Thread on the theme of an "open mind,"

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

Date: Tue Mar 14, 1995 2:09 pm PST
Subject: Open loops, open minds

[From Rick Marken (950314.1100)]

Bruce Abbott (950313.1325 EST) --

> I'm afraid I don't see it [the contest between EAB and PCT] in those black-and-white terms. Perhaps there are elements of both views that are correct -- I'm willing to keep an open mind to that possibility.

[EAB stands for Experimental Analysis of Behavior -- the study of conditions which influence operant behavior]

An open mind, like an open loop [Bill Powers (950313.1540 MST)--PCTDOCS\SIMPLE_F.AST], is not always all that it is touted to be. Do you have an open mind about the structure of the universe? the composition of matter? the origin of species? I hope you do have an open mind about these things but I suspect that there is a limit to that openness -- or are you open to the possibility that the earth is the center of the universe, that matter is made of indivisible atoms and that all species were created simultaneously (on the fifth day)? I think it's good to be open-minded about some "possibilities" (those that have not been appropriately rejected by science) but not others (those that have). The latter type of open-mindedness -- open-mindedness about possibilities that are no longer possibilities -- is simply wishful thinking. This kind of "open mindedness" is the reason PCT has gotten nowhere in thirty years; reinforcement, stimulus control, discriminative stimuli, etc etc. are still seen as "possibilities".

It is hard for me to be open-minded about EAB in the same way that it is hard for me to be open-minded about creationism. EAB is inconsistent with the facts of behavior just as creationism is inconsistent with the facts of speciation. One fact of behavior is that organisms are locked in a feedback loop with respect to their sensory input: there is no way an organism can "behave" without producing sensory consequences for itself; to the extent that behavior depends on these sensory consequences, the organism is locked in a feedback loop. The other fact is that organisms produce consistent results in an inconsistent world -- that is, they _control_. These two facts can only be handled by a model of behavior that views organisms as perceptual control systems -- a model that is completely inconsistent with the EAB view of organisms.

Claims that EAB is consistent (in some ways) with the facts of behavior seem to me to be precisely analogous to claims that Genesis is consistent with the fossil record.

Best Rick

Date: Tue Mar 14, 1995 4:15 pm PST
Subject: Re: Open loops, open minds

[From Bruce Abbott (950314.1900 EST)]

>Rick Marken (950314.1100) --

> It is hard for me to be open-minded about EAB in the same way that it is hard for me to be open-minded about creationism.

Rick, I was tempted to entitle this post "closed loops, closed mind" because it struck me as a cute play on the title to your post (and its contents), but then I thought the better of it as it would convey an attitude I don't hold. How open- or closed-minded one is at a given moment to a given idea depends on a bunch of factors, many of which we aren't even conscious of. You've drawn your conclusions about EAB (and, indeed, the whole of conventional

psychology), for reasons unique to you. In my current state of understanding of both PCT and traditional psychology, I'm still quite willing to explore possibilities you evidently have already ruled out. If you're trying to convince me that the effort is not worthwhile, making bald assertions comparing EAB to creationism ain't gonna do it.

In my view, you are mistaking the part for the whole in rejecting the whole of the traditional approach to learning and behavior simply because one of its fundamental principles is wrong. It's like throwing out Boyle's Law because you don't subscribe to the molecular theory of gases when in fact, Boyle's law is just an empirically established relationship that will remain (approximately) true regardless of what theory is used to explain it. Does PCT offer a theory of perception? Does it explain how memory works? How, if at all, does it explain the phenomenon known as association? I'm not asserting that PCT cannot do these things, only that it is worthwhile asking whether it does, and if it does not, asking what ideas from areas of psychology outside PCT might be worth considering when developing our models.

Now these proposals of mine may be ideas you've already considered carefully and rejected; if so, I'm more than happy to hear your reasoning. I do appreciate your input and consider it carefully, even if we don't always end up agreeing. In fact, one of the best things about CSG-L in general is that it provides such an excellent forum in which to introduce and debate these issues with intelligent, thoughtful people who care about them.

Regards, Bruce

Date: Tue Mar 14, 1995 10:00 pm PST
Subject: Re: Open loops, closed minds

[From Rick Marken (950314.2130)]

Bruce Abbott (950314.1900 EST) --

> In my view, you are mistaking the part for the whole in rejecting the whole of the traditional approach to learning and behavior simply because one of its fundamental principles is wrong.

I believe that the "whole of the traditional approach to learning and behavior" is built on a _model_ of how the nervous system produces behavior: the cause-effect model of behavior. The model turns up in various guises -- S-R, selection by consequences, information processing, etc -- but it is the same basic model. If you don't buy the notion that a single, simple model (cause-effect) is the basis of conventional psychology (and I think many people don't) then I don't think you will see anything revolutionary about PCT.

PCT shows that the cause-effect model cannot possibly behave like a living system. This is because living systems produce consistent behavior despite the fact that they are not the only cause of that behavior: living systems control. Only control systems control.

If living systems control then the cause-effect model of behavior (in any form) must be rejected. Thus, any demonstration of control, like a simple tracking task, rejects the cause-effect model. Such a demonstration will also show why control is likely to be mistaken for a cause-effect process; since control actions must compensate for disturbances to a controlled variable, an observer is likely to see the disturbance as the cause of behavior -- even when this disturbance is invisible to the controller.

> It's like throwing out Boyle's Law because you don't subscribe to the molecular theory of gases when in fact, Boyle's law is just an empirically established relationship that will remain (approximately) true regardless of what theory is used to explain it.

I think it's more like throwing out the molecular theory of gases because it doesn't account for Boyle's law. Of course, the molecular theory of gases _does_ account for Boyle's law. I reject conventional psychology because the cause-effect model does NOT account for "Powers' law", the fact that organisms

maintain certain variables in constant or varying reference states despite disturbances: that is, the cause-effect model doesn't account for Powers' law of control.

> Now these proposals of mine may be ideas you've already considered carefully and rejected; if so, I'm more than happy to hear your reasoning.

I reject the aspects of conventional psychology that are based on a cause-effect model of behavior -- a model that cannot account for the controlling done by living organisms. The evidence against conventional psychology is everywhere there are people who control: when you consistently point at your glasses, open the door, lift the book, make the bed, pour the coffee, teach the class, etc you are rejecting the cause-effect model of behavior. And PCT shows that you are also rejecting the value of most of the observations of behavior made in the context of the cause-effect model because these are likely to be observations (such as the observation of "stimulus control") of irrelevant side effect of controlling.

PCT explains control; conventional psychology doesn't explain control or even recognize that it exists. I wonder, therefore, why you think conventional psychology merits any serious attention at all. That is, if you buy PCT (a model of the controlling done by living systems) then what do you imagine could possibly be learned from a "science" (conventional psychology) that has nothing to do with control?

One last point on open and closed minds. I would say that an open mind is one that is willing to be changed by the evidence. After at least a month of providing evidence that (from my perspective) clearly showed that consequences cannot possibly select behavior (the notorious E.coli series) it seems to me that nobody changed their mind; Bill, Tom and I still believe that behavior selects consequences, not vice versa (and we are convinced that the E. coli demos provided resounding evidence that this is the case); I think that you still believe that consequences can select something about behavior (like its parameters). So whose mind is closed? When your colleagues who are analyzing the tracking task see no challenge to the IV-DV approach to research in the fact that the correlation between cursor and output is 0.1 while that between invisible disturbance and output is .99 are they being closed minded? Or are we being closed minded to think that such results deal a death blow to the basic research paradigm in psychology?

I really don't know the answer to these questions. If you (or Bill) do, please let me know.

Best Rick

Date: Wed Mar 15, 1995 11:04 am PST
Subject: Re: causalgia; traditional approach

[From Bill Powers (950315.0739 MST)]

Bruce Abbott (950314.1900 EST) (writing to Rick Marken) --

> In my view, you are mistaking the part for the whole in rejecting the whole of the traditional approach to learning and behavior simply because one of its fundamental principles is wrong.

From my point of view, the wrongness of the principle is only a symptom of the basic problem. To me, the basic problem is the general approach to learning and behavior that allows making causal statements without reference to the internal organization of the behaving system.

The concept "the stimulus" is a primary example. A stimulus is simply an impingement of physical energy on sensory organs. The only properties of a stimulus that matter are those that result in raising the neuron past the threshold of firing. Once that has happened, the stimulus itself has no further influence on subsequent events; everything that happens after the neuron fires is due to properties of the nervous system and the rest of the organism. What the organism does during and after the stimulus event depends

entirely on what the organism makes of this signal, what signal it wants, what other signals are present, what functions are applied to the signals, and so forth.

That's the point of view I bring to my attempts to understand behavior. But what do behavioral psychologists do? They treat stimuli not in terms of actual processes at a sensory interface or in a brain, but in terms of what they themselves experience of the environment (through their own senses). They speak of stimulus-objects and stimulus-events as if there is something about the objects and events they perceive in the environment that endows them with special properties that relate to behavior. They speak of masking stimuli, and salient stimuli, and noxious, aversive, or rewarding stimuli, and stimuli competing for effects on the organism, and supernormal stimuli, and controlling stimuli. In short, they speak as if the environment contains special nonphysical properties that have special effects on organisms.

This I simply can't buy. I just don't believe that the environment contains any such properties. I think that psychologists are attributing to the environment properties that belong _inside the organism_. If you examine the stimulus objects or events themselves, in isolation from organisms, you find none of these properties; there is no test to reveal them, and no justification for claiming that they exist as part of external nature. They are a metaphor that has got out of hand. They are figments of the imagination.

Do you really, literally, believe that there is something about a pellet of food that can cause a change in the relationship of behavioral actions to objects and events in the local environment? When you call a piece of kibble a "reinforcer", that's what you're literally claiming. From what I know of you from your writing and programming, I simply can't accept that you believe literally in reinforcement or in controlling stimuli or in any of that stuff. And if you don't believe literally what you're saying, why go on using that language? It's like an atheist saying "bless you" when you sneeze, or a physicist saying he hopes that a lot of neutrinos want to enter his apparatus today. People do say such things in jest, but they would be insulted to hear themselves described as if they took their own words literally.

Best to all, Bill P.

Date: Wed Mar 15, 1995 1:31 pm PST
Subject: Re: traditional approach

[From Bruce Abbott (950315.1545 EST)]

>Bill Powers (950315.0739 MST) --

> From my point of view, the wrongness of the principle is only a symptom of the basic problem. To me, the basic problem is the general approach to learning and behavior that allows making causal statements without reference to the internal organization of the behaving system.

This criticism is specific to EAB, and I agree. (Shhhh! They'll take my union card away if this gets out!)

> Do you really, literally, believe that there is something about a pellet of food that can cause a change in the relationship of behavioral actions to objects and events in the local environment? When you call a piece of kibble a "reinforcer", that's what you're literally claiming. From what I know of you from your writing and programming, I simply can't accept that you believe literally in reinforcement or in controlling stimuli or in any of that stuff.

Well, you're right, I don't, except for the part about "in any of that stuff," because that covers a lot of territory.

> And if you don't believe literally what you're saying, why go on using that language?

First of all, it is a vocabulary with which I am familiar and comfortable. I see no good reason for abandoning perfectly good descriptors of behavioral phenomena (e.g., "stimulus control") and then having to convey what I'm talking about in lengthy, awkward sentences. (Technical terms are invented to eliminate that problem.) Third, I want to be able to "translate" our models back into behaviorist terms in order to demonstrate to behaviorists how PCT accounts for the phenomena to which they have applied these names.

I think it important to distinguish the descriptive from the explanatory application of these terms. For example, "reinforcement" can refer to an empirical observation (e.g., when I follow the rat's lever-press with immediate access to food, and the rat has been deprived of food for awhile, the rate of lever-pressing increases over time), or to a theoretical explanation (e.g., following a response with a reinforcer acts to increase the future probability of the response). I can certainly talk about reinforcement, or extinction, or stimulus control as empirical phenomena without necessarily subscribing to the theoretical view within which the terms arose. I recall having recently read a provocative analysis of classical conditioning in which Pavlovian terms like "conditioned stimulus" were used. Sounds like something I would do--I wonder who wrote it? (;->

> People do say such things in jest, but they would be insulted to hear themselves described as if they took their own words literally.

You mean words like "stimulus control" taken to mean literally that the stimulus has the purpose of trying to control someone? Who would be silly enough to do that? (;->

Regards, Bruce