From: Richard Marken <rsmarken@gmail.com>

Date: Sat, 5 May 2018 14:59:53 -0700

Subject: Approaches to PCT and Friction on CSGNet

To: csgnet@lists.illinois.edu, mol@mail-list.com

[From Rick Marken (2018.05.05.1500)]

RM: I must admit that when I posted my “Minority Report”, announcing that I would stop unsolicited posting to CSGNet, I was busy feeling rather sorry for myself. Since Bill passed away I have been alternately surprised, frustrated, angered and hurt by the response to my posts to CSGNet. But over the last few days I’ve had what I think was a nice “up a level” experience, which basically involved realizing that one of my highest level goals is to present an accurate account of PCT that can be the basis of furthering PCT science; and that I realized that I can achieve this goal by, among other things, posting to CSGNet, regardless of how well those posts are received.

RM: So I will start my return to unsolicited posting by explaining why the “fundamental misunderstandings” about PCT that I mentioned in my “Minority Report” are, indeed, fundamental misunderstandings. (This post is not really unsolicited; after giving me some MOL therapy, my co-author on “Controlling People”, Tim Carey, suggested that it would be useful if I would post an explanation of why these misunderstandings about PCT are, indeed, misunderstandings, and maybe even post a copy of it to the MOL list.) So here it goes:

RM: First here is the list of misunderstandings about PCT that I posted in my “Minority Report”:

* Organisms control only perceptions, not the aspects of the environment that correspond to those perceptions.
* The test for the controlled variable is not an essential component of PCT-based research.
* PCT shows that you can’t control the behavior of another person.
* Social stability arises from interpersonal conflict.
* The power law of movement is not an example of a behavioral illusion.

RM: I believe that all of these misunderstandings are a result from taking a “mathematical-logical” rather than a “scientific/engineering” approach to PCT. The mathematical-logical approach treats PCT as a set of axioms from which conclusions about behavior are derived like mathematical theorems. The scientific/engineering approach, on the other hand, treats PCT as a set of tentative guesses about the mechanisms that explain the behavior that is actually observed. The correctness of PCT explanations of behavior is, therefore, evaluated very differently by these two approaches.  The mathematical-logical approach evaluates the correctness of these explanations in terms of whether they are properly derived from the theoretical “axioms” .The scientific/engineering approach, on the other hand, evaluates the correctness of these explanations by testing to see whether they correspond to what is actually observer in appropriate tests or demonstrations.

RM: It’s easy to see how taking the mathematical-logical approach to PCT could lead one to conclude that the five misunderstandings listed above are actually correct implications about behavior from a PCT perspective. For example, PCT does say that organisms control only a perceptual signal. This perceptual signal is not the same as the aspect of the environment to which it corresponds. So it’s logically correct to conclude that PCT says that organisms control only perceptions, not the aspects of the environment that correspond to those perceptions. And the logical corollary to this is that one cannot necessarily tell what perception an organism is controlling by testing to see whether a particular aspect of the environment is being controlled. So the test for the controlled variable, which involves only testing to see whether certain aspects of the environment are under control, can’t possibly be an essential component of PCT because there is no necessary relationship between any aspect of the environment that might be under control and the perceptual variables that the organism is actually controlling.

RM: Because PCT says that a person’s behavior involves control of a perceptual signal that is accessible only to the person themselves it is also logical to conclude that it is impossible to control that person’s behavior. And PCT also shows that, under the appropriate conditions, when several control systems control the same perceptual variable relative to somewhat different reference levels, there will be a conflict but the perceptual variable will be maintained at virtual reference level that is the average of the reference levels of all the systems involved in the conflict. Since the perceptual variable is being stabilized by a “society” of control systems, it is logical to conclude that social stability arises from interpersonal conflict.

RM: Finally, PCT says that there will be a consistent, negative relationship between the disturbance to a controlled variable and the actions that compensate for the effect of that disturbance. The power law of movement seems to be an example of such a disturbance-action relationship; the power law is the observation that when we make curved movements we slow down in proportion to the degree of curvature through which we are moving. So it looks like variations in the speed of movement are actions that compensate for the disturbance of variations in the degree of curvature through which the movement is being made. So the power law is not a behavioral illusion because that relationship is not assumed to reflect characteristics of the organism that transform the “stimulus” of curvature in to “response” of movement speed.

RM: So all five misunderstandings of PCT listed above are perfectly logical deductions about behavior when one takes the mathematical-logical approach to PCT. They can only be seen as misunderstandings when one takes the scientific/engineering approach to PCT. From the scientific engineering perspective the idea that organisms control only perceptions, not the aspects of the environment that correspond to those perceptions, is instantly seen as having things completely backwards; the idea that organisms control perceptions is a theory designed to account for the observation that organisms can be seen to control aspects of their environment. What we know for a fact is that organisms control aspects of their environment; we guess that this happening because organisms are controlling perceptual signals that are analogs of variations in the aspects of the environment that we see being controlled.

RM: We use the test for the controlled variable to determine the aspects of their environment that organisms control. So the idea that the test for the controlled variable is not essential because the results do not necessarily reveal the aspects of the environment that correspond to the perceptions that are controlled is nonsense from the point of view of the scientific/engineering approach to PCT. The test for the controlled variable is the only basis we have for determining what perceptual variables an organism might be controlling. And it is easy to demonstrate (<http://www.mindreadings.com/ControlDemo/Mindread.html>) that the results of the test for the controlled variable provide a very reliable indication of what aspects of the environment are under control and, thus, what perceptions we should imagine are being be controlled; the theoretical perceptions we imagine to be controlled correspond exactly to the aspects of the environment that we have found to be controlled using the test for the controlled variable.

RM: A crucial difference between the mathematical-logical and scientific/engineering approach to PCT to in the attitude toward observation and test. Those who take the mathematical-logical approach to PCT express a particular disdain for observation and test in the form of what are called the PCT “demos”, such as the computer demos  at <http://www.mindreadings.com/demos.htm>. But these demos have been an essential part of the scientific/engineering approach to PCT from the very start, as can be seen in the lovely “portable demos” described in the 1960 Powers, Clark and McFarland article “A General Feedback Theory of Human Behavior: Part II” reprinted in Living Control Systems, pp 25-45.  Using demos such as this it is easy to show that you can control the behavior of another person (<http://www.mindreadings.com/ControlDemo/BehavioralControl.html>) and that that the power law of movement is an example of a behavioral illusion (as shown by the demo described in the “Facts of Logic” section of Marken and Shaffer, 2018 (<https://www.dropbox.com/s/3m51ko4vs1xdult/MarkenShafferReappraisal.pdf?dl=0>).

RM: The scientific/engineering approach to PCT doesn’t deny that social stability can arise from interpersonal conflict. However, there is, to my knowledge, no demonstration of that it can. Such a demonstration would involve showing that groups of real organisms produce stable results like those produced by the simulations of multiple control systems control the same variable relative to different references. Until such demonstrations are produced, the idea that social stability can arise from interpersonal conflict must remain a conclusion that is true only from the perspective of the mathematical-logical approach to PCT.

RM: I’ll conclude this already rather long post with a copy of an even longer post from Bill Powers. This post was sent to me by someone who is not on CSGNet but is one of the few people who respect my work on PCT (and the feeling is mutual). It’s a post from 1995 where Bill provides and analysis of why he thinks there is so much friction between himself and Martin Taylor. I’m posting it to show that Bill’s approach to PCT was most emphatically the scientific/engineering approach. And that he was not a big fan of the mathematical-logical approach. I have bolded some of the sections that I think are particularly germane to the problems I have been having in discussions on CSGNet.  Like Bill, who says at the end of this post “I have no illusions about changing your style to correct what I see as mistakes”, I also have no illusions about changing the approach to PCT of those on CSGNet to correct what I see as mistakes. But I hope this helps you see why the friction on CSGNet exists.

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Date: Wed, 17 May 1995 05:24:50 -0600

Subject: Friction

Hello, Martin -- (no CCs)

I have become increasingly frustrated with our communications and have been trying to figure out what is wrong. In the middle of the night a possibility occurred to me. A bit of browsing through the archives -- not exhaustive -- has brought up a number of topics all of which have led me to the same frustration with your approach that I am currently experiencing. The ones I recall now, which are probably not all of them, are (in no particular order)

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Information about the disturbance flowing through the perceptual signal to enable control to take place.

The perceptual function composed of an S-shaped response followed by an integrator.

A discussion on bandwidth in relation to maximum realizable gain in a control system.

The “bomb” effect.

Flip-flops or cross-connections as explanations of category perceptions, association, contrast.

Categories as existing parallel to the analogue hierarchy. Control system organization as being a model of the environment.

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I finally realized that there is a common element in your treatment of all these subjects. It is very much like the way you took off on the basis of assuming that my limitation of the disturbance magnitude in Hans’ set of disturbances was due to insufficient output strength in my model, which in turn was caused by too short a word length. Having assumed the truth of your premise without particularly checking to see if it was true, you then built a series of plausible deductions from the assumption, which happened to support a general principle you were trying to get across. Unfortunately, the premise was false. I would not be surprised, however, if you decided that even if the premise happened to be false in that case, the deductions you made from it were probably true.

In each of the above subjects, you began with a theoretical possibility and developed it just far enough to see some possible implications of it. Then you quickly built a plausible and ever-more-detailed series of deductions from those implications, and arrived at what seemed to you an interesting new phenomenon. You could see in your mind’s eye how the Bomb would sit there ticking, ready to go off if the right combination of disturbances occurred.

You could imagine information flowing from the disturbance through the perceptual system to the output, where it got used up in producing the effects that would counteract the disturbance. You could see the s-shaped curves and integrators acting like a perceptron for the input part of a control system. You could see a whole hierarchy of discrete categories with hysteresis, running in parallel to the analog hierarchy. And the fact that you could see in principle how certain other phenomena might flow from the initial conceptualizations was enough to convince you that the initial conceptualizations must be correct.

**So what happens is that the tail wags the dog: the attractiveness and richness of the conclusions drawn from the initial assumptions convinces you that the initial assumptions must have been right.** And once that has happened, you forget completely that the initial assumptions were never established as true, and you speak of the conclusions as if they were now established facts; you even start using them to prove other conclusions.

**The name of this type of reasoning process, or one name, is of course “mathematics.” In mathematics (including logic, or is it the other way around), it doesn’t matter whether the initial assumptions are factually true or in some way supportable by evidence.** The assumptions are simply the initial process of setting up the chessboard with a problem, so you can work out a solution to it. Once the field of play is established, you can then start working out the theorems and proofs, encountering beauty and entertainment at many stages along the way. You begin to get a feel for the system you have created, so its major conclusions become familiar parts of that conceptual world. These major conclusions become theorems on which to build further; they get names like “information about the disturbance” and “The Bomb” and “crossconnections.” Since they have been derived by correct reasoning from the premises, there is no reason to doubt them any more; they become real. The premises drop out of sight; they were never very important anyway, except as a way to get the game started. The real fun is in building the structure of ideas on those premises.

Judging from various comments you have made about your interests and preferences, I don’t think that this is a completely inappropriate assessment of your modus operandi. **Your approach is not the engineering approach to a physical system, but the mathematical-logical approach to a hypothetico-deductive system.**

**This hypothesis explains to me your disdain for “mere demonstrations.” If you have worked out the logic correctly, what is the point in doing an actual demonstration of it, and doing different demonstrations to bring out one point or another?** If you understand addition, what is the point of demonstrating that 9 + 1 = 10, and 8 + 2 = 10, and so forth? If you understand the complete structure of information theory from Shannon on up, what is the point in demonstrating what you already know to be true: that the signals inside a control system must contain or pass along information about the disturbance, and that it is this information that makes control (and everything else) possible? And most important, **if you have shown that there are no logical errors in reaching a conclusion about real behavior, what is the point in going through the labor of showing by direct experiment that the conclusion actually fits the data? If the data do not agree with the conclusion, there must have been some error or something unaccounted for in the experiment.** That last if-then is the only way I can explain your reaction to difficulties when we actually try out some of your proposals. **In the long information -in-perception debacle, we tried computing the reduction in the uncertainty in The perception, then in its first derivative, then both again with temporal shifts, and in every case the results disagreed with your deductions about what we should find. By rights, this should have brought you up short and caused you to question the very basis on which you built your deductions. But that didn’t occur: you simply abandoned the attempt to make a correct deduction that would fit the data and turned to other subjects.**

If I had been in your shoes, I would have had to backtrack through the logic trying to find the error, and eventually (if no logical mistake could be found that would fix the problem) I would have gone all the way back to the simple starting premises on which the whole logical structure is built: if there are no mistakes in the logic, yet the conclusions do not fit observation, then the only place left to find an error is in the premises. And for me, however painful the decision, the only conclusion I could then reach is that the entire system is built on false-to-reality premises.

When I went through the process of computing reduction in uncertainty about the disturbance due to the perceptual signal, under your tutelage, I noticed a fact, and mentioned it, that seemed significant to me. In the process of computing the conditional probabilities, I noticed that I would get the same conditional probabilities no matter in what order I did the sampling of the disturbance waveform. So in principle there was an infinity of different waveforms that would allow me to compute the same quantity of information in the perception. This made it very hard for me to see how the outcome could be an output waveform based on the “information” that was arranged in the same sequence as the elements of the disturbance waveform, which of course is necessary if the effect of the disturbance is to be canceled.

Your reply was brief and dismissive: you just compute the conditional probabilities on pairs of successive values of the waveform, and get the probabilities of the first derivatives. But after thinking that over, I realized that the same problem still existed: one could rearrange the pairs and get the same conditional probabilities. So how could the information passed in the perceptual signal possibly be responsible for producing the RIGHT output waveform?

When I mentioned this (I am pretty sure I mentioned it), there was no reply that I recall. The failure to get the right results when we used the first derivatives as elements, even time-shifted, reinforced my doubts about the process, but not being an expert in information theory I did not feel competent to ferret out the cause of the problem. I now realize that you did not search for the cause of the problem by backtracking through information theory. You just gave up on it. This did not solve the problem, but it left the intellectual structure of information theory in your head undisturbed. If PCT is correct, we can use this phenomenon to guess at the nature of the variable you were -- and are -- controlling. I remember getting a frantic phone call from Chris Love shortly after the start of the Little Baby project. He had tried to set up a big complex hierarchy of control systems in which, per the boss’s suggestion, the perceptual function was an S-shaped curve followed by an integrator. The reason he called was that he hadn’t been able to get even a single elementary control system to work. I tried to explain to him that a control system organized that way would be trying to control a variable that was the inverse function of the proposed form, namely a nonlinear first derivative that went to infinity at zero and maximum perceptual signal. He was not then knowledgeable about control theory, so I just suggested that he move the integrator to the output function, and preferably make the input function linear. He tried that, and got a working control system for the first time, several months into the project. I felt very sorry for Chris, because he had to try to make the suggested model work, and it could not work.

On other occasions, I have pointed out to you a shortcoming of the perceptron approach, in that it doesn’t yield perceptual signals which are continuous representations of controlled variables. The nonlinearities and other properties limit the output to a yes-no signal, which is good only for discrete control. However, in the fairly recent past, I noticed that you were still referring to the S-shaped input function with an integrator as part of the model. Chris’ problems do not seem to have shaken your faith one bit. Or perhaps they have simply led you to abandon that problem, and go to modeling discrete systems. Obviously it has not led you to re-examine the premises behind the perceptron approach.

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**I think that in deciding to be an abstract theoretician, you have simply cut off your higher level systems from lower-level perceptions, operating the higher-level systems in the imagination mode. And I think that this is a mistake. If you don’t continually check your higher-level models against experiences by interacting with the outside world at the lowest levels, you run the risk of creating a systematic delusion about the nature of the world; one that is internally consistent, but which is not consistent with what your senses could tell you if you consulted them. Abstract thought alone is simply not a reliable way to learn about nature.**

**This is why I am so adamant about demonstrations and experiments.** You have to close the loop through the external environment if you’re to achieve real control. No matter how self-evident or obvious or logically necessary a conclusion may seem, it is still necessary to find a way to test it by interacting with the world. And when you do such tests, it is necessary to pay attention to the outcome, because if the outcomes don’t agree with the logic, it says that something is wrong with the logic or with the premises on which it’s founded. **No matter how convinced you are that you have the right idea, nature is perfectly capable of contradicting you.**

**And this says something else, too. It says that there is really very little point in building up big deductive structures on premises that have not been experimentally demonstrated.** Your cross-connection ideas about category perception may prove to be quite right, but you have no way to verify that such cross connections exist or work in the ways you assume they work. Technology has simply not reached the stage where we can do this in a living working brain. Perhaps it would be possible to do experiments to check, at least, the conclusions, to see if people actually work in the way that your hypothetical model works. But unless you can also check the premises, you are on very uncertain ground. For any circuit that accomplishes a given result, there are a dozen different ones that would do the same thing. There will always be uncertainties in our models, but why deliberately make them as large as possible?

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**I have no illusions about changing your style to correct what I see as mistakes. What you make of what I say is in your hands alone.** But if you want to understand where our frictions come from, you have to know how I perceive the way you work, and how limited it looks to me. **You have to understand that even where you think you see agreement, you may be considering only a narrow range of meanings of what I or others say, meanings that fit your world-view but that may only represent one point of intersection of trajectories that are headed in different directions. And you have to realize that you often read hastily, making assumptions that a more careful reading would quickly set straight and then leaping ahead to draw unwarranted conclusions -- largely, seeing agreement where there is actually no agreement, or only a very partial agreement.** This is another penalty for working in the imagination mode. You are far from the only person to work this way, of course. Obviously, I have considered only YOUR problems, not my own. I am sure that all of this looks quite different to you. If you want to turn the tables, you have every right.

Bill

-- Richard S. Marken

“Perfection is achieved not when you have nothing more to add, but when you
have nothing left to take away.”

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