

Behaviorist terms. Theory, imagery, language

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

Date: Tue Apr 11, 1995 3:42 pm PST
Subject: Re: Extremism

[From Rick Marken (950410.2145)]

Bruce Abbott (950410.1100 EST) --

> So, Rick, how about offering a reasoned reply to my argument? What DO you suggest we use in place of "predictive cue" or "anticipation?"

Well, as you can tell, reason is not my strong suit. But I'll give it a shot.

I suggest calling them what they are. Here is the beginning of a glossary translating the animistic terms of behaviorism into the scientific terms of PCT:

Animistic term	Scientific term
Predictive cue	Disturbance variable
Anticipation	Control of imagination
Reinforcement	Controlled variable
Schedule of Reinforcement	Feedback function
Discriminative stimulus	Perceptual variable
Stimulus control	Response to disturbance
Control by consequences	Control of consequences

Feel free to add to the list. Because animistic terms are based on a magical view of the world they often have more than one scientific meaning. For example, a predictive cue can refer to a disturbance variable (like the target in a tracking task) or to the value of a perception that is influenced by that disturbance variable (such as the rate of change in the distance between cursor and target). I used the scientific term that seemed to capture the most common use of the animistic term but feel free to use all relevant scientific terms for an animistic term when completing the glossary.

> I'm really confused at the debate we seem to be having about "anticipation," as I didn't think I was saying anything controversial;

That's why I said PCT is no fun for conventional psychologists. You are confused because you assume that PCT accounts for phenomena that conventional psychologists think are important. But, as Bill Powers (950410.0900 MST) pointed out, the "phenomena" of conventional psychology are contaminated by theoretical interpretation. Even the things you think of as pure, objective phenomena (like "anticipation") contain theoretical assumptions that are being made in order to avoid facing the fact that organisms control.

> Meanwhile, here's Rick, off on some tangent having little to do with this discussion (so far as I can tell) ranting about "myths" and asserting that phenomena like "anticipation" don't exist at all.

Thanks for providing the opportunity to present a nice, clear example of how you try (probably unconsciously) to make it seem like Rick is off all alone, ranting about extremes. See if you can see anything familiar in the following rantings:

> So some APPARENT [emphasis mine] anticipations might arise from continuous control of a relationship.

> If we want to model anticipatory behavior, let's use the theory at hand and see what it can do. I think we'll find that many APPARENT EXAMPLES [of anticipation -- my emphasis again] (like a few I've mentioned above) can be handled with a model that doesn't actually involve any anticipation at all.

Whoever made these statements seems to believe that examples of the "phenomenon" of anticipation probably don't involve anticipation (prediction of the future) at all. Was this said by Rick, the ranting extremist? Why no. It was none other than Bill Powers (950410.0900), the (closet) ranting extremist.

> Of course, I offered the counterargument that these things are not "myths," they are objective phenomena, which, as it turns out, can be nicely explained via PCT.

And PCT shows that they don't involve anticipation or prediction.

> Do I get a response to this argument?

I've tried to explain the PCT position on "anticipation" several times -- not very well, apparently. I think my best attempt so far is in my reply this morning (950410.0920) to Wayne Hershberger.

> How about a reply that addresses the argument?

See my post to Wayne. It's a start. The basic answer is "control of perception".

> Predictive cues are rarely 100% reliable, and they don't always arrive at the most opportune moment. This does not prevent them from being powerfully useful on most occasions on which they occur.

Well, it sounds like a lot of faith is involved here. What, for example, does the organism do on those (not infrequent) occasions when the predictive cues turn out to be completely wrong? Die?

Me:

> Oh no. I thought you had become a PCTer.

Bruce:

> I'm just saying that I'm not Rick Marken.

Lucky for me;-)

> I'd guess the one that comes closest to your description [of a predictive "control" system] would be the one presented a short while ago by Bill Powers, proposed as a model for classical conditioning.

I don't remember Bill's model of classical conditioning as being anything at all like my description of predictive control. What I described was not even a control system. It was a stimulus-response system that keeps the cursor on target because the stimulus ($x(t)$) is one of those "helpful" little predictive variables that happens to generate just the right responses. If $x(t)$ goes south (as a predictor) so does tracking -- and there's nothing the system can do about it.

By the way, thanks for the report on the BAAM talk. And don't be disappointed by the turn-out. When we go to conventional psychology meetings we count it as a great victory if we get more than two; 20 is a rock concert;-) I was a little disappointed with your description of the talk, however. It sounds like the emphasis was on the theory rather than on the phenomenon of control. It seems like nobody was "blown away" by the theory, which is not very surprising. Indeed, I would imagine that many in your audience were already familiar with control theory. What they might have been less familiar with is the nature of control as it appears in operant studies? Did you tell them how to tell whether or not an organism is controlling a particular variable in an operant experiment? Did you explain how reinforcement is actually a controlled variable and that it's apparent effect on behavior is an illusion? Did you explain why conventional operant research tells us almost nothing about what organisms are doing (controlling)? Did you explain why attempts to control behavior using reinforcement are an almost sure fire way to create interpersonal conflict? Or would these little points (facts) have been too "extreme"?

Best Rick

Date: Wed Apr 12, 1995 7:38 am PST
Subject: Re: Extremism

[From Bruce Abbott (950411.1620 EST)]

>Rick Marken (950410.2145)]

>>Bruce Abbott (950410.1100 EST) --

>> So, Rick, how about offering a reasoned reply to my argument? What DO you suggest we use in place of "predictive cue" or "anticipation?"

> I suggest calling them what they are. Here is the beginning of a glossary translating the animistic terms of behaviorism into the scientific terms of PCT:

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Well, that's a start. I don't follow the one for reinforcement. I suggest a closer approximation would be "reinforcER."

I also don't think "disturbance variable" quite captures the essence of what I have defined as a "predictive cue," as it fails to differentiate what I deem to be a crucial difference: predictive cues not only act as disturbances at one level, they predict disturbances at another level. I have a similar problem with your definition of "stimulus control," but I am aware that in both these cases you see no need for special terms for the phenomena that define these terms.

> Even the things you think of as pure, objective phenomena (like "anticipation") contain theoretical assumptions that are being made in order to avoid facing the fact that organisms control.

I don't think we should let that possibility interfere with scientific analysis. Objective phenomena are objective phenomena; we are free to describe and explain them any way we please, with whatever theoretical assumptions we choose to make.

> Thanks for providing the opportunity to present a nice, clear example of how you try (probably unconsciously) to make it seem like Rick is off all alone, ranting about extremes.

Sorry, but it just seems to me that your concerns are off the mark; I'm talking about ordinary control systems and you're responding as if I'm talking about "feedforward" and S-R mechanisms, which I am not. Bill seems to have made the same mistake. Also, I'm not really concerned whether you and Bill are of like mind on these issues; what does concern me is when that position appears to be at odds with mine. I try very hard to understand why you take the view that you do, because it is likely that I've missed something important and could learn a valuable lesson from the attempt. However, I don't believe that you, or Bill, or I, for that matter, have a lock on the "truth;" we are all fallible human beings. Therefore, my criterion for belief is not whether Bill or you agree with my position, but whether that position makes sense to me.

> Whoever made these statements seems to believe that examples of the "phenomenon" of anticipation probably don't involve anticipation (prediction of the future) at all. Was this said by Rick, the ranting extremist? Why no. It was none other than Bill Powers (950410.0900 MST), the (closet) ranting extremist.

The problem here is that you want to define anticipation as involving some explicit algorithm by means of which the system generates an output in response to the "predictive cue." I don't. For me, the heart of anticipation is that the system begins reacting to a lower-level disturbance in advance of (or timed with the occurrence of) that disturbance. Describing how it achieves this miracle is the job of the explanation. Given these different definitions of the term, it is hardly surprising that we disagree whether a given example involves anticipation. It turns out that our explanations are basically identical, which is why I've had such a hard time understanding all the fuss ("ranting").

> I've tried to explain the PCT position on "anticipation" several times -- not very well, apparently. I think my best attempt so far is in my reply this morning (950410.0920) to Wayne Hershberger.

Yes, I read it and found little to disagree with, except for the question Wayne subsequently raised as to how it just happens that the disturbance produced by the predictive cue produces an action that tends to reduce the effect of the disturbance to the lower-level system. Seems more than coincidental to me.

>> Predictive cues are rarely 100% reliable, and they don't always arrive at the most opportune moment. This does not prevent them from being powerfully useful on most occasions on which they occur.

> Well, it sounds like a lot of faith is involved here. What, for example, does the organism do on those (not infrequent) occasions when the predictive cues turn out to be completely wrong? Die?

Usually, there is an unnecessary action on the part of the lower-level system to counter a disturbance that fails to materialize, these often then produce disturbances of their own which must be countered. As I mentioned before, failing to prepare for the disturbance-to-come usually leads to more serious problems. If the fly doesn't move, then yes, death is a likely consequence.

>> I'd guess the one that comes closest to your description [of a predictive "control" system] would be the one presented a short while ago by Bill Powers, proposed as a model for classical conditioning.

> I don't remember Bill's model of classical conditioning as being anything at all like my description of predictive control. What I described was not even a control system. It was a stimulus-response system that keeps the cursor on target because the stimulus ($x(t)$) is one of those "helpful" little predictive variables that happens to generate just the right responses. If $x(t)$ goes south (as a predictor) so does tracking -- and there's nothing the system can do about it.

Correct. And, as I never tire of saying, your model (which I believe you thought, for reasons I can only guess, was my model) is NOT my model. Imagine my surprise when I applied Bill's model to the "anticipation" problem and found myself facing stiff opposition from both of you! In at least some of the anticipatory situations we've been discussing, the "predictive cue" I've been speaking of is nothing more (or less) than the CS in that model. Ironic, isn't it?

Regards, Bruce

Date: Wed, 19 Apr 1995 09:04:34 -0700
Subject: Language, Models

[From Rick Marken (950419.0900)]

Bruce Abbott (950418.1515 EST) --

> I like Chuck Tucker's (950418) suggestion, copied below.

>>CHUCK TUCKER 950418

>> Let us get back to the glossary idea and preface all posts with "I mean by X" so such silly discussions can be avoided.

I don't like this idea at all. The notion that these "silly discussions" are based on a failure to define terms strikes me as misleading. We are using the language many of us have been using for over 30 years to do what many of us have been trying to do for over 30 years -- to communicate our thoughts and experiences to others. When it comes to talking about PCT, I have found that this ordinary language, combined with "pointing" at the behavior of working models, communicates the basic ideas of PCT just fine. There are some terms that require more careful definition -- "control", for example -- but there are not many of these special terms so I think we should be able to talk about PCT without having a glossary in hand.

It seems to me that linguistic "nitpicking" occurs (people saying things like "what, exactly, did you mean by "prediction", "reward", "information", "consequence", "of", "by", "the", etc") when people are trying to make a non-PCT idea seem consistent with PCT -- or vice versa. I think this is what is going on in the discussion of "prediction" and "anticipation". Bruce Abbott, for example, sees nothing wrong with viewing the perception of the rate of change of a variable as an example of "prediction" or "anticipation" in control. Bruce said, for example:

> The use of the target velocity to predict future target position can be considered a form of "anticipation"

which elicited the following beauty from Bill Powers:

> Gravitational acceleration can be considered a form of affinity; momentum can be considered a form of impetus ... Control of consequences by behavior can be consider a form of control of behavior by consequences.

Why all the fuss? Because Bruce's statement evokes the wrong imagery about the behavior of control system and running to the glossary won't help. Control systems don't "use" perceptions; they control them. Target velocity is not used by a control system to predict the future state of anything; it is part of a present time perception that is being controlled relative to a present time reference -- the way control always works. So whether what Bruce described can be "considered a form of anticipation" is moot because control systems don't "use target velocity to predict future target position".

Language does matter. And it seems to me (since it worked for me) that everyday language (sans glossary) is completely up to the task of describing the phenomenon of control and the model thereof. If you look carefully, you will see that the only time people want to be VERY precise about what they mean by a term is when they want to make ideas that are inconsistent with PCT seem like they are NOT. A wonderful example of this occurred in the "silly discussion" of information theory. "Information" was defined and redefined in the hopes that one could talk about it in a way that did not contradict PCT.

There are ways to talk about "information" (and "prediction" and "anticipation", etc) that ARE consistent with PCT. But this can be done without looking for the "precise" definitions of these terms. It can be done by applying the term (as ordinarily understood) appropriately to control. Perception does have information about the state of a controlled variable. This is just another way of saying $p = f(i)$. Perception just doesn't have any information about the cause of the state of the controlled variable. The output of a control system sometimes does anticipate the disturbance to a controlled variable. But this is just another way of saying that $o = -d$ and that $d(t) = f(d(t-dt))$. The control system itself does not operate by anticipating or predicting anything.

Best Rick