Underpinnings of PCT; Systems Theory and PCT

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[From Bill Powers (940224.2030 MST)]
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Regarding the underpinnings of PCT:
There was no one in cybernetics/systems theory after Ashby's book in 1953 (Design for a Brain) from whom I learned anything about control theory and its role in behavior. Wiener's book of 1948, which I read in 1952 thanks to Kirk Sattley, got me started: the concept of feedback control, and the particular relations to behavior that he laid out, clicked in my mind as the obvious successor to all the psychological models I had ever heard of, including the one in which I then believed. Ashby's book gave me an organized view of how one would start applying these principles on a grander scale—it was as much his organization as his ideas that turned me on.

But Ashby lost me when he starting treating behavior as if it came in little either-or packages—I felt he had abandoned the main trail and was going off in unproductive directions. I especially felt, later, that his drive for the utmost generality was premature and based on only a sketchy understanding of control systems.

My main mentors were the control engineers themselves, and especially the pioneers of analogue computing and simulation: Philbrick, Korn and Korn, and Soroka, who not only provided the machinery and systematized the art of analogue computing, but developed penetrating insights into the principles of negative feedback. I never met any of my mentors, in or out of cybernetics: I just read their books and manuals. Wiener and Ashby inspired me to go back to the sources of the ideas that they had adopted. When I did, I gradually came to realize that neither of them had learned very much about control systems.

You question the primacy of control theory as used in PCT:

> (B) the particular negative feedback loop architecture that PCT advocates.

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

>... you have shown a very interesting result of SYSTEMS THEORY: namely, that a particular real-world phenomenon requires a particular system architecture, independent of the type of components.

But isn't this a platitude? It would be more surprising if a real-world phenomenon required NO particular system architecture. The phenomenon is simply an expression of the architecture; a different architecture would result in different phenomena. It has been the case for over 300 years that when we observe a phenomenon, we try to relate it to the properties of the objects involved in it. If a general theory is to prove useful or interesting, at some point it must tell us something we didn't already know.

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

> If BOTH (propositions mentioned) are true then you have correctly defined PCT with respect to ST, namely that it concerns systems OF ANY TYPE which demonstrate control phenomena and, equivalently, have correctly constructed negative feedback loops.

We have shown that a negative feedback system with a specific architecture will reproduce the phenomenon we call control (as opposed to what some others call control). Neither we nor any other person knows whether some other kind of system could not equally well explain the same phenomenon. We may not now know what such a system might be, but simply to assume that no other idea will ever be discovered is unwarranted; we have simply come up with one positive instance of a type of system that will create the observed phenomenon. To claim on this basis that PCT is the ultimate general theory of control is not legitimate and I do not make that claim. Any theory depends on the factual truth of its postulates. This is the Achilles' heel of all claims about "general" theories. You can show that a general theory is consistent with its premises, but theorizing will not show whether those premises are related to the real world or whether some other set of premises would not serve just as well and will not turn up tomorrow.

In discussing how ST people could be doing PCT "without knowing it," you say

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

How many of these people, in considering the operation of living systems, have considered the phenomena with which PCT is concerned? How many, in considering that feedback may be important, have correctly analyzed the way in which it is important, and the consequences that it creates? How many, in using feedback to describe some interesting result, have used it correctly, and with respect to a result that actually occurs as opposed to one that is only imagined? "Bingo" requires that you have markers on all five numbers, and I have seen no evidence of that outside PCT.

> > Also, it depends on if you take the term "living system" to STRICTLY mean a single organism or merely a system which INCLUDES an organism.

From your own writings, I glean that there is very little agreement in ST on what constitutes a "system" or how a living system differs from other sorts. If you can freely apply a basic term to vastly different situations, you may create the illusion of generality but what you actually achieve is vagueness. I don't really care what you call "a system." The term is hopelessly compromised by careless usage and lack of definition. What I care about is explaining behavior.

> >For example, is an economy a living system or not?

If we agree on an answer, what will we know that we don't know now? We can create categories at the drop of a hat, with any membership we please. Sure, if you want to include organisms and interactions among organisms in the same category, an economy is a living system. If you don't, it isn't. What difference does it make?

> >If so (I think this is cleaner), then for example any economist, whether an ST economist or not, who presumes that individuals have desires (like the desire for food) and make economic decisions based on satisfying those desires (like purchasing food) is ACTUALLY doing PCT.

No, that's too much! PCT is about what it is to have a desire, about the relationship of desires to actions and their consequences. It's about how making a decision or having a desire gets turned into just those actions which will have effects in the real world that result in an outcome that matches the decision or satisfies the desire, even if the action required differs from one instance to another. An economist who says only what you describe hasn't a clue about how any of these obvious phenomena come into being: he's simply describing the phenomena that need an explanation.

The conclusions you can draw from PCT match what anyone can observe under natural conditions. That says it is a good theory. It should surprise nobody that an economist who uses common sense will see that desires relate to what people purchase. That's commonplace, it's not an insight and it's not a theory. It's just a description of something ordinary in ordinary terms. That is where you would START if you wanted to apply PCT. You don't need PCT to conclude that people desire things and act to satisfy the desires. What you need PCT for is to explain how they can possibly do that. Can this economist of whom you speak explain how it is that when a person decides to purchase Grape-Nuts, the result is a long train of motor actions that carries the person from one store to another until the Grape-Nuts are in fact selected, carried to the checkout counter, and paid for? Of course not. The economist has no idea how a decision or a desire gets fulfilled, because the

economist doesn't know anything about PCT. I know of only one economist who does know anything about it.

> >... the study of systems of all kinds, NO MATTER HOW THEY'RE HOOKED UP, is ALSO very interesting (at least to me!), and THAT'S what ST is about.

I dispute whether ST is about systems of ALL kinds, and whether it has deduced the properties of ALL systems NO MATTER HOW THEY ARE HOOKED UP. It is about a certain range of systems that fall within the definitions of system with which ST begins. It is unlikely, furthermore, that ST will have deduced everything there is to say even about systems within that range, because essentially no time is spent exploring the properties of specific examples of systems, and looking for unexpected behaviors in natural examples of those systems (when the systems are physically realizable). Or put it this way: in general statements about systems, how come I can so often think of counterexamples?

Everyone is entitled to be interested in whatever seems interesting. Conflict arises, however, when there is competition to see whose idea anticipates whose idea. A common strategy, in and out of science, is for people to go up a level of abstraction, trying to make true statements that anticipate true statements that others might make at a lower level. You say, "It's going to rain tomorrow." I say "There is a chance of rain tomorrow," thereby seizing the opportunity to prove me wrong and you right if it doesn't rain tomorrow. And the third guy, looking for another step up, says "Of course it could snow as well," thus showing that he has a more general understanding of the situation than either of us. In this game of who is rightest, the temptation is strong to rely on more and more remote abstractions with less and less chance of being contradicted by the facts.

But in my book, it's the guy who says "It's going to rain tomorrow" who wins in the end. Even if this guy is wrong, he is going to be less wrong the next time, and finally he will be right most of the time. The guy at the top level of abstraction will see to it that he is right all of the time, but that will be only because he has covered his ass in all possible ways. There are no prizes for predicting that tomorrow there will be weather, even if that should prove to be true.

Best, Bill P.