804.1519], I was talking about the *proportion* *of* *variance* *"explained"*, not about variances per se. The distance from proportions to probabilities is not as great as that from probabilities to variances.

Besides, the onus is still on you to defend the use, by psychologists, of pre-employment tests that correlate no higher than .3 or .6 with performance on the job. Above all else, I am curious about why you think testing as poor as that has anything significant to add to the theory of behavior in PCT. In behavioral science, correlations that poor should be viewed as sources of *noise*, not as sources of *facts* to be explained. For correlations that low, there are no "facts" to be explained.

Later, Tom

PS Copies of the papers you requested are in the mail to you.

Date: Mon, 8 Aug 1994 13:23:54 -0400
Subj: Re: useless Psych

[from Jeff Vancouver 940808] >Tom Bourbon [940805.1702]

> I'm eagerly watching for your next comments about the goodness and usefulness of psychological testing when it is used as a crutch to justify major decisions about the lives of people who are required to take the tests.

I am not sure people are using it the "justify" major decisions, but they do help provide information for those decisions. This seems to be one of the differences in our debate. You describe the often inaccurate attributions lay individuals give to the meaning of the test scores. I am referring to careful use of the scores.

> Besides, the onus is still on you to defend the use, by psychologists, of pre-employment tests that correlate no higher than .3 or .6 with performance on the job. Above all else, I am curious about why you think testing as poor as that has anything significant to add to the theory of behavior in PCT. In behavioral science, correlations that poor should be viewed as sources of *noise*, not as sources of *facts* to be explained. For correlations that low, there are no "facts" to be explained.

I think I see our problem here. I am not suggesting these tests add anything significant to the theory of behavior. Quite the contrary, PCT can probably add something to the construction and use of these instruments. The instruments, because they are attempting to predict results of behavior in the future, will always be 1) statistical and 2) low by testing specimen standards.

Let me ask you, have you every hired anyone? Selected a roommate? Chosen an auto-repair shop? Voted for an politician? What information would you use/not use?

Later Jeff

Date: Mon, 8 Aug 1994 15:55:25 CST
Subj: Re: useless Psych

Tom Bourbon [940808.1544] >[Jeff Vancouver 940808]

>>Tom Bourbon [940805.1702]

>> I'm eagerly watching for your next comments about the goodness and usefulness of psychological testing when it is used as a crutch to justify major decisions about the lives of people who are required to take the tests.

> I am not sure people are using it the "justify" major decisions, but they do help provide information for those decisions. This seems to be one of the differences in our debate. You describe the often inaccurate attributions lay individuals give to the meaning of the test scores. I am referring to careful use of the scores.

Jeff, I don't really intend to come across as a "heavy" on this topic. I do, however, have some pretty serious problems with the uses of psychological "assessment instruments" *within* the profession, where everyone assumes the uses are careful. Test scores that correlate .3 and .6 with real life do not justify most of the conclusions reached by careful professionals, no matter how high their intentions might be.
. . .

> I think I see our problem here. I am not suggesting these tests add anything significant to the theory of behavior. Quite the contrary, PCT can probably add something to the construction and use of these instruments. The instruments, because they are attempting to predict results of behavior in the future, will always be 1) statistical and 2) low by testing specimen standards.

And it is precisely their very poor statistical nature that renders them inappropriate for reaching conclusions about individuals.

> Let me ask you, have you every hired anyone?

Yes.

> Selected a roommate?

Yes.

> Chosen an auto-repair shop?

Yes.

> Voted for an politician?

Yes.

> What information would you use/not use?

In every case you mentioned, the *last* thing I would think of using would be a "standardized psychological instrument." The MMPI for selecting a mechanic? The Wechsler Adult Intelligence Scale for selecting a senator? The Draw-A-Person Test for selecting a room mate?

Later, Tom

Date: Mon, 8 Aug 1994 17:19:00 -0400
From: Jeff Vancouver
Subj: Re: useless Psych

>Tom Bourbon [940808.1544]

>> Let me ask you, have you every hired anyone?

> Yes.

>> Selected a roommate?

> Yes.

>> Chosen an auto-repair shop?

> Yes.

>> Voted for an politician?

> Yes.

>> What information would you use/not use?

> In every case you mentioned, the *last* thing I would think of using would be a "standardized psychological instrument." The MMPI for selecting a mechanic? The Wechsler Adult Intelligence Scale for selecting a senator? The Draw-A-Person Test for selecting a room mate?

What did you use? Past experience? Past behavior? Typing speed (if you were hiring a secretary), position of a stereo (for the roommate). My guess is that you used some of these pieces of information. The role of the industrial psychologists is to see if one's belief about the relationship between these pieces of information (particularly past behavior) actually relates to future behavior (or in your words, future results). We look for matches between needs (e.g., a stereo) and measures of haves (imprecise though they may be). We try to get at the skills (e.g., reading) learned in experience (e.g., school) rather than relying on the simply having the experience.

I would never use the MMPI to select anyone for a job. I do not remember the Draw a Man test. I would like to look into the relationship between The Weschler and senate performance - I suspect we would find a relationship there. I would be more likely to want to know that than whether they have a family, which seems the criteria of choice these days.

The point is you must make decisions on imperfect information. If one can provide at least some information, which our net (Runkel) has shown might predict behavior, I might very much want it.

later Jeff

Date: Tue, 9 Aug 1994 17:06:47 CST
Subj: Re: useless Psych

Tom Bourbon [940809.1554]

Jeff Vancouver [940809?]:

> What did you use? Past experience? Past behavior? Typing speed (if you were hiring a secretary), position of a stereo (for the roommate). My guess is that you used some of these pieces of information. . . .

I don't think I used any "information" at all. (Maybe I did, and I just didn't realize that was what I was doing ;-) None of the things you mentioned seem to fit my idea of what "pieces" of "information" would look like.

> The role of the industrial psychologists is to see if one's belief about the relationship between these pieces of information (particularly past behavior) actually relates to future behavior (or in your words, future results). We look for matches between needs (e.g., a stereo) and measures of haves (imprecise though they may be). We try to get at the skills (e.g., reading) learned in experience (e.g., school) rather than relying on the simply having the experience.

Jeff, I don't argue what industrial psychologists say they try to do. That is not my "gripe." You began this line of discussion by describing tests that correlate .3 to .6 with performance on the job. Correlations that low don't allow one to improve very much over simply "playing a hunch" or "trusting to intuition" when it comes to hiring a new employee. On the down side, reliance on tests as poor as those *will* lead to many incorrect decisions that will be defended as correct, with all sorts of "scientific" justifications to support the defense. If there are tests that perform better than r = .3 to .6, they will improve the picture a little, but not very much -- not unless they correlate .98 or more with performance on the work place. When r = .98, % variance "explained" = .96, coefficient of alienation = .199, and % reduction in errors of prediction (compared to not knowing the correlation) = 80.1. Those are simple facts of correlational relationships and are not matters of my opinion, or of PCT the theory. . . .

> The point is you must make decisions on imperfect information. If one can provide at least some information, which our net (Runkel) has shown might predict behavior, I might very much want it.

When Phil Runkel (Casting Nets and Testing Specimens, Praeger, 1990 -- highly recommended to everyone) wrote about statistically-based surveys and experiments, he carefully showed that the results of such procedures *cannot* be used to say things about any particular individual. Period. No exceptions. End of subject.

I believe Phil would be saddened to learn that his work is cited in an attempt to justify applications of (poor) group statistics to the selection of individuals for employment.

Later, Tom

Date: Thu, 11 Aug 1994 15:59:53 -0400
Subj: hiatus

[Jeff Vancouver 940811]

I am leaving for a conference and vacation for 3 1/2 weeks, so I will not be responding. I do not want anyone to think I am ignoring them. I have comments for Bill L and Tom B. specifically, but no time before I leave (sorry a paper and presentation take precedent).

BTW, the presentation is on my application of PCT to Person-organization fit. Many would probably hate it; some might like it. I will be at the academy of management conference in Dallas. The presentation is 8:00 am if anyone is interested.

Marken, thanks for the Closed Loop. I sent two papers your way. There may be more in September.

See you after labor day. Jeff

Date: Wed, 14 Sep 1994 17:33:40 -0400
Subj: return of a fringe element

[Jeff Vancouver 940913]

Well, I am back from giving my paper and visiting family. Add a week of prepping for classes and the beginning of the term folderol and it comes to about a month off the net. That is a lot of catching up. I have been skimming the posts and have noticed a couple of things I should probably address.

First, the Ayn Rand thread was interesting. Upshot, she is very close to PCT in principle, but errors on some of the details. This is the basic argument I have been making to the Locke et al camp.

Second, regarding the work of Lord and associates. One of Lord's major errors is in placing a decision mechanism (function) between the comparator and the output function. It is an attempt on his part to include a DME within control theory. I give him credit for trying. Choices are made, incorporating that into the control theory model is paramount on my list of things to do. He took a stab at it.

Third, the issue of the use of statistics (conventional behavioral science methods) continues to bother many on the net. I had been advised to read Runkel and have gotten most of the way through it. There was one particular figure that hit home to the argument. Runkel discussed a "drawing" program that required the subject to make slight deviations with the mouse around a circle to make a cursor draw whatever is was the subject wanted. He noted that an examination of the circle or the deviations about that circle told one very little (or nothing) about the desired drawing. He showed a picture of a square with a triangle in it and a circle. Both were jagged, but their shapes were clear. He pointed out that if one examined the circle (which represent the actions of the individual) or the deviations from the circle, one would never be able to derive the square and triangle (which was the actual goal of the subject). A very interesting point, because much of psychology spends its time examining the deviations from the circle, thus missing the point of the behavior.

However, the square and triangle were not very well drawn. That is, the lines were not straight, but jagged. Runkel was not arguing that the desire of the subject was to draw a jagged square and triangle, it just happened that the mechanism for drawing was fairly difficult for the subject, so the results was jagged. For us in the applied side of psychology, the jaggedness of those squares and triangles is exactly the error we wish to study. We are interested in individual differences, training, or devices that related to the jaggedness of results. It is extremely important that we study the jaggedness of the squares and triangle and not the circle, but jaggedness we study.

Finally, to continue on the psychology as useless thread, all I am trying to say when it comes to using instruments to gather information for making selections, is that the information will reduce the probability of making a type I or type II error or both. That probability may be small, but it is type I or type II or both that we all abhor (which is worse depends on the decision and who one asks).

I also thought of a set of psychological experiments that many would not think useless or dangerous (although McPhail beat me to it with the Sherif studies). Specifically, the studies that Thurgood Marshall used to argue the Brown v. Topeka BoE case. They were the studies were the black children identified white dolls as better than the black dolls. He used those studies to argue that the black children's self-esteem (self-concept in today's parlance) was lower than whites.

I also always thought that Asch's conformity experiments were very interesting.

The paper I mentioned early was a presentation (15 minutes) at the Academy of Management Conference on fitting persons and organizations using a systems/cybernetic/control theory framework. I see those as subsets of each other. Anyway, 15 minutes is not long enough to say much and I did not get much of a response from the audience. The organizational theorists were more receptive than the organizational behaviorists. The discussant claimed to like it - "after getting through the jargon" - I had terms like input function, equifinality, and requisite variety in it.

I did interact with others at the conference who were sympathetic with PCT or some variation (most have only been exposed to Carver & Scheier, Lord, or some other "heretic"). I was exposed to more on action theory (aka, German psychology), too much TOTE, but some nice stuff. I chatted with Locke, asking him if it bothered him that his performance cycle (a positive feedback loop) had no negative loop (in the systems dynamics sense). He did not seem to care.

Anyway, back to trying to get tenure.

Jeff

(P.S. I got the closed loop articles but have not read them yet, thanks)

Date: Thu, 15 Sep 1994 10:18:39 -0600
Subj: Re: intensional; the beak of the finch

[From Bill Powers (940915.0830 MDT)]
. . . . .
Jeff Vancouver (940913) --

Welcome back. I hope I can persuade Mary to give a report on her attempts to communicate with Locke; you should find them interesting. . . . . . Best to all,

Bill P.

Date: Mon, 19 Sep 1994 10:22:53 -0700
Subj: Beyond the fringe

[From Rick Marken (940919.1015)] >Jeff Vancouver (940913) --

> Upshot, she is very close to PCT in principle, but errors on some of the details. This is the basic argument I have been making to the Locke et al camp.

Ah, but in those details is where the god of PCT lives. The difference between PCT and conventional behavioral science is based on one, tiny detail; a preposition. In conventional behavioral science, behavior is controlled _by_ perception; PCT shows that behavior is the control _of_ perception. Just a detail.

> incorporating that [DME] into the control theory model is paramount on my list of things to do.

Why? Doesn't it seem like the first thing to do would be to obtain some nice, clear, reliable data and THEN decide what kind of model might be needed to explain it?

> Third, the issue of the use of statistics (conventional behavioral science methods) continues to bother many on the net.

No. It's the MISuse of statistics that bothers us; in particular, using aggregate data as the basis for conclusions about individuals.

> systems/cybernetic/control theory framework. I see those as subsets of each other.

Me too. "Cybernetics" is a subset of "systems" because the latter is a type of perceptual variable and the former is a state of that type of variable (other states include "S-R", "information theory" and "control theory" itself). Control theory is the superset of both "cybernetics" and "systems" because it explains how and why people maintain (control) systems variables in states like "cybernetics" or "control theory".

Best Rick

Date: Mon, 19 Sep 1994 12:57:45 -0600
Subj: Re: Locke and PCT

[from Mary Powers 940919]

Jeff Vancouver 94013

Your comment on Locke's indifference to the implications of having a positive feedback loop was interesting ("he did not seem to care")

I will probably not hear back from him, because in his last to me he called me presumptuous (for suggesting that he should learn something about control theory before criticizing it). He said

> I do not plan to read the 1973 book you cite [BCP], partly, because based on what you have said in your letter, it will be more of the same, and partly because the title itself is invalid. People do not behave to control perceptions but to achieve values, ie, to live. In short, I believe that your premises are _fundamentally_ mistaken. Given this, there is little point haggling over details.

Maybe Bill should retitle the book "Behavior, the control of perception in order to achieve values". In any event, it should be noted that when Locke talks about control theory it is not just from ignorance - it is from deliberate ignorance.

Mary P.

Date: Tue, 20 Sep 1994 09:05:44 CST
Subj: Re: Locke and PCT

Tom Bourbon [940920.0835] >[Mary Powers 940919]

>>Jeff Vancouver 940913

> Your comment on Locke's indifference to the implications of having a positive feedback loop was interesting ("he did not seem to care")

> I will probably not hear back from him, because in his last to me he called me presumptuous (for suggesting that he should learn something about control theory before criticizing it). He said

Wonderful, Mary. Henceforth we cannot be accused of being nasty when we say that, on the subject of control theory, there is no reason to take Locke seriously. He is a popular dispenser of -- who knows what, but it is not control theory as a theory that explains the phenomenon of control.

We can add Locke to a growing list of experts on control theory who declined our earnest offers to communicate on the subject: Carver, Lord, Bandura. Can anyone else suggest other names for the list? How many more must we add before it becomes obvious why there is a lack of "bridges" between PCTers and other experts on control theory?

You quoted Locke:

> I do not plan to read the 1973 book you cite [BCP], partly, because based on what you have said in your letter, it will be more of the same, and partly because the title itself is invalid. People do not behave to control perceptions but to achieve values, ie, to live. In short, I believe that your premises are _fundamentally_ mistaken. Given this, there is little point haggling over details.

This beautiful piece (which deserves framing on the wall above every PCT modeler's computer) demonstrates that we have been right, all along, when we told people it was pointless to look for similarities between PCT and whatever that stuff is that Locke dishes out. When it comes to the phenomenon of control, it seems there is no point to looking for people whose ideas are "close, in principle," to PCT. After witnessing the antics of many almost-

PCTers, I am just about convinced there is no such animal as an expert on control theory whose ideas are close to PCT, "in principle." It has always turned out to be all or nothing.

> Maybe Bill should retitle the book "Behavior, the control of perception in order to achieve values". In any event, it should be noted that when Locke talks about control theory it is not just from ignorance - it is from deliberate ignorance.

It is noted. In my biased book, innocent ignorance is no vice, but the deliberate ignorance I have seen in many so-called "almost-PCT" experts is another matter. Thanks, Mary.

Later, Tom

Date: Tue, 20 Sep 1994 10:53:56 -0400
Subj: Details, Decisions, and Locke

[from Jeff Vancouver 940920]

First, I would like to congratulate Rick for a terse response. It facilitates discussion

>[From Rick Marken (940919.1015)]

>> Me

>> Upshot, she is very close to PCT in principle, but errors on some of the details. This is the basic argument I have been making to the Locke et al camp.

> Rick

> Ah, but in those details is where the god of PCT lives. The difference between PCT and conventional behavioral science is based on one, tiny detail; a preposition. In conventional behavioral science, behavior is controlled _by_ perception; PCT shows that behavior is the control _of_ perception. Just a detail.

I think that Ayn Rand is arguing against behavior is controlled by perception as well, but their are fundamental problems with many of her arguments that prevent good science. A detail that Locke is clearly missing (and I thank Mary Powers [940919] for another compact response) is that values are merely a type of perception (the perception one wishes to perceive). Both Locke and Rick seem think the differences in their models are much larger than I think. On the other hand, the differences in their approaches are fundamental. Locke believes in grounded research (inductive reasoning) and Rick believes in deductive reasoning. Again, though, both do more of the other than Locke wishes to admit (Rick admits his need for data to inform theory). In both cases, their approaches have served them well, in that it has provided them with a method to accomplish their goals.

Mary, I would not worry about convincing Locke directly. He does have a blind spot toward control theory. My goal is merely to convince many of those who use his theory that a more elaborate and well specified theory exists that can take them much further than goal-setting theory.

However, that more elaborate, well specified theory (PCT) needs to incorporate decision making:

ME:

>> incorporating that[DME] into the control theory model is paramount on my list of things to do.

Rick:

> Why? Doesn't it seem like the first thing to do would be to obtain some nice, clear, reliable data and THEN decide what kind of model might be needed to explain it?

I am not sure what constitutes "nice, clear, reliable data" without some theoretical filter for interpreting it. Reliability has it own theoretical precepts. Nice and clear require others.

The data/observation that compels me and others to explain DM is the observation that we make choices. Given 2 alternatives, we chose one. What this means with a PCT framework is important. For example, when does the individual go into thinking mode to evaluate alternatives? How to higher-order control systems enter the equation? I could go on, but I am trying to be terse.

Me:

>> Third, the issue of the use of statistics (conventional behavioral science methods) continues to bother many on the net.

Rick:

> No. It's the MISuse of statistics that bothers us; in particular, using aggregate data as the basis for conclusions about individuals.

The applied researcher is much more interested in casting nets. What you label misuse, others label useful.

Me:

>> systems/cybernetic/control theory framework. I see those as subsets of each other.

Rick:

> Me too. "Cybernetics" is a subset of "systems" because the latter is a type of perceptual variable and the former is a state of that type of variable (other states include "S-R", "information theory" and "control theory" itself). Control theory is the superset of both "cybernetics" and "systems" because it explains how and why people maintain (control) systems variables in states like "cybernetics" or "control theory".

I got confused in the multiple uses of the phrase "control theory" in this passage.

Later Jeff

Date: Tue, 20 Sep 1994 11:32:15 -0700
Subj: Locke'd Out

[From Rick Marken (940920.1130)] Mary Powers (940919) --

> I will probably not hear back from him [Locke], because in his last to me he called me presumptuous (for suggesting that he should learn something about control theory before criticizing it). He said

>> I do not plan to read the 1973 book you cite [BCP], partly, because based on what you have said in your letter, it will be more of the same, and partly because the title itself is invalid. People do not behave to control perceptions but to achieve values, ie, to live. In short, I believe that your premises are _fundamentally_ mistaken. Given this, there is little point haggling over details.

As my daughter would say, it looks like you've been "dis-ed", Mary, my dear. I suppose it has never occurred to Locke that the "mistaken-ness" of your premises (and the correctness of his) could be tested by experimentation.

> it should be noted that when Locke talks about control theory it is not just from ignorance - it is from deliberate ignorance.

Beautifully put.

I forget who this Locke fellow is. It seems to me that there is this bunch of people, including Carver, Scheier, Locke, Karoly, etc who are fighting over the virtues of variants of a "control theory" model of human nature. But since they all seem to be clueless about 1) the nature of behavior 2) the nature of control 3)the nature of control theory 4) the nature of modelling and 5) the nature of science itself, it's hard for me to remember who is ostensibly "for" and who is "against" control theory. To me, it looks like a confederacy of dunces.

Best Rick

Date: Wed, 21 Sep 1994 07:03:29 -0600
Subj: Decisions; self-esteem

[From Bill Powers (940921.0400 MDT)]

Jeff Vancouver (940913) --

> One of Lord's major errors is in placing a decision mechanism (function) between the comparator and the output function. It is an attempt on his part to include a DME within control theory. I give him credit for trying. Choices are made, incorporating that into the control theory model is paramount on my list of things to do. He took a stab at it.

My problem with a DME is that I don't know what it is supposed to do. You present me with a piece of candy and a dime and tell me I can take one or the other but not both. After I pick up one of them, I am said to have made a "decision" or a "choice." But that is only the outcome; what is the process? How did I get from the state of looking at both items to the state of having picked one of them up? Can you describe that process without using "decide" or "choose" or other synonyms?

"Decision" and "choice" are words we learned when we were very young. They are "natural language" terms with no formal meanings -- that is, meanings derived from any systematic understanding, any model. Like most such terms, we use them without giving much thought to them; they seem too obvious to define further. We are just supposed to know what they mean.

As a parallel problem, consider the term "comparison." We all know that a control system needs a comparator, a way of comparing the perceived state of affairs with a reference state. We all know what "compare" means. But how do we construct a comparator for a computer model of a control system? If you look at the instructions available in any low- level programming language for any computer, you will not find an operation called "compare." Of course we all have a natural-language meaning for the term; if I show you a lemon and a grapefruit and ask you which is larger, you compare one size with the other and pick the larger. But how did you do that? What operation did you perform on the two sizes that resulted in knowing which was the greater? There is only one operation you can find in the computer that will create this result, and that is _subtraction_. You form a perception of size for each one, subtract one size-measure from the other, and assign the terms "larger" and "smaller" according to the sign of the result. Are there any other operations that will qualify as "comparison?" Perhaps there are some logical operations, but as far as I can see they reduce in the end to subtracting one measure from another. And I haven't found a case where this is not true. That doesn't mean that no such case exists; I simply haven't found it.

So wherever we see the term "compare," we can substitute "subtract" after a suitable analysis of the situation. Now we have a meaning for compare that we can put into a model that will run on a computer.

What I am asking with regard to "choose" or "decide" is what operation I need to program into a computer in order to do these things. If there is no such operation, then I need a definition of what the operation should be, whether or not it can run on any existing computer. If there is such an operation, then we can hope to incorporate choice and decision into a working model because now we know what operations to perform that will lead to having made a decision or choice.

The only such operations that I have ever seen proposed are arithmetical or logical in nature. You consider alternatives, you weigh them against each other, and you subject them to some rational process that results in an outcome. But as soon as you lay out all the details in this way, there is no longer any decision or choice to be made. You consider that you could spend the dime on something you like better than the candy, but also that the candy is right here and requires no trip to a store to buy a dime's worth of something else. You compare the relative effort involved and let the course of action that involves the least energy expenditure in comparison with the expected satisfaction take place. There is no decision to be made, once you have picked the grounds on which the decision is based. That is, "decision" involves nothing beyond perceiving the elements of the situation and applying an algorithm that yields an answer based on the elements and logic or arithmetic.

If that is all that decision means, then we don't need the term decision except as a convenient shorthand. And we don't need to add anything to the PCT model, because all the processes and perceptions involved in making a decision are already part of the model, with a specific level already assigned to doing the operations that result in decisions. The only thing that would make us add a separate box labelled "decision" would be to find something in the process that can't be explained by applying rules or algorithms to perceptions. Of course if "decision" means something else, then we have to specify what that something else is, and we have to add a level to the model capable of doing that sort of thing.

PCT resulted from spending a lot of time, a very LARGE amount of time, analyzing common-language terms that are used to describe behavior and experience. It was during this process that I realized that there were large numbers of words that I used every day without ever having asked myself what they are supposed to mean. I used to think of myself as quite the intellectual; I could argue on any subject with anybody and as far as I was concerned, win. Words, words, words, and none of the important ones had any meanings that I had thought through. When I think now about some of my youthful pontifications, I want to avoid meeting anyone who knew me then.

It seems to me that Locke, Bandura, Lord, and others in their field rely almost exclusively on words with no meanings that have ever been thought through. They throw common-language terms around as if merely saying them is enough. To pick a central term, what is a "goal?" This word is used as if everybody knows what it means, but none of the writers who use this term ever stops to ask what a goal is, how it can have any effects, why the actions we perform have the result of achieving a goal, how we know when there is a "goal-discrepancy."

Bandura is very proud of the term "self-efficacy", but what does it mean? It's a bastard construction to begin with, which leads to confusion with the forms of "self-starting" and "self-explanatory" and the like. Does it mean "efficacy of the self in creating actions?" "Efficacy of actions in producing effects pertaining to the self?" And what does "efficacy" mean? What does "self" mean? When you hear sentences constructed with terms like these you get a sense of knowing what the speaker means, but it is a very vague sense and subject to much idiosyncratic interpretation. The discourse is composed exclusively of unanalyzed natural-language terms. As long as you just let the words flow by, you get a feeling of understanding what the speaker is going on about, but the moment you pause to examine any part of it, and ask what it means, the sense of meaning disappears. Self-efficacy is simply a conjunction of two terms, one sort of referring to the aspect of experience we call ourselves, and the other sort of meaning being able to have effects or be effective or accomplish things. It's much like saying "food nice." Say self. Say efficacy. Now say them together. Get it?

PCT puts a layer of formal meanings underneath the natural-language terms in which we talk about behavior and experience. Of course these are _proposed_ meanings, and are always open to modification. But when we talk in ways that have no such layer of formal meanings beneath them, we are just talking; we aren't being theorists. We're conveying meanings as best we can without having any formal understanding of what terms are to mean. This is how people naturally converse with each other, how children talk and how adults talk who have never thought deeply about what their own words mean to them. It is somewhat miraculous that people are no worse at grasping what others are talking about, and at conveying to others what they mean, than they are.

Science can happen only when people get together and say "Now look, we just have to reach some agreements about what certain terms are going to mean." When they actually get down to doing this, they find that they were far less in agreement than they thought they were. They find that they have to work their way down and down through layers of language until they can find common experiences so simple that misunderstanding is next to impossible. "I say that meter reads 3.0." "Well, I say it reads 3.1." "Move over so you're looking straight down at it." "OK, it's 3.0." Once that's settled, they can start working out the question of 3.0 _whats_.

Rick Marken's example of the three blind men and the elephant has another side to it. The three blind men, if they just rely on words, can think they are talking about the same thing when they're really talking about different experiences. The one at the tail says "I feel a cylindrical appendage -- let's call it a pachyoid." The one at the legs says, "OK, right, I feel four pachyoids," and the one at the trunk says "There's another one here." When they write their report, they say that elephants are made of six pachyoids. And they go their separate ways thinking they are talking about the same thing just because they are using the same word.

New subject:

> I also thought of a set of psychological experiments that many would not think useless or dangerous (although McPhail beat me to it with the Sherif studies). Specifically, the studies that Thurgood Marshall used to argue the Brown v. Topeka BoE case. They were the studies were the black children identified white dolls as better than the black dolls. He used those studies to argue that the black children's self-esteem (self-concept in today's parlance) was lower than whites.

This illustrates another problem I have with much (not all) psychological research. What the studies you cite showed was that black children treated white dolls as being better than black dolls. From this, it is informally inferred that therefore black children think that white children are better than black children, and from that we conclude that black children consider _themselves_ worse than white children, and therefore that black children have lower self-esteem than white children.

All that seems reasonable enough to base a decision on in the terms in which most decisions are actually made, but it's not enough to call it science. The problem is that _plausibility_ is substituted for _proof_. If the proposed logical connections were actually valid, then the final conclusion would be valid. But not otherwise.

Proof in this case would amount to finding some independent way of determining the black children's degree of self-esteem. This is the only way to validate this procedure for determining self-esteem. Before this procedure is applied to a general population, we must establish that when black children say white dolls are better, they are also experiencing low self-esteem. We can see which dolls they choose, but how can we see their self-esteem? What we're trying to establish is that the choice of dolls, which is easy to measure, is a reliable indication of self-esteem, which is not so easy to measure. If we could establish that connection, we could then substitute the easy measure for the hard measure in testing a general population. So the problem of showing that the test means something boils down to measuring self-esteem in some direct and independent way.

But here we are stymied. We can't see the self-esteem directly, unless we happen to be the particular black child under study, and have the sophistication to think in such terms. We can't just ask a three-year- old child how much self-esteem he or she feels; that's a meaningless term to a three-year-old.

The only recourse is to try to devise a completely different test for self-esteem. We see how black children behave when getting into lines with white children. We see how "assertive" they are in competing for toys with white children. We see what roles they play in pretend games. But in each case we run up against the same problem: there is no direct measure of self-esteem available for validating any of the tests. In each case, we simply assume that the behavior we can observe is indicative of self-esteem.

So what do we end up with? A series of tests which are validated against each other, and none of which is actually known to indicate self-esteem. This thing called self-esteem remains hidden from observation; in fact we don't know what any of these tests is indicating, if anything.

This problem became evident to me when I began taking psychology courses in college. And it led me to wonder about something that psychologists didn't seem to care to think about: did the things that these tests supposedly measure exist at all? In the above example, I would have asked, is there really such a thing as self-esteem? Is this something that people have? Or is it just a way of talking?

Now I think I would say that yes, there is such a thing as self-esteem, but that term is only a rough indicator of what is involved. To get any closer to a useful understanding, we have to ask what "self" means, and what "esteem" means, so we can see some sort of process going on. Obviously, self-esteem is a perception that can be had only by the person involved. It is a perception of one's own characteristics. And this perception, to have any value, must be compared with a reference level for the each state of those characteristics one has chosen as a target. If you perceive yourself as less than you want to be in each relevant respect, then you are probably experiencing what people call low self-esteem.

But there is another side to this, which becomes visible only when you use the PCT model. What if your perception of your self matches the reference level with respect to each variable involved in characterizing yourself, but you have set your reference levels very low for characteristics that other people usually set to a high level? Your perception of self-esteem would then be exactly what you have chosen in each respect. It would not seem low to you, but others would still judge you as having low self-esteem. You, as a black child, would go behind the white children in the line, hand over a toy to a white child without protest, and play the black victim rather than the white victor in pretend games. Perceiving yourself doing these things would fit perfectly with your reference levels for how you should be.

How could such reference levels come to be set so low? Obviously, because these are the settings that work, all things considered. If you are raised in a society that punishes you for assuming equality or ascendance in the company of white people, you can avoid that punishment by choosing ways of being that do not cause punishment. You remember how you were when you were getting along as well as possible, and you pick those memories as reference levels. This does not require any thought; all it requires is reorganizing until the pain is minimized.

So now we are beginning to get a handle on self-esteem. We can distinguish between a person who perceives a self that is less than what is wanted in various dimensions, and a person who has come to want less in these same dimensions. And while we're at it, we have arrived at some definitions of the dimensions that pertain to self-perception. With this understanding, we're ready to apply control theory to the problem of determining a person's self-esteem.

The procedure goes this way. You pick a person, and start interacting with that person. What you're looking for are controlled variables that pertain to self-perception, as you define it. You see how this person reacts to disturbances of the variables you have chosen; if there is no reaction, you strike that variable from the list. Eventually, after a lot of interaction, you arrive at definitions of variables relevant to self-perception, as you see it, that you can demonstrate to be under control by this person. And you also find the reference levels relative to which each variable is controlled, so you can tell if low self-esteem is due to a large error or to a low setting of the reference levels.

Then you pick a second person and go through this procedure again. And a third, and a fourth, until you have done all the people in your test group, one at a time. By the end of this time you will know 50 or 100 or 200 people very well indeed, with respect to what you see as their self- concepts.

Then you can start looking for common factors, for variables that indicate common problems, for deviations of individuals' reference levels from those of most of the group. You look for indirect indicators that show highly reliable correlations with either felt lack of esteem or low levels of aspiration for esteem. Now you have a direct measure of self-esteem, as you have carefully defined it, for a large enough group of individuals to allow validating other kinds of tests that are easier to apply. Of course these other tests will NEVER yield the kind of reliability that the basic study of one person at a time will yield, and no important decisions should be made about any one person on the basis of such tests. But such tests can be used as the basis for modifying policies that apply to many people or for assessing the overall effectiveness of a program, a teacher, or an administrator.

Compare where we started with where we ended. We began with children playing with dolls, and we ended with a study of individuals aimed at discovering how their self-concepts compare with their desires, and what those desires are. We started with a vague common-language term, self- esteem, and ended by talking about specific variables associated with self-perception. Which approach is going to yield solid knowledge of how people perceive themselves, how children learn to perceive themselves? I submit that the answer is obvious.

It's much, much easier to hand out a set of questionnaires to 30 people than it is to sit down with each one of them for hours and hours, learning how each person ticks. There are pressures of time and competition and pride and funding that make psychologists look for quick and easy ways to get information about important aspects of human nature. But the results reflect the amount of effort and thought expended. If anyone wants to conduct psychological investigations in a way that will yield reliable knowledge about human nature, there is really only one way to start: one person at a time. Without that kind of basic understanding, all generalizations are fuzzy and empty and hardly worth the trouble.

Best, Bill P.

Date: Wed, 21 Sep 1994 08:55:41 CST
Subj: Re: Details, Decisions, and Locke

Tom Bourbon [940921.0806]

Breaking in on Jeff's reply to Rick. Out of respect for Jeff's expressed preference, I'll try to be "terse."

>[from Jeff Vancouver 940920]
Re: [From Rick Marken (940919.1015)]

. . .

Jeff:

> . . . Both Locke and Rick seem think the differences in their models are much larger than I think. On the other hand, the differences in their approaches are fundamental. Locke believes in grounded research

(inductive reasoning) and Rick believes in deductive reasoning. Again, though, both do more of the other than Locke wishes to admit (Rick admits his need for data to inform theory). In both cases, their approaches have served them well, in that it has provided them with a method to accomplish their goals.

Yes, Jeff. Locke believes in, and stakes his reputation on, research using the traditional experimental designs in behavioral science. The kind of research in which the experimenter creates Hypothethetical Nomothetic Androgynous Persons (HYNAPs) -- Neutered Average Persons possessed of all manner of drives, traits, qualities and other causal demons-- then talks about then as though they (the HYNAPs and the demons) are real. The kind of research in which the "experimental hypothesis" is _never_ evaluated by the design or the analysis of data -- the occurrence of a "statistically significant difference" between the average HYNAPs in two or more groups of subjects in no way confirms the experimenter's hypotheses about the demons that made the hypothetical persons act as they did. The statistical procedures are constructed so that nothing whatsoever can be said about any specific person. That's one of the differences Rick was talking about.

Of course you are correct when you say Locke's use of that kind of research has served him well. It is the ticket into respectable mainstream "behavioral science." But does it _ever_ address the truth or falsehood of the experimenter's hypotheses? To be terse, no, never.

> Mary, I would not worry about convincing Locke directly. He does have a blind spot toward control theory. My goal is merely to convince many of those who use his theory that a more elaborate and well specified theory exists that can take them much further than goal-setting theory.

Brave soul! Good luck.

Jeff:

>>> incorporating that[DME] into the control theory model is paramount on my list of things to do.

Rick:

>> Why? Doesn't it seem like the first thing to do would be to obtain some nice, clear, reliable data and THEN decide what kind of model might be needed to explain it?

. . .

> The data/observation that compels me and others to explain DM is the observation that we make choices. Given 2 alternatives, we chose one. What this means with a PCT framework is important. For example, when does the individual go into thinking mode to evaluate alternatives? How to higher-order control systems enter the equation? I could go on, but I am trying to be terse.

Jeff, I don't think Rick was asking if people ever make decisions. It seems to me that he was asking if the occurrence of decisions in itself requires additions to or modifications of the PCT model. Have you tested the model to determine if, in its present form, it can produce the "phenomenon" of "making a decision?" Or do you simply like the idea of DMEs and want to include them in the model for reasons that are more aesthetic, or political, or whatever? You would not be the first to do such a thing. At one time or another all of us have probably done something like that.

Me:

>>> Third, the issue of the use of statistics (conventional behavioral science methods) continues to bother many on the net.

Rick:

>> No. It's the MISuse of statistics that bothers us; in particular, using aggregate data as the basis for conclusions about individuals.

> The applied researcher is much more interested in casting nets. What you label misuse, others label useful.

Yes. And when methods are used in ways which violate all of the underlying criteria that determine their appropriateness and relevance, that is also misuse and it is the reason we question or reject the majority of research in the social-behavioral-life sciences. The fact that traditional behavioral scientists are "interested in" their work does not alter the fact that they often (practically always?) violate the rules that might make their work legitimate as a kind of net casting. (Phil Runkel speaks eloquently to this issue.)

We play our different games.

The rules of terse talk require that I quit.

Later, Tom

Date: Wed, 21 Sep 1994 11:35:56 -0400
Subj: Locke, all or nothing

[From Jeff Vancouver 940921]

to [Tom Bourbon 940929.0835][Rick Marken 940920.1130 & .1800]

I am glad to see that you all have the corner on truth. I suppose we should all take the perspective that our way and our understanding is the correct one. We would not be bothered by gaps in our understanding or differences of opinion.

I could argue that you are as guilty as Locke for deliberate ignorance, but you will say you have read (at least some) of their stuff. Of course, so has he, and he did not fully grasp it.

I could argue that your goal (developing a model of humans) and theirs (developing applications to help individuals and organizations) are slightly different and require different methods (casting nets v. testing specimens to use Runkel's language), but you seem to be ignoring that argument.

I could say that if you don't have something good to say, don't say anything. But I do not even believe that (or I would not be writing this response). I prefer, if you don't have something constructive to say, do not say anything. But you will respond that you do have something constructive to say, namely, don't waste your time looking at the work of these researchers. Yet, that is the deliberate ignorance you just railed against. Ah, we come full circle.

Einstein did not believe in quantum physics. I tend to agree with him in principle, I do not believe uncertainty is a physical property (although I am no theoretical physicist). Nonetheless, much has been achieved both practically and theoretically by adopting a quantum framework. If you believe Hawking, the combination of general relativity and quantum physics will lead to the end of physics because all the questions will be answered. Hawking has certainly made contributions combining the perspectives.

I see science as a big hierarchical control system. Conflicts between reference signals should be addressed, not ignored. Otherwise we are wasting resources. I don't think everyone needs to be addressing the conflicts, but do not begrudge those of us that attempt it.

Later Jeff

Date: Wed, 21 Sep 1994 11:28:17 -0700
Subj: Terse Replies

[From Rick Marken (940921.1100)] Jeff Vancouver (940920)

> First, I would like to congratulate Rick for a terse response. It facilitates discussion

Thanks. For you, it will be terse all the way.

> Locke believes in grounded research (inductive reasoning) and Rick believes in deductive reasoning.

Could you tersely explain what this means?

> In both cases, their approaches have served them well, in that it has provided them with a method to accomplish their goals.

True. My goal is understanding control; Locke's goal is getting professional recognition.

> However, that more elaborate, well specified theory (PCT) needs to incorporate decision making:

It already does.

> I am not sure what constitutes "nice, clear, reliable data" without some theoretical filter for interpreting it.

There are many examples in my "Mind Readings" book and in Bourbon and Powers' "Models and Their World's" article.

> The data/observation that compels me and others to explain DM is the observation that we make choices.

But a choice-making mechanism may not be needed to explain this. People can also be observed responding to stimuli but an SR mechanism is not needed to explain this.

> The applied researcher is much more interested in casting nets. What you label misuse, others label useful.

Yes. And what some label "shit", others label "caviar". But it is still what it is. The goal of applied PCT is to improve individual control. I can't see how this goal can possibly be achieved by "casting nets".

Tersely Rick

Date: Wed, 21 Sep 1994 18:54:25 -0700
Subj: Locke, stock and barrel

[From Rick Marken (940921.1900)]

Ah, I am getting posts on the day they were posted, for the time being, anyway.

Jeff Vancouver (940921) --

> I could argue that you are as guilty as Locke for deliberate ignorance, but you will say you have read (at least some) of their stuff.

Not only that, but the two articles you sent are right here in front of me.

> if you don't have something constructive to say, do not say anything. But you will respond that you do have something constructive to say, namely, don't waste your time looking at the work of these researchers. Yet, that is the deliberate ignorance you just railed against.

In fact, I say "look at the work of these researchers carefully and you will see why, what they are doing, has nothing to do with understanding purposeful behavior". What is wrong (from a PCT perspective) with the work of Locke, et al is what is wrong with conventional psychology in general. The PCT literature is one long, careful, model and research-based explanation of why the kind of research and "theory" found in Locke's articles tells us nothing about the nature of control. Believe me, if there were anything of even the slightest value in Locke's papers -- anything that could be used to leverage a PCT based approach to research and theory in this area (which seems to be called "goal setting")-- I would run with it immediately. But there isn't; in fact, there isn't even anything to argue against (with models and research); it is just, plain irrelevant.

The only thing that distinguishes Locke's work from the rest of the research in conventional psychology is his attempt to use the vocabulary of PCT (or, at least, of control theory). But it is all just words; there are no tests for controlled variables, no one-person-at-a- time research, no working models of control, nothing but the SOS (same old fecal material). This has nothing to do with access to "the truth". It has to do with understanding the nature of purposeful behavior, how to model it and how to study it. Locke et al are clueless about this -- as is most of the rest of the conventional psychological community. Clearly, these people are happy with the kind of research they are doing and what they are learning from it. I'm glad that they are happy. You are the one who seems to think that this research has some relevance to PCT, or vice versa. We have been trying to tell you (with clearly diminishing levels of patience on Tom and my part) that this is simply not true. You don't seem to agree with us, so, fine. I think we're pretty explained out.

Tom and I get impatient with your claims about the relevance of Locke to PCT, not only because we have bad values (but I promise to peek though Bill Bennett's book so that I can learn the right values to have) but because we have been through this 1000 times before, with reviewers, psychologists and even some people in CSG who are sure that we can and should build "bridges" to those doing work that seems "close" to PCT. It just doesn't work. PCT is PCT; it directly contradicts (or differs completely from) every other model of behavior that has ever been proposed (that we are aware of).

> Conflicts between reference signals should be addressed, not ignored. Otherwise we are wasting resources. I don't think everyone needs to be addressing the conflicts, but do not begrudge those of us that attempt it.

But there is no conflict. The articles by Locke that are sitting in front of me have nothing to do with what I am controlling for; an understanding of the nature of purposeful behavior through research and modelling. The only thing in these articles that could be of any use to PCT is the data and models. There are no models (except for an incorrect diagram of a control system) and the data is useless because it is a summary of group performance and is extremely noisy at that.

I feel no conflict with Locke et al; we are clearly not trying to control the same variables. His research is no more of a disturbance than most of the other work in conventional psychology. It is completely and utterly irrelevant. The only disturbance occurs when people (like you) say that it is relevant. It ain't. You seem to think we (Tom and I) are in conflict with Locke; that is wrong. What we are saying is that Locke's work is useless to us -- as useless, it appears, as ours is to him.

Untersely Rick

Date: Thu, 22 Sep 1994 08:35:29 CST
Subj: Re: Locke, all or nothing

Tom Bourbon [940922.0833] >[From Jeff Vancouver 940921]

>to [Tom Bourbon 940929.0835][Rick Marken 940920.1130 & .1800]

I read your post, Jeff. This will be terse.

Read [From Bill Powers (940921.0400 MDT)], in which you will see once again the way PCT modelers go about the business of trying to understand human behavior and to help them. After you read those ideas one more time, I hope you will give some thought to abandoning your ad hominem style when you are on this net.

Later, Tom

Date: Thu, 22 Sep 1994 10:28:07 -0400
Subj: Locke tersely

[from Jeff Vancouver 940922]
[Rick Marken 940921.1100 & 1900] & [Tom Bourbon 940921.0806]

Deductive reasoning is when the theorists focuses on the model and makes inferences about the observations (i.e., reasoning from the general to the specific). Inductive reasoning is when one focuses on observations and attempts to draw general premises (i.e., reasoning from the specific to the general).

When you (Tom & Rick and others) focus on creating your simulation and _then_ testing it against observation, you are doing deductive research. A very legitimate enterprise, particularly in the long run.

When Locke and others are collecting data and then trying to make inferences of the underlying causes, they are doing inductive research. Very useful at early stages of phenomenon examination, but limited in the end.

When one has a good model from which to base deductive research, they should do it until their hearts are content. You have such a model and that is what you are doing. Great. You really do have little reason to bother with the others' work (except, as Runkel maintains and I concur, to provide clues for controlled variables; I also believe there are gaps that could use some help from other places, but you have located the most useful places - like perception psych). I do think you are wasting _your_ time keeping up on the likes of Locke.

When you have a flawed model, like Locke's, deductive research is often a less than useful and can lead one to believe deductive research as a method is flawed. I think this is one of the things that is going on in Locke's head. Ironically, Locke's model is close to being like your model (actually, not so ironically, because the observations are of the same phenomenon, so that the models are similar is not surprising). But close does not cut it, _for the purposes of understanding control_. Probably true.

But you also note that it is difficult to get tenure doing the PCT thing (deductive research using the PCT model). This is because the larger scientific community (or at least a critical mass) has not accepted aspects of the model, the method, or the attitude of the proponents of PCT. So what does one do? 1) Give up convincing the scientific community if they can afford to, or 2) try the convince some of them. You have chosen the former because you can afford to (or perceive that you can, which I do not question). I however, cannot afford to (or perceive that I cannot). Thus I am trying to convince.

All I am asking is that you do not make my job harder than it already is!

Obviously, their are other differences in our position than this (I am much less negative about averaging across people, etc.), but you have made these points (to me anyway) and we are agreeing to disagree.

One final point, I found Mary's post about Locke very informative. I do not want to stifle that kind of post. I gives me a better appreciation of where the distractors are, what kinds of arguments might work, what might not, and who might not be worth trying to convince. For example, I know the method arguments will not work very well. I still make them, but put the thrust of my arguments elsewhere.

I will address Bill's post regarding DM and self-esteem after getting some work done.

Later Jeff

Date: Thu, 22 Sep 1994 12:26:40 -0600
Subj: PCT and its critics

[From Bill Powers (940922.1035 MDT)] Jeff Vancouver (940921)

> I am glad to see that you all have the corner on truth. I suppose we should all take the perspective that our way and our understanding is the correct one. We would not be bothered by gaps in our understanding or differences of opinion.

Anyone who understands the PCT model is welcome to tear it to shreds if they see something wrong in it. Unfortunately, the people who try hardest to tear it to shreds are those who have given it the least study and who, as a consequence, have leaped to a lot of erroneous conclusions about it. They're criticizing a creature of their imaginations, not PCT. But PCT theorists suffer the effects nonetheless when they try to make their views known.

I had written to Locke some time ago; with his response he sent a preprint of a paper titled "The Emperor is Naked", to be published in _Applied Psychology: an international review_. Here is his theme paragraph from page 3:

> I must conclude at this point that control theory, as presented by those who claim to be control theorists, is so diverse in meaning, so all-encompassing in scope, and so devoid of specific, consistent content that it is everything in general and therefore nothing in particular. That which is nothing in particular is: nothing. I feel obliged, therefore, to play the role of the little boy in the children's story and declare the emperor to be naked. I believe we would do well to abandon control theory altogether.

What Locke terms control theory is what he has picked up from reading Carver and Scheier, Hyland, Lord, and others writing in his own field. With that understanding, I tend to sympathize with his opinion. It would be better not to mention control theory at all than to present it in a distorted and superficial way. The outcome of such presentations was evidently not impressive to Locke and others in his coterie. I was not impressed, either.

However, PCT now stands discredited by a major player in the field of personality research (if that is the right term for Locke's field). If past experience is any guide, we can predict what will now happen if a PCT researcher sends a paper to Applied Psychology. The referees, having read what Locke and Bandura and Binswanger have said about control theory, will realize that control theory has no content and has been abandoned by right-thinking scientists; they will cite these scientists, add a few irrelevant criticisms, and reject the paper. With luck, the referees will not be Locke et. al. themselves, but if they are not, they will still have no knowledge of control theory of their own, and will take the word of experts in their field. Since few scientists read much if anything outside their own journals, that will be that for PCT -- once again.

Jeff, don't forget that you have just started learning PCT. You have a year or so to go before your understanding of it will be complete enough to see what is wrong in the way both Locke and the people he lambastes are treating it. It is to your credit that coming from a different field and feeling strongly challenged by PCT, you have stuck around to learn more and do your own thinking about it. But that simply puts you in the same position that nearly all current PCTers were once in. Nobody on this net was conscripted and forced to read all this stuff. Every person was self-selected and came from fields built on ideas that clash in one way or another with PCT.

Tom Bourbon and Rick Marken did not begin life as PCTers; they were both conventional psychologists who learned and taught all the things that conventional psychologists learn and teach, including statistical methods. They were granted PhDs by mainstream psychologists. Rick Marken even wrote a textbook on statistical methods in experimental psychology. Almost all of the old PCTers except me were once just like you, your brethren in psychology and sociology and so on. All of them had the same problems you are having, accepting parts of PCT with great excitement but finding other parts in conflict with important things that they had accepted as truth.

PCT requires re-thinking essentially everything in the behavioral and life sciences, because it introduces a new kind of organization that was never part of those sciences. The phenomenon of control has always been present in the behavior of living systems, yet for at least 300 years the life sciences developed without one mention of it. How, we have to ask, did scientists manage to explain behavior while ignoring the most central aspect of it? The only way we can answer this question is to look at parallels in other fields. How did people explain the way ships disappear over the horizon while they believed the world is flat? How did they explain the movements of the sun, the moon, the planets, the stars, and comets while they believed that the universe revolved around the earth? How did they explain combustion while they believed that combustion required giving off phlogiston rather than combination with oxygen?

What's most important to realize is that THEY DID FIND EXPLANATIONS. Furthermore, the best scientific minds of the times found these explanations to be complete, plausible, and convincing. Scientists wrote in learned journals about these explanations, and applied the explanations to all sorts of aspects of life and nature. We know little about dissenters from these mainstream ideas; our experiences with PCT may give some insights into why. There were surely always dissenters, but dissenters do not write the history of science until they have prevailed. And then they, too, become the stodgy mainstreamers, fending off new dissenters.

Remember that life scientists DID FIND EXPLANATIONS of behavior without ever considering the phenomenon of control. This means that no matter how plausible their arguments seem, how water-tight their reasoning sounds, how much experimental evidence they have amassed, _there is something wrong with what they say_. They have explained behavior without mentioning its basic organizing principle. The more we learn about PCT, the more things about behavior that we see it explaining simply and clearly, the more obvious it is that the life sciences have not been playing with a full deck. A great deal of self-delusion must have been taking place. Important problems must have been ignored, explanations must have been offered which are as flawed as those the old flat-earthers used to explain why you can see only the topmast of a ship eight miles out to sea, yet can see the whole ship again if you just climb a hill. How simple it is to propose that the earth is a ball, and how stoutly people insisted that their own complicated explanations were better!

And how simple it is to say that organisms behave in order to control what happens to themselves. Yet look at all the complex, fuzzy, hard-to- test ideas that people have offered in place of this simple idea. Look at all the terms like motivation and aspiration and traits, tendencies, propensities, proclivities, and habits that have been offered as explanations. Look at the unending attempts to trace the causes of behavior back to external stimuli, to inborn characteristics, to influences from situations and events and cues, to complex mental calculations. Look at all the experimentation in which even the vaguest relationships are seized upon as if they gave us the secrets of life. The sciences of behavior have been floundering in confusion since they first began, all because they did not notice a simple, and now easily explainable, phenomenon.

The problems that PCT has had with conventional scientists are nothing new to science. We can only be encouraged that there are 150 or so scientists interested enough in PCT to keep tabs on what is going on, and even to participate in discussions about it and attend meetings about it. Who knows? There may be many more from whom we never hear, who are waiting to see which way the ball bounces before committing themselves. There are over 5000 copies of B:CP floating about somewhere in the world, not subtracting the ones hurled angrily into trash bins. Even though PCT has not figured prominently in the literature (it has been mentioned more often in refutation than in support), there is obviously some pressure felt in the scientific community from this idea. Why else would an author writing about something else insert a gratuitous objection to PCT in a paper? The only thing that stands in the way of widespread acceptance is the magnitude of the changes that are implied by PCT. It is not what PCT has to offer that is the problem; it is what has to be given up in order to understand it.

Best, Bill P.

Date: Thu, 22 Sep 1994 14:49:59 -0400
Subj: Re: Decisions; self-esteem

[from Jeff Vancouver 940922] >[Bill Powers (940921.0400)]

> My problem with a DME is that I don't know what it is supposed to do. You present me with a piece of candy and a dime and tell me I can take one or the other but not both. After I pick up one of them, I am said to have made a "decision" or a "choice." But that is only the outcome; what is the process? How did I get from the state of looking at both items to the state of having picked one of them up? Can you describe that process without using "decide" or "choose" or other synonyms?

I think one problem with this discussion the use of a special designate: DME. I think the question of DM is the question of which reference signal is passed down from the control unit that has as a reference signal "spend this dime."

Once that reference signal is determined, the process is the usually propagation of reference signals down the hierarchy (which might have decision points - I see eat candy, from which store do I get it?). Let's focus on one point in the hierarchy.

> The only such operations that I have ever seen proposed are arithmetical or logical in nature. You consider alternatives, you weigh them against each other, and you subject them to some rational process that results in an outcome. But as soon as you lay out all the details in this way, there is no longer any decision or choice to be made. You consider that you could spend the dime on something you like better than the candy, but also that the candy is right here and requires no trip to a store to buy a dime's worth of something else. You compare the relative effort involved and let the course of action that involves the least energy expenditure in comparison with the expected satisfaction take place. There is no decision to be made, once you have picked the grounds on which the decision is based. That is, "decision" involves nothing beyond perceiving the elements of the situation and applying an algorithm that yields an answer based on the elements and logic or arithmetic.

Couple of issues in this paragraph. First, "there is no decision to be made, once you have picked..." what is picking if not deciding. You are saying there is no decision once you decided.

Second, I have no trouble (as a working hypothesis) with "nothing beyond perceiving the elements of the situation and applying an algorithm that yields an answer based on the elements and logic or arithmetic." This strikes me as a reasonable description of the DM process. But the next question is which elements are perceived? What is the nature of the algorithm? That is, what are the parameters? Does the organism need to go into thinking mode to access parameters for the algorithm? Can individuals control for perceptions that cause (even in their own world view) other higher-order perceptions to go out of line (i.e., why are some perceptual elements not represented in the algorithm)? Where is the algorithm?

I think the answer to the last question is the output function and hence Lord's error. I think the structure for the answer is present in your model. It may be more fleshed out than I know because I have not read everything or forgotten some things. By the way, why do we forget?

I think that one of the elements in the algorithm, that is in the output function is an estimate of the probability of attaining a reference signal (what Bandura calls self-efficacy). I believe the value for that estimate is partially a function of self-concepts like self-esteem (more on that later) and perceptions related to achieving the perception in the past (via actual interaction or perceptions of others doing or saying it). I believe it is reasonable to attempt to influence those parameters. I believe Bandura and others have shown us the reasonableness of these propositions. I believe Bandura's model is flawed and incomplete and could be substantially improved by your model (with the appropriate changes to the Bandura's model and possible further specification of your model).

> It seems to me that Locke, Bandura, Lord, and others in their field rely almost exclusively on words with no meanings that have ever been thought through. They throw common-language terms around as if merely saying them is enough. To pick a central term, what is a "goal?" This word is used as if everybody knows what it means, but none of the writers who use this term ever stops to ask what a goal is, how it can have any effects, why the actions we perform have the result of achieving a goal, how we know when there is a "goal-discrepancy."

We spend a great deal of time haggling over the meaning, measurement, and even existence of our constructs. Sometimes this involves mathematical symbols, sometimes it does not. But we should not be fooled into believing that a greek symbol is any more closer to the truth than an English one.

> PCT puts a layer of formal meanings underneath the natural-language terms in which we talk about behavior and experience. Of course these are _proposed_ meanings, and are always open to modification. But when we talk in ways that have no such layer of formal meanings beneath them, we are just talking; we aren't being theorists. We're conveying meanings as best we can without having any formal understanding of what terms are to mean. This is how people naturally converse with each other, how children talk and how adults talk who have never thought deeply about what their own words mean to them. It is somewhat miraculous that people are no worse at grasping what others are talking about, and at conveying to others what they mean, than they are.

As I said above, we look for deep meaning more than you give us credit. I use your BCP chapter on theory in my motivation class. Just taught it yesterday. Your division of extrapolation, abstraction, and modeling I find useful, but your examples of personnel selection as extrapolation were off the mark (mostly, some instruments are purely empirically based). And the abstraction you describe you never get published in our theoretical journals. There needs to be at least one layer lower of why we should see the abstractions we hypothesize. But you are right about the modeling. We would do well to become that sophisticated.

> Science can happen only when people get together and say "Now look, we just have to reach some agreements about what certain terms are going to mean." When they actually get down to doing this, they find that they were far less in agreement than they thought they were.

Often that is true about the definitions of their constructs, but once passed that they often find the processes they were talking about were the same.

> New subject:

>> I also thought of a set of psychological experiments that many would not think useless or dangerous (although McPhail beat me to it with the Sherif studies). Specifically, the studies that Thurgood Marshall used to argue the Brown v. Topeka BoE case. They were the studies were the black children identified white dolls as better than the black dolls. He used

those studies to argue that the black children's self-esteem (self-concept in today's parlance) was lower than whites.

> This illustrates another problem I have with much (not all) psychological research.

Maybe we should focus on the not all part :-)

> What the studies you cite showed was that black children treated white dolls as being better than black dolls. From this, it is informally inferred that therefore black children think that white children are better than black children, and from that we conclude that black children consider _themselves_ worse than white children, and therefore that black children have lower self-esteem than white children.

> All that seems reasonable enough to base a decision on in the terms in which most decisions are actually made, but it's not enough to call it science. The problem is that _plausibility_ is substituted for _proof_. If the proposed logical connections were actually valid, then the final conclusion would be valid. But not otherwise.

Only a poor scientist would call this proof. Runkel is correct that we shorthand our writing often, but skepticism is a value communicated to all researchers. We our much less accepting than Runkel or you seem to think. I would begin your next sentence with "Strong supporting evidence..." instead of proof, 'cause I do at least strive for accuracy in my writing.

> Proof in this case would amount to finding some independent way of determining the black children's degree of self-esteem. This is the only way to validate this procedure for determining self-esteem. Before this procedure is applied to a general population, we must establish that when black children say white dolls are better, they are also experiencing low self-esteem. We can see which dolls they choose, but how can we see their self-esteem? What we're trying to establish is that the choice of dolls, which is easy to measure, is a reliable indication of self-esteem, which is not so easy to measure. If we could establish that connection, we could then substitute the easy measure for the hard measure in testing a general population. So the problem of showing that the test means something boils down to measuring self-esteem in some direct and independent way.

But here we are stymied. We can't see the self-esteem directly, unless we happen to be the particular black child under study, and have the sophistication to think in such terms. We can't just ask a three-year- old child how much self-esteem he or she feels; that's a meaningless term to a three-year-old.

The only recourse is to try to devise a completely different test for self-esteem. We see how black children behave when getting into lines with white children. We see how "assertive" they are in competing for toys with white children. We see what roles they play in pretend games. But in each case we run up against the same problem: there is no direct measure of self-esteem available for validating any of the tests. In each case, we simply assume that the behavior we can observe is indicative of self-esteem. So what do we end up with? A series of tests which are validated against each other, and none of which is actually known to indicate self-esteem. This thing called self-esteem remains hidden from observation; in fact we don't know what any of these tests is indicating, if anything.

This problem became evident to me when I began taking psychology courses in college. And it led me to wonder about something that psychologists didn't seem to care to think about: did the things that these tests supposedly measure exist at all? In the above example, I would have asked, is there really such a thing as self-esteem? Is this something that people have? Or is it just a way of talking?

I was asking that question several paragraphs before. Part of that skepticism we scientist have. If your psych teacher did not mention it, that was his/her bad.

> Now I think I would say that yes, there is such a thing as self-esteem, but that term is only a rough indicator of what is involved. To get any closer to a useful understanding, we have to ask what "self" means, and what "esteem" means, so we can see some sort of process going on. Obviously, self-esteem is a perception that can be had only by the person involved. It is a perception of one's own characteristics. And this perception, to have any value, must be compared with a reference level for the each state of those characteristics one has chosen as a target. If you perceive yourself as less than you want to be in each relevant respect, then you are probably experiencing what people call low self-esteem.

> But there is another side to this, which becomes visible only when you use the PCT model. What if your perception of your self matches the reference level with respect to each variable involved in characterizing yourself, but you have set your reference levels very low for characteristics that other people usually set to a high level? Your perception of self-esteem would then be exactly what you have chosen in each respect. It would not seem low to you, but others would still judge you as having low self-esteem. You, as a black child, would go behind the white children in the line, hand over a toy to a white child without protest, and play the black victim rather than the white victor in pretend games. Perceiving yourself doing these things would fit perfectly with your reference levels for how you should be.

> How could such reference levels come to be set so low? Obviously, because these are the settings that work, all things considered. If you are raised in a society that punishes you for assuming equality or ascendance in the company of white people, you can avoid that punishment by choosing ways of being that do not cause punishment. You remember how you were when you were getting along as well as possible, and you pick those memories as reference levels. This does not require any thought; all it requires is reorganizing until the pain is minimized.

I tend to agree with your analysis. It is an excellent example of why PCT is so important. It also shows why self-report questionnaires would not have been appropriate for they can only hope to get at discrepancies, and not necessarily very well. It also shows why the doll method is reasonable, if not perfect. But I think what you miss is that the researcher had some vague notion that the level of SE was more critical, not the perception of low SE. I think he (I believe it was a he) did a pretty good job given he was probably not exposed to your model. The darkness that psychologist are stumbling around in is not as dark as you think.

[you then describe the test...]

> Then you can start looking for common factors, for variables that indicate common problems, for deviations of individuals' reference levels from those of most of the group. You look for indirect indicators that show highly reliable correlations with either felt lack of esteem or low levels of aspiration for esteem. Now you have a direct measure of self-esteem, as you have carefully defined it, for a large enough group of individuals to allow validating other kinds of tests that are easier to apply. Of course these other tests will NEVER yield the kind of reliability that the basic study of one person at a time will yield, and no important decisions should be made about any one person on the basis of such tests. But such tests can be used as the basis for modifying policies that apply to many people or for assessing the overall effectiveness of a program, a teacher, or an administrator.

> Compare where we started with where we ended. We began with children playing with dolls, and we ended with a study of individuals aimed at discovering how their self-concepts compare with their desires, and what those desires are. We started with a vague common-language term, self-esteem, and ended by talking about specific variables associated with self-perception. Which approach is going to yield solid knowledge of how people perceive themselves, how children learn to perceive themselves? I submit that the answer is obvious.

> It's much, much easier to hand out a set of questionnaires to 30 people than it is to sit down with each one of them for hours and hours, learning how each person ticks. There are pressures of time and competition and pride and funding that make psychologists look for quick and easy ways to get information about important aspects of human nature. But the results reflect the amount of effort and thought expended. If anyone wants to conduct psychological investigations in a way that will yield reliable knowledge about human nature, there is really only one way to start: one person at a time. Without that kind of basic understanding, all generalizations are fuzzy and empty and hardly worth the trouble.

Have you done the analysis you suggested? Have you looked for SE using the Test? If you have, it is because of all the psych research on it that clued you into it. If you have not, why do you believe it is a dimension of self-perception? Because psychologists, like clinicians, have interviewed people...that is informally applied the test? (I am guessing of course - I do not know you mind.)

Is it a dimension of everyone's self-perception? We cannot definitively answer that unless we give everyone the test. But is there some critical mass? That would be an empirical approach. What if we run models that showed complex systems with certain properties like those of humans, always formed a reference signal that acted somewhat like SE is described? This is my guess. SE is slightly different for everyone, but the protection of one's sense of self is a critical feature of a self-organizing, self-aware, system.

This was not terse, but important I think. I hope you agree.

Later Jeff

Date: Thu, 22 Sep 1994 16:14:10 -0400
Subj: Re: Locke, all or nothing

[from Jeff Vancouver 940922.1610]

On Thu, 22 Sep 1994, Tom Bourbon wrote:

> I read your post, Jeff. This will be terse.

> Read [From Bill Powers (940921.0400 MDT)], in which you will see once again the way PCT modelers go about the business of trying to understand human behavior and to help them. After you read those ideas one more time, I hope you will give some thought to abandoning your ad hominem style when you are on this net.

If you think that I apply an ad hominem style than we do have miscommunication. I have a problem with the way you refer to non-PCTers, not with you per se. I apologize if you see that as an attack on you. That is the last perception I am trying to maintain.

BTW, I did (non-tersely) reply to Bill P.

Later Jeff

Date: Thu, 22 Sep 1994 19:57:52 -0700
Subj: Simply PCT

[From Rick Marken (940922.2000)] Jeff Vancouver (940922)

> When Locke and others are collecting data and then trying to make inferences of the underlying causes, they are doing inductive research.

We use both "inductive" and "deductive" research in PCT; we favor neither. The combination of the two is called "science".

Locke and others are, indeed, "collecting data and then trying to make inferences of the underlying causes" but their data (as we know from many PCT studies of human behavior) simply represent averaged side effects of the controlling done by groups of people. Their inferences of the underlying causes of this behavior are bound to be wrong; that was the point of my "Blind men and the elephant" paper. Locke et al are playing a losing game. This is all well known and clearly demonstrated (with .99 precision) in numerous PCT demos and experiments. Locke et al are studying irrelevant side effects of control and they don't know it. It's not like we've been trying to hide this fact from them (or anyone else). If Locke et al were astronomers they would still be looking for new epicycles. There's not much one can say to people like this except "goodby and good luck".

> you also note that it is difficult to get tenure doing the PCT thing

I did it. Tom did it.

> So what does one do? 1) Give up convincing the scientific community if they can afford to, or 2) try the convince some of them. You have chosen the former because you can afford to (or perceive that you can, which I do not question).

I don't think we have given up trying to convince the scientific community (or anyone else) that PCT is the best scientific model of purposeful behavior. The only thing I've given up is the expectation that I will be successful in my lifetime.

> All I am asking is that you do not make my job harder than it already is!

We don't want to make your job hard. All we want to do (here on CSG) is provide an accurate representation of PCT. If you want to get tenure by selling what amounts to a false representation of PCT to journal editors and your tenure committee, that's up to you (or, as my grandpa would say, gezunter hait). I would certainly understand why you might want to do it, but please don't ask me for help.

Truth be told, I don't think it is beyond one's capability to get published or to get tenure doing PCT. Bill Powers described an approach to individual, applied PCT that could certainly be turned into an acceptable research program by someone who understands the basics of PCT.

I appreciate your concerns about getting tenure, but they are not the same as the ones that drove me from academia. I didn't have a problem because people rejected PCT; I had a problem of conscience because I didn't want to teach conventional psychology but, at the same time, I didn't want to be unfair to my students by teaching PCT in a non- PCT curriculum. But other people have been able to teach an accurate version of PCT and maintain tenure and their academic position as well. Dick Robertson comes to mind as one stand-out example. Clark McPhail and Chuck Tucker are sociologists who seem to have been able to do it. Gary Cziko has managed to as well. I'm sure there are several others. Perhaps you could ask these people (those who are on the net) how they have been able to survive as academic PCTers in a non-PCT world.

Best Rick

Date: Thu, 22 Sep 1994 19:21:39 -0600
Subj: PCT and psychology

[from Mary Powers 9409.22] Jeff Vancouver 940921

Well, for heaven's sake, what provoked that outburst?

You _could_ argue that we are as guilty as Locke for deliberate ignorance, etc., but you would be arguing from false premises. It is because we _have_ read Locke and Lord and Carver and so on that we feel entitled to comment on their failure to grasp control theory.

You _could_ argue that developing a model is different from developing applications. It's more fundamental, for one thing. How can you develop good applications if your model is a) absent or b) wrong?

Apparently the constructiveness or destructiveness of what we have to say is debatable (since you are debating it). Is knocking down a theory that doesn't work really a bad thing to do? We have wasted a _lot_ of time looking at what these researchers do, and we are unimpressed - not ignorant.

Finally, although PCTers spend a vast amount of time on this net teaching and explaining, that is not the sole function of csg-l. This extremely small corner of cyberspace was established so that PCTers, who are scattered rather thinly over the planet, could talk to each other about control theory. Listeners, questioners, students, and critics are welcome, BUT, the basic premise of csg- l is that PCT is a viable, productive theory of the organization of living systems. In some quarters, that qualifies it, operationally, as a reasonable approximation of "the truth" (whatever that is). Since most of psychology has ignored control theory, and the rest misunderstands it, we do indeed have a "corner" on it. But it's not because we _want_ to have a monopoly of it, which is what a corner means - it's more like a sack of kittens. We can't seem to give it away. Meanwhile, this is the only place where PCTers can talk freely to one another and express their feelings about what, in the context of control theory, looks like bad psychology and worse science. And if we are so terribly, terribly thoughtless as to say, on the net we established, for our own use, that our theory is better than anything else going in psychology, then what exactly is your problem with that?

Mary P.

Date: Fri, 23 Sep 1994 10:01:40 CST
Subj: Re: Locke, all or nothing

Tom Bourbon [940923.0946] >[Jeff Vancouver 940922.1610]

> On Thu, 22 Sep 1994, Tom Bourbon wrote:

>> . . I hope you will give some thought to abandoning your ad hominem style when you are on this net.

> If you think that I apply an ad hominem style than we do have miscommunication. I have a problem with the way you refer to non-PCTers, not with you per se. I apologize if you see that as an attack on you. That is the last perception I am trying to maintain.

Understood, and I apologize for my remark. After sending it, and after reading some of the subsequent posts from you and others on the Locke thread (which includes ideas about research, theories and statistics in behavioral science), I have been wondering what led me to interpret your earlier remarks as "ad hominem." I have a few ideas on the topic and will say more about that in a later post, after I've thought some more about it, and after I finish two proposals to the local Human Subjects Committee -- for studies of PCT tracking tasks as quantitative measures of neurologic deficit.

Remember that I am not saying bad things about non-PCT researchers, as people. However, I certainly do criticize, with passion, the near-universal practice of using statistical mush (mush in fact, not merely in my opinion) to support all manner of "plausible" psychological constructs.

The deadline for proposals draws near, and I have reams to write.

More later, Tom

Date: Fri, 23 Sep 1994 14:22:32 -0700
Subj: Decisions; self-esteem

[From Rick Marken (940923.1400)] Jeff Vancouver (940922) --

> I think the question of DM is the question of which reference signal is passed down from the control unit that has as a reference signal "spend this dime."

What you seem to be saying (given my understanding of HPCT) is that DM refers to the output of the control system that is trying to produce the perception of a "spent dime" -- the perception specified by the reference signal. The output of this control system sets (varies, actually) the reference inputs to the lower level systems that are the means by which the perception of a spent dime is produced. To see how this process works, get a copy of my spreadsheet model, which is on Dag Forssell's PCT demo disk. There is no DM (other than subtraction and integration) involved in hierarchical control.

> I have no trouble (as a working hypothesis) with "nothing beyond perceiving the elements of the situation and applying an algorithm that yields an answer based on the elements and logic or arithmetic." This strikes me as a reasonable description of the DM process. But the next question is which elements are perceived?

A question that can only be answered by doing The Test.

> What is the nature of the algorithm? That is, what are the parameters?

A question that can only be answered by building a hierarchical control model.

> Does the organism need to go into thinking mode to access parameters for the algorithm? Can individuals control for perceptions that cause (even in their own world view) other higher-order perceptions to go out of line (i.e., why are some perceptual elements not represented in the algorithm)? Where is the algorithm?

All questions that can only be answered by appropriate testing and modelling. When you start to do this, you will find that some of these questions (like the one about "thinking mode") are probably not really that well conceived.

> I think that one of the elements in the algorithm, that is in the output function is an estimate of the probability of attaining a reference signal (what Bandura calls self-efficacy).

Why don't you try to implement this in a model? I think what you are saying here makes no sense but I don't think you will believe me until you actually try to build a model of the kind of behavior you have in mind. One reason for my suspicion that this makes no sense is that a reference signal is not "attained"; it is determined. Perhaps you meant that the algorithm estimates the probability of "attaining" the perceptual signal -- ie. getting it to match the reference signal. This sounds like a feedforward process - where outputs are selected on the basis of estimates of the likelihood that they will produce the desired results. People do go through these prediction exercises in imagination but there is no evidence I know of that suggests that such predictions are an important component of on- going controlling.

> I believe the value for that estimate is partially a function of self-concepts like self-esteem

What is the data that leads you to this belief; what does the model look like that implements this idea.

> I believe Bandura and others have shown us the reasonableness of these propositions.

To paraphrase George S. Kaufman (who, on his deathbed, said "Dying is easy; comedy is hard" -- a favorite expression of my comedy-writer father-in-law, who died) belief is easy; knowledge is hard. What leads you to believe that Bandura has shown us the "reasonableness" of these propositions. I judge "reasonableness" in terms of observation (is there evidence of the phenomenon to be explained; if so, it's reasonable) and modelling (does the proposed mechanism actually behave and does it behave like the observed phenomenon; if so, it's reasonable). I have never seen anything produced by Bandura that met my reasonableness criteria.

> I believe Bandura's model is flawed

A model that neither works nor predicts the phenomenon it is designed to explain is, indeed, flawed.

> why do you believe it (self-esteem) is a dimension of self-perception?

I'm willing to call some perceptions I have "self esteem". That is why I believe that there might be such dimensions of perception. Whether anyone else is controlling such perceptions is a job for The Test.

> Because psychologists, like clinicians, have interviewed people...that is informally applied the test?

Yes, hypotheses about controlled variables can certainly come from these sources. They can even come from looking at conventional psychology data. You've gotta start somewhere. The "doll" study (as Bill pointed out) can be the start of a systematic attempt to test for the variables that these kids are actually controlling.

What you should be clear about is that conventional psychology data (including that from Locke et al) is neither a better nor a worse source of _initial_ hypotheses about controlled variables than simple, causal observation. But you can't find out what people are actually doing unless you do The Test -- that means iteratively testing (and eliminating) hypotheses about what a person is controlling. This is the step that conventional psychologists have not taken.

If you and Martin Taylor were only looking to the conventional literature as a source of hypotheses about controlled variables (hypotheses that are almost certain to be wrong, but are at least a start) I would have no complaint. But you guys seems to think that conventional psychological research has already discovered something about behavior. It is possible that there is some evidence for controlled variables lurking somewhere in conventional psychological data - - maybe people do control a variable that we could all agree could be called "self- esteem"; maybe people do control the equivalent of the energy output of a band pass filter (in signal detection studies); but we can't know this until we start testing hypotheses about what variables individuals control -- that is, by doing The Test. Conventional psychologists don't do The Test so there is no possibility that they have discovered anything about purposeful behavior.

Best Rick

Date: Sat, 24 Sep 1994 11:01:37 -0600
Subj: Data; decisions; tempting proposal

[From Bill Powers (940924.0829 MDT)]

Rick Marken, Jeff Vancouver --

Jeff:

> I believe Bandura and others have shown us the reasonableness of these propositions.

Rick:

> To paraphrase George S. Kaufman (who, on his deathbed, said "Dying is easy; comedy is hard" -- a favorite expression of my comedy-writer father-in-law, who died) belief is easy; knowledge is hard. What leads you to believe that Bandura has shown us the "reasonableness" of these propositions.

I'm beginning to get the feeling of watching trench warfare here. One side lobs a general reference to unnamed literature at the other; the other lobs back statements that nothing in that literature is any good. To one who has read little of the literature, the whole argument is content-free. It's like hearing two people arguing about what a friend of theirs said, without ever mentioning what was said.

If we're going to make any pronouncements about Bandura's scientific findings, how about someone summarizing the data? Even just one experiment. I see nothing terribly unreasonable about saying that people will choose goals on the basis of their assessment of their own ability to reach them, if that is what "self-efficacy" means. That is just what common sense and personal experience would say. I would like to experience space flight, but I don't think I could make it through the astronaut training program at my age, so I have regretfully deleted that goal from the list of those I actually try to achieve.

The only question I have is whether we can take this informal sort of statement as having been formally established as a scientific truth. As everyone must be aware by now, I am concerned about general statements that are untrue of a substantial part of a tested population, yet which are treated as if they were true of everyone. I'm concerned with the truth-value of such statements, not so much one statement at a time but as these statements are used in systems of reasoning that depend on the simultaneous truth of several or many statements. I don't need to go through that argument again (unless asked by someone who missed it).

So if Bandura has made statements about the relationship of self- efficacy to other factors, I would like to know, aside from what the statements were, how well his data support them. How many individuals counted as positive instances of the statements, how many as negative instances, and how many were indeterminate, as a fraction of the total population studied? Before I accept experimental evidence as supporting any general statement about people, I would really like to know that essentially all of the people in the study behaved in a way that supports the general statement. I vastly prefer statements that apply to every individual.

The reason I ask is that if Bandura or Locke or anyone else has noticed real phenomena of goal-seeking behavior, these phenomena should be grist for the PCT mill. If we think that the phenomena are interesting but that the method of establishing them was lacking in some respect, then we should be able to design a PCT experiment to investigate further, and carry it out. Even if Bandura and Locke aren't interested in the result, some people might be interested.

Jeff:

> I have no trouble (as a working hypothesis) with "nothing beyond perceiving the elements of the situation and applying an algorithm that yields an answer based on the elements and logic or arithmetic." This strikes me as a reasonable description of the DM process. But the next question is which elements are perceived?

Is any general answer to that question possible? There are so many different bases for "decision making" and so many algorithms, ranging from mathematical to crazy, that it would be hard to come up with "the" elements that are perceived when decisions or choices are made. Better to start with examples, and see what is generalizable and what is not.

When Mary goes grocery-shopping, she decides which product to buy, for certain products but not all, on the basis of the price-per-weight sticker that grocery stores now carry. Of course quality also figures in; carob cookies are cheaper than chocolate ones, but she knows that I turn my nose up at carob. There are many variables to consider, but the overall result is pretty much ordained by the weights given to the variables and the logic that is applied. The final result in each case is to pick up this package rather than that, from a set of similar products. As Mary has said on the net, the only time she thinks of this process as "decision-making" is when there is a conflict: when the data and the reasoning do not automatically select just one result, and there are several possibilities with no basis for choosing among them.

My shopping methods are very different. I know what kinds of cookies I like, so I scan along the shelves until I find one of them, and I buy it. If I had scanned further I might have found another kind I like better, but my loop gain for cookies is not very high so that doesn't bother me. If I don't find any cookies I like I don't buy any.

Mary no longer allows me to go shopping with her. I am in and out of there in 10 minutes, about 50 minutes before Mary is ready to go. She considers a lot more variables than I do, and likes to have in mind what all the choices are before she lands on one. And she buys groceries for a lot less money than I would.

Now, both of us go into a store, pick items from a collection of similar items, and buy them. Just looking at the behavior, an onlooker would say that we are both making decisions and therefore must have a Decision Maker inside of us. But there is not very much in common about our methods for making selections. I do it by setting up a few target items and scanning along the perceived items until there is a significant dip in the error signal, a serial search process coupled with monitoring for a reasonable degree of match with any of several reference signals. Mary does it in parallel, keeping memories of available items in mind, assigning weights (I presume) to them based on multiple characteristics of each item, and looking for the item with the largest score (I suppose). These are both methods for "making decisions," but the algorithms are clearly very different (hers requires a lot more brain power and time).

Also, if you look at the detailed processes involved in either method, there isn't any one process that amounts to making a decision. Making a decision -- that is, actually picking up one of the items -- is the outcome of applying a lot of processes, none of which is a decision- making process, and any one of which could be used for all kinds of other processes as well.

I'm working on a paper that contains a little essay on two kinds of models (descriptive and generative) and the problems that arise from trying to mix them. I think that the concept of decision-making belongs among descriptive models, while the PCT explanation of decision-making, which does not include any explicit decision-maker, belongs among generative models. Decision-making is a phenomenon; the processes that create the appearance of decision-making consist of underlying functions none of which is a decision-making process. Note that in the control- system model itself, there is no box labeled "controller." Control is a process at the descriptive level which arises from connecting functions in a hypothetical underlying reality none of which can be called "controlling." I'll post this essay when it's in a little better shape.
--- ------
Francisco (Arocha?) (940924.0325) --

> I've always been very suspicious and critical of conventional psychology (Myself, with some friends, wrote a book about it)

Full reference, please!

> Forget about trying to convert the children of the Lockes and the Banduras of this world. They have already made up their minds. They already know what science is. They already know what psychology is. Let them live their delusions.

A very tempting proposal. After all, how much time do they spend patiently trying to understand _us_? I have come very close more than once to reaching this conclusion. The only thing that stops me is knowing that practically everyone who is now on our side came from conventional fields. There are always a few who get the message. The problem with appealing ONLY to dissenters is that you get not only the free spirits and the independent thinkers, but all the crazies, too. At least the people in conventional fields have some experience with disciplined thinking, even if what they think about isn't so great.

Psychologists seem to have a great deal of loyalty to psychology -- my science, right or wrong. For me this is a constant conflict that makes it difficult to find a direction and pursue it. It doesn't seem possible to have a simple discussion of things like statistics without arousing instant opposition, from those who happen to use it a lot. Actually, it really doesn't matter what the subject is; if PCTers offer even an implied criticism, the defenders of the faith will oppose it even before they start thinking about it. If we must continually worry about what we say for fear of threatening someone's beliefs, we will always be diverted into unprofitable arguments and away from our own pursuit of truth, beauty, science, and chocolate-chip cookies.

But the other side of the coin is not only seeming to become a cult, but actually doing so. If we ever start thinking that we are no longer subject to the requirement of persuading others through logic and demonstration, we will start acting exactly like our opponents, assuming automatically that we are right and everyone else is so obviously wrong that debate is no longer necessary. After that, Saint Bill is just around the corner.

I know what we should do! We should train young people who haven't been contaminated yet by the behavioral sciences and make sure they understand PCT from the ground up. Then we should require them, right after their training, to put on blue suits, white shirts, and neckties, and go in pairs from door to door all across the country, carrying copies of B:CP and equipped with openers such as "Do you know that Bill loves you and your control systems?" We could prepare little pamphlets ....

I guess that this conflict will just go on, until we find a way out of it that doesn't lead to a worse situation. Maybe the answer will come out of our attempts to form a Center for the Study of Living Control Systems. We'll think of something.

> As for Rick M., well, I like him. Stop complaining about his "bad manners". He has done great experimental work and is the first defender of the basic ideas of PCT. Besides, for me, he is to CSG-L what chilies is to mexican food. It may make me sweat, but I sure like it.

Yes, people do tend to forget that Rick has done some of the best experiments in PCT-world. When he demands that others come up with some real evidence, he speaks as one who has DONE it. When people argue with him, I think they're obligated to produce results of equal quality and specificity before they're entitled to be taken seriously. I am getting mighty tired of people putting their 0.3 correlations up against his 0.95+ correlations. It gets ridiculous.

Thanks for spicing up the conversation.

Best to all, Bill P.

Date: Thu, 29 Sep 1994 16:12:20 -0400
Subj: goals, models, and salutation.

[from Jeff Vancouver 940929]

This is going to be my last post for awhile. I cannot maintain my involvement and still maintain what I thing is sufficient progress toward tenure. Also, perhaps more importantly, I echo Bruce Nevin's position. I do not know how to model and it seems I need to learn to continue the debates I am participating in. Tom, Mark, and Bill (and probably others) cannot be expected to respect my position until I respect theirs. I don't think the relevance of Kernan & Lord's data is explainable until the "lobs" we are making can be reduced. That will happen by either me adopting your mind set or me expanding your mind set through your mind set (i.e., modeling).

I think Bruce Buchanan [940927] made some excellent points (too bad about the behavior/perception mix-up, but it is a closed loop - I am very forgiving). I just gave my conference presentation from August (at the Academy of Management) locally (NYU I/O Psych colloquium) the other day. The feedback was most informative. Basically, I need to show how PCT helps the individual researcher do their work. I cannot argue that the science needs this model as I have been, only that you (i.e., some individual scientist) needs it (or it will help the scientist do his/her work). Just like with the individuals on the net, I need to do that from their perspectives/beliefs etc., by showing them where their believe/perspectives are limiting their progress and provide the PCT alternative. (Bruce B.- send me your address and I will send my revised paper that I think you will appreciate - it is almost done.)

I have learn a lot from this interaction and loath to give it up, but the time drain to just too much. I have one specific, practical suggestion regarding the goals/future of CSG-L. Perhaps a short prefix in the subject field can clue readers and lurkers into the domain of the message. For example, posts on models having to do with levels 1-3 begin M1-3:. Philosophy of science post begin PS: etc. A post that lists the agreed upon prefixes could be accessible or made part of the intro post. It is just a thought.

Two more notes, I saw Locke's paper in Applied Psychology this last weekend. It was appalling. You [I cannot remember who] did not quote the truly wretched sentences where Locke says:

> "...I received a letter from a prominent advocate and founding father of control theory .... This theorist was not exactly clear on just what control theory really was..."

This is from the guy who refused to read the book, which I assume Mary (or Bill) cited in their letter to Locke. Correct me if I am wrong. BTW, I am trashing Locke and Bandura in my revision (I am side-swiping Lord)

2nd note is a quote from my wife after describing the situation and the net with her:

"It sounds like Locke should be shut out, Lord is not God, and that Powers defines right."

I thought that summed it up pretty well.

Much Later Jeff

Date: Fri, 30 Sep 1994 10:36:51 -0600
Subj: Replication;

[From Bill Powers (940930.0655 MDT)]

Jeff Vancouver (940929) --

I'll send this direct to you also in case you signed off already.

We'll be sorry to see you go, as you are a link to the mainstream world. I hope your experiences with PCT so far have not spoiled anything for you!

I hope you had a chance to read Chuck Tucker's wonderful post of 940929. [PCTDOCS\REPLICAT.ION] Chuck is in much the same position you are in, not being a programmer and not having had any previous experience with modeling. He is the perfect example of how non-technical CSG members work with those who can write programs and set up models. Chuck decided that he would simply run our models and get some experience with how they work; as can be seen in his post, this has led to a tremendous increase in understanding of PCT, and the development of abilities to see the possibilities of PCT research of many kinds. He can now spot previous research in which there were approaches compatible with PCT even if their data and analyses were not presented in a way we could use directly. This is going to lead, eventually, to a whole methodology that sociologists can use to apply PCT in their own fields -- without ever requiring them to become modelers or programmers.

I sympathize completely with your need to pursue tenure; what a world! But I hope you will stay in touch with Chuck, because he can help you see how to apply PCT without having to be a techie.

Best to all, Bill P.