A reasonable set of comments and questions

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

Date: Mon Mar 29, 1993 11:14 am PST
Subject: Ken Hacker's !s and ?s

[From Bill Powers (930329.1000 MST)] Ken Hacker (930328) --

A reasonable set of comments and questions. I'll try to deal with some of them.

Your list of our criticisms of conventional behavior science is generally on the mark, but without any reasons being supplied they sound more extreme than they are.

Consider:

* All social science is wrong.
* All behavioral science is wrong.
* All social scientists and behavioral scientists believe what is wrong.
* All social scientists and behavioral scientists do not know what they are doing with research.

In the first place, I always try to put somewhere in the discussion the proviso "if the PCT model actually explains behavior...", which make this somewhat less arbitrary and ad hominum.

In the second place, it makes a difference just what claim of wrongness is made. Wrong about what? The fundamental disparity between PCT and conventional theories of behavior is the treatment of cause and effect. Under PCT, the actions of an organism are not caused by stimuli with the organism simply mediating between cause and effect. In the PCT model, but not in any conventional model, there is a controlled variable between the apparent cause and its apparent effect as usually observed. The behavior is directed toward control of the controlled variable; it is not simply a response to the stimulus or a dependent variable being determined by an independent variable. As a result, all theories in the conventional sciences that rely on some cause-effect or IV-DV paradigm, including methods of data analysis that assume such a relationship, are fundamentally contradicted by PCT. It isn't necessary to investigate every detail of any theory that is proposed; all that is needed is to see whether the internal validity of the theory depends on assuming that behavior is a dependent variable and inputs to the organism are independent variables. If that is true, then no more need be known: the theory is contradicted by PCT and in terms of PCT the conclusions are false.

I leave it to you to decide how much of the social and behavioral sciences would remain intact if it were to prove true that all behavior is organized around controlled variables, and none is open-loop.

* Only PCT has something useful to say about how humans regulate their behaviors.

As stated, this is contradictory to PCT on the face of it. Human beings, under PCT, do not regulate their behaviors at all, as least as behavior is conventionally understood. PCT says nothing at all about the regulation of behavior; it is concerned with the regulation of perceptions, of inputs. Behavior, or action, becomes what it must become to prevent disturbances from having an important effect on the controlled inputs. So it is true that only PCT has something useful to say about the regulation of input, if that is what is meant by behavior. But that is not what conventional sciences have meant by behavior.

* Communication theory is wrong.

Communication theory, as a mathematical treatment of certain ways of representing variables and signals, is completely right in terms of its mathematical operations. Where I have difficulties with it is in the initial

definitions and assumptions, which do not seem to me to have any necessity to them. But that's me, not PCT.

* PCT equations (confirmed by PCT theorists) PROVE that PCT is correct.

Fortunately, confirmation of the PCT equations by comparison with experiment is done by public methods easily replicable by anyone, with high reliability. No faith or special knowledge is required. The validity of these equations is put at risk every time they are used to fit a model to real behavior. The equations don't prove that PCT is correct; as far as they go, they ARE PCT.

* PCT is real science. Other approaches to human behavior are pseudo-scientific or "half-assed."

PCT is real science because it risks everything with every application to data. It isn't simply assumed to be correct and twisted to make the data seem to fit it. It can't live with serious exceptions or counterexamples. Its premises are themselves testable through experiment, and are tested every time they are used.

I would accept as real science any other approach to behavior that had the same characteristics. We're not talking about control theory here: we're discussing what science is about.

* People who challenge PCT are misguided, ignorant, and not yet fully developed intellectually.

Depends on how they go about challenging it. I see no shame in being any of the things you mention -- is there anyone who doesn't fit the description? I object to only one kind of challenge to PCT: the kind that is made without knowing what PCT is about, and is based only on a difference in conclusions.

The questions:

a. The scope of PCT. Is it limited to describing and explaining physical behaviors of human beings such as motor actions?

In that all visible behaviors are physical and entail motor actions, including debating about quantum physics and women's rights as well as tracking behavior, yes. One could say, given a few precautions, that there are "really" only four kinds of behavior: push, pull, twist, and squeeze. All the rest is controlled perception, consequences of behavior. And even those four are controlled perceptions. PCT can be applied to behavior at any level of organization you please. HPCT is meant to apply at all levels at once.

b. If there is no information inside of input, what prevents one from asserting behavioristic-like internal responses to internal stimuli or a kind of reverse behaviorism?

Nothing. You can propose that behavior is caused by the phases of the moon if you like, but you'll have to defend the proposal. See my earlier post to Rick on this subject. I'm not going to generalize about information and input any more until I'm sure what's being claimed or asserted.

c. What happens to Ashby's Law of Requisite Variety?

I don't use it, although it's implicit in control theory. Variety is defined by Ashby as follows:

"The word _variety_, in relation to a set of distinguishable elements, will be used to mean either (1) the number of distinct elements, or (2) the logarithm to the base 2 of the number, the context indicating the sense used." (p.126 of _introduction to cybernetics_).

The Law of Requisite Variety states that the output of a regulator must have at least as much variety as the disturbance, if the result is to be regulation

of some variable. That is to say, the output must have at least as many discriminable states as the disturbance has.

In terms of control theory, we would say that if a controlled variable is to be maintained exactly at a reference level, the output of the system must be at all times quantitatively equal and opposite to the magnitude of the disturbing variable, both measured in terms of effect on the controlled variable.

Note that the control-theoretic statement goes much farther than the LRV goes. It says that not only must the output have AS MANY discriminable states as the disturbance, but that these states must have the correct quantitative magnitudes, and they must occur in pairs: one specific output state for each state of the disturbing variable. The output and disturbing states must be quantitatively equal, and of opposite signs. So the LRV, while it states a weak necessary condition, by no means states a sufficient condition for control or regulation to exist.

d. How does PCT account for stored knowledge such as schemata (or whatever other term you choose)?

PCT itself doesn't; that's a question of fact for neurophysiology or neurochemistry. I have proposed some possible relationships of stored information to the operation of control systems in the hierarchical model. As Greg Williams puts it, those proposals are embellishments on the basic model.

"Schemata", as I have heard the term used, could have various meanings, corresponding to reference signals (when they relate to goals), to perceptual input functions (when they determine how lower-level perceptions are interpreted) or memories (in various circumstances). "Knowledge" has similar usages. Because these words are used in relation to functionally very different aspects of a brain model, it's hard to pin them down to any one meaning. The problem is somewhat similar to the uses of "want." If you say "I want some ice cream" you could mean to point to a reference signal, the definition of what it is that you want, or you could mean to indicate an error signal, emphasizing the fact that you don't have the ice cream. In my opinion, terms like these are too loose to be used in a model, although we may use them informally when context supplies the missing discriminations.

d. What makes PCT more than intensive descriptions and explanations of neural pathways and signals?

The fact that it makes almost no attempt to describe neural pathways and signals. The primary uses of PCT are in modeling externally-visible behavior in real environments, showing that relationships expected under the hypothesis of control do in fact occur and can be predicted. The models propose that certain functions are carried out inside the control system: perception, comparison, and conversion of error to output. Nothing is said about what neural circuits are involved in implementing these functions, although in some cases we know quite a bit about specific neural signals and pathways involved in specific control systems.

e. Why must PCT be understood as some sort of revolution as opposed to some sort of new and productive thinking?

Because PCT directly contradicts most traditional conceptions of how behavior itself works. It also predicts phenomena of a kind that conventional sciences have known nothing about, or that under conventional assumptions go in a direction opposite to the direction that control theory would predict and that experiment supports. For example, under control theory, the prediction (and the observation) is that doubling the sensitivity of a perceptual function to stimuli will result in halving the input to the perceptual function, not doubling its output signal.

Control theory is to conventional theory as Newtonian celestial mechanics was to Ptolemaic epicycles. Many of the observations may be the same, but the explanation is radically different.

f. What are essential differences between PCT and cybernetics?

Cybernetics abandoned control theory at about the time of the 5th Macy conference, when most of the main misconceptions about control systems were laid in.

g. What do axioms or propositions from PCT contribute to everyday human adapting, living, changing, improving, that other forms of analysis do not? Where is the proof of human behavioral successes with PCT, as opposed to success in the abstract?

I'd say you should ask people who are applying it. Try Ed Ford, Dag Forssell, and David Goldstein. Or ask people in the CSG who have been interacting with each other for 9 years with an understanding of PCT. In a lot of cases, PCT vindicates commonsense ideas that science has rejected, such as the importance of goal-setting, perception, and intention. If you see concepts like that creeping back into polite scientific society, I think you can credit PCT with having inspired at least some of those changes in thinking. Even when an idea is rejected, something of it sticks.

h. Most scientists are concerned with not only describing and explaining phenomena, but also wish to predict and sometimes "control" them. Why does PCT deny the desirability of prediction?

PCT does not deny the desirability of prediction. I, personally, deny the desirability of lousy predictions, particularly when they're used as if they were good predictions.

> Is prediction not always part of human anticipation, whether in ordinary life or in science?

Yes, I think it is. It's explained, more or less, in HPCT as a phenomenon of imagination, which is part of the story of mental modeling. People do it, so it belongs in the model. But I can't say a lot, theoretically, about HOW they do it. Most of my objections to prediction involve pointing out that it's not being done very well, and shouldn't be relied on as a method of control in most situations -- not if good control is important.

i. Where do reference levels or signal originate? In other words, if my control system has error signals, what constitutes the sources of the error signals?

I guess you haven't read anything about the hierarchical model. When you say "my control system" I wonder if you have read anything about the model at all. People have hundreds, thousands of control systems, all active at the same time and at different levels. Higher-level systems act not by producing motor outputs but by adjusting reference signals for lower systems. If you haven't been aware of this aspect of the model, you must have found a lot of the conversations on the net pretty confusing. Next you'll ask "But what about the highest levels of control system? Where do THEY get their reference signals?" And I will answer, your guess is as good as mine, but asking the question shows that you get the picture.

> What is stored that makes me think that I do not like or wish to accept certain input?

You're assuming that it's something stored that "makes you think" that you don't like etc. A reference signal specifies a certain amount of a given perception, anywhere between the maximum possible and zero. That becomes the amount that you prefer; when you say you prefer that state of that perception, control theory explains this by saying that there is a reference signal in some control system set to that value, probably as part of controlling for some higher-level perception. This may or may not involve "storage" of something. There are lots of proposals on this subject in BCP.

> How conscious are the processes of matching input to the reference signals?

It depends. The same control system can operate consciously or automatically, at any level. A perceptual signal can occur with or without consciousness of its presence. The nature of consciousness is not explained in PCT or HPCT (or as far as I know, in any T).

> As PCT should not be criticized by me or anyone else for not explaining social behavior, social sciences which do focus on social behaviors should not be excoriated for not explaining individual human control mechanisms and processes.

Clark McPhail, Chuck Tucker, and Kent McClelland (the CSG's sociologists) and Tom Bourbon (who has modeled simple social interactions) ought to comment on this. My view is that PCT, by explaining the interactions of individuals with their surroundings, lays the groundwork for explaining what happens when groups of independent control systems interact with other control systems. Social "laws" emerge from the properties of interacting individuals and the shared environment. While naturalistic observation is needed to determine the existence of regularities in social behavior, PCT, I claim, is needed to explain these regularities.

> a. Communication studies, unlike traditional psychology and maybe other behavior/social sciences, no longer assumes that people are simply affected by stimuli in varying conditions and ways. Since the 1970s, my discipline has said that each human TAKES from mass media, from interpersonal interactions, from printed words, from any messages, and recodes what is decoded. We threw out Shannon and Weaver's model in the 1970s as anything useful to explain human communication. Both Aristotelian and electrical engineering models of message sending and receiving say little of importance to understanding the complexities of human communication, mainly because people do not simply take in messages. Nor do messages do contain meaning.

Good. Give my regards to Klaus. And what is the reason for all these changes in thought? Is there any theory from which you could have deduced all these new ideas? Or are they simply ideas that were proposed, and that others found acceptable for unstated reasons? Everything in this paragraph could be deduced from PCT, and has been familiar to PCTers for many years (except for the remaining buzzwords like "coding" and "recoding"). You're talking about observable phenomena, and that's what PCT is for: to bring observable phenomena into a single common framework and make some sort of sense of them -- not just to accept that they happen, but to explain, in terms of a unified structure of theory, why they must happen. The same theory that explains tracking behavior explains why people give their own meanings to their inputs, and why that is the only way it can happen.

Hope you had fun with the grading....

Best, Bill P.

Date: Mon Mar 29, 1993 1:16 pm PST
Subject: Mary on Hacker's comments and questions

[Mary Powers 9303.29] Ken Hacker:

I hope that a number of people try answering your questions - various approaches may yield one that's right on the money for you.

> a. The scope of PCT. Is it limited to describing and explaining physical behaviors of human beings such as motor actions?

The problem with this question is an implication (that I perceive, and think you intend, consciously or not) that movement is the outcome or end result of whatever is going on in the organism. In PCT, motor actions are the means by which perceptions are controlled - all perceptions, at every level.

The idea that PCT is all very well for explaining mere movement has been around in cybernetics for years - Klaus probably heard it the same time I did, from Varela, in November, 1984. The idea was that it did not explain having

high thoughts about great cybernetic ideas. How one went about speaking to an audience about those great thoughts didn't enter into it.

> b. if there is no information inside of input, what prevents one from asserting behavioristic-like internal responses to internal stimuli or a kind of reverse behaviorism?

I'm not sure I understand this question. Do you mean input as a stimulus causing behavior? The point is joint causation - there has to be sensory input AND a reference input to a comparator. The difference drives the output. If both signals have the same value, nothing happens, no matter how stimulating the input appears to an outsider.

> c. What happens to Ashby's Law of Requisite Variety.

I'll leave this to Bill.

> d. How does PCT account for stored knowledge such as schemata (or whatever term you choose).

I'll leave this to neurophysiology. It suffices for the moment that knowledge is stored and can be accessed.

> d. (again) What makes PCT more than intensive descriptions and explanations of neural pathways and signals?

PCT is an explanation of the functional organization of neural pathways and signals. There are a lot of pathways and signals whose functions are unknown unless they are seen as components of control systems. PCT is consistent with what is known to exist in the nervous system, but a lot of what exists is not explained by those unfamiliar with PCT.

> e. Why must PCT be understood as some sort of revolution as opposed to some sort of new and productive thinking?

Because of the active resistance and consistent misinterpretations with which it has been received over the past 33 years. PCT was offered initially as simply something new and productive. The idea that it is revolutionary was a gradual development of an explanation for other people's reaction to it.

> f. What are essential differences between PCT and cybernetics?

This would be easier to answer if cyberneticists were willing to define cybernetics. Those I have asked have been unbearably coy.

I think the answer is that PCT IS cybernetics. In The Science of Control and Communication in the Animal and the Machine, PCT is the science of control. No one in cybernetics has even come close to PCT in developing it. PCT began when Bill P. read Wiener. While waiting for further developments of these marvelous new ideas, he began working them out for himself. No one else seems to have bothered over the past 40 years. So by default, PCT is it.

> g. What do axioms or propositions from PCT contribute to everyday human adapting, living, changing, improving, that other forms of analysis do not? Where is the proof of human behavioral successes with PCT, as opposed to success in the abstract? Is [it] too soon to ask this question?

PCT affirms and explains some principles that have been around a long time. For instance, if you push on people, they push back. Why? Because they are also control systems, resisting disturbance. Terry Brazelton advises parents to let their children be autonomous as much as possible, and only insist and clamp down when it is really necessary, in order not to establish a pattern of resistance to everything. This is empirical wisdom. PCT explains it.

PCT explains why Rick has escalated over the years into the high emotional pitch you see now - he keeps meeting resistance and pushes back harder. Maybe some day he will reorganize and find a new way to go about getting PCT across to people who don't want to know about it. He's tried quite a few, and I hope

keeps on thinking up more demos and experiments. Meanwhile, ranting and raving on the net keeps that error from getting too large. (Incidentally, what's wrong with arrogant statements on the net? This isn't formal publication, it's a conversation. Mostly polite, sometimes not. Pretty mild compared to some, we are told).

Control systems are all around us, and include us. Our proposition is that people are doing their everyday adapting, etc etc AS CONTROL SYSTEMS. Once you start analyzing them this way, it is obvious. PCT contributes a point of view.

It is probably too soon to ask about the practical value of PCT, by which I assume you mean changing people. There are a few therapists, a few people involved in education, a few in management. What they are doing, primarily, is teaching people that they, and others, are control systems. Adopting this point of view seems to help people understand themselves, others, and why some things they do don't and can't work in getting along with people, resolving conflicts, achieving goals, etc. But so far the evidence of success is anecdotal.

> h. Most scientists are concerned with not only describing and explaining phenomena, but also wish to predict and sometimes "control" them. Why does PCT deny the desirability of prediction...is prediction not always a part of human anticipation, whether in ordinary life or science?

Sure. People want to predict and control. We act to prevent anticipated errors all the time. I control for not running out of milk, toilet paper and cigarettes by buying enough to last at a predicted rate of consumption until the next time I plan to drive into town.

Scientists and others who want to control behavior are not likely to succeed unless they understand what behavior is FOR. The behavior they want to control is now viewed as a bunch of undesirable outputs. Ultimately, the only way to control those is by physically preventing them - lock up the bank robbers, shoot the abortionists, kick the trouble-makers out of school.

Everyone has goals. What their behavior is for is to achieve those goals - that is, to make their perceptions match their reference levels. If the behavior they are using to achieve those goals is unacceptable, in some cases simply pointing this out and asking for different behavior is enough. Sometimes teaching new ways of behaving that hadn't occurred to the person is enough. Sometimes nothing is enough, and then you have to get into the particular structure of reference levels in the individual person - what they want. Much of what they want may be unconscious, conflicting, and impossible. I can't get into how a psychologist can deal with that, but the PCT approach of going up levels and developing awareness can work - and I think provides a rationale for how therapy works. If therapy isn't available or successful, control comes back down to physical force.

> i. Where do reference levels or signals originate? In other words, if my control system has error signals, what constitutes the source of the error signals? What is stored that makes me think that I do not like or wish to accept certain input? How conscious are the processes of matching input to the reference signals?

Reference signals originate in the brain. Where in the brain depends on which reference signals you are talking about - for breathing, walking, speaking, painting a picture, etc. See BCP.

The sources of error signals are comparators, which receive reference signals and input signals, compare them, and generate a signal representing the difference. That isn't the question you thought you were asking, is it?

Not liking an input means that there is an emotional component to error. I don't know where reference signals hide out. Not the liver or the spleen.

The process of matching input to reference signals may be totally unconscious or totally conscious or in between. One problem with the self-regulatory folks is a preconception that it is all conscious - thus Karoly mis-cites Bill as claiming that humans are unique in the ability to achieve consistent ends by

variable means (so there, e. coli!). I personally have no idea how my body weight stays the same month after month. I am conscious of hunger periodically, and of buying and cooking and eating food, and I'm certainly aware of the need to finish the digestive process, though I don't consciously control intestinal contractions, or any of the digestive processes that precede them, or cellular metabolism. Lots of control systems doing their thing, all me, some conscious some of the time.

Mary Powers