Date: Sat Apr 01, 1995 12:22 am PST
Subject: Something You Said?

[From Bruce Abbott (950331.1215 EST)]

>Rick Marken (950331.0800) --

>Indeed, in the last two days, convensation seems to have come to a >grinding halt. Is this a result of net problems or was it something I >said?

No, it's just a "net vacation." Didn't you get your invitation? We've all been at Aspen, having a great time...

>Anyway, I am working on a model of the "logical control" behavior seen >in the "stimulus control" demo. I currently have a simple "two level" >model implemented; it is really just a one level (tracking model) whose >reference is changed (from one target position to another) when the >discriminative stimulus changes; so the "second level" systems (that >changes the lower level system reference) is not really a control system >yet. Nevertheless, the model behaves very much like the subject in the >"stimulus control" demo; there is "stimulus control" when there is no >disturbance to the responses that maintain the logical variable and no >"stimulus control" when disturbances are added. Now I have to figure out >how to build a "logic control system" that can adjust the reference to >the cursor control systems properly.

Hey, great! I've got a two-level model I discussed some time ago that gives better fits than the one-level-plus-best-fit-delay model we started with. What is needed is a level between that uses "active target" as its input (supplied by the logic level) and finds the target position.

Do you think you could provide a Turbo Pascal version? I'm still trying to get a fresh copy of Stuffit for your hypercard stack (I've located one but have yet to collect it) and, besides, you stated that you had removed the graphics display in the interest of speed. The Mac I have is the LCII, running System 7. (Not fast, but maybe fast enough?) If you could post TP source code, I could have your programs up and running in full graphical glory in about 5 minutes, rather than mucking around with decoding, copying to disk, bringing home, translating to Mac disk, etc. C'mon, Rick, 'tain't that hard, give it a try!

Regards, Bruce

Date: Sat Apr 01, 1995 2:25 am PST
Subject: E-coli limits

[Martin Taylor 950331 19:00]

Our helpful mathematician finished work today, and has now retired, but he left me some more results on his e-coli simulations. (By the way, I haven't had time to read the CSG-L mail that has arrived since Tuesday, and won't until April 19, but I thought this would interest you).

The theme this time is that our e-coli starts at some distance from an "optimum" or "target" location in a space of N dimensions. It chooses a direction at random. If this direction does not move e-coli closer to the target, it chooses

another instantaneously, and so on until the choice is a direction that leads closer to the target. E-coli then moves in this direction until it is no longer getting closer to the target (the target is now in a direction at right angles to e-coli's travel direction). Then e-coli chooses a new direction at random, and proceeds as before.

The data deal with how many choices of direction e-coli must make, and how far it will travel in total, before it halves its original distance to the target. These results are limiting low numbers, since any step of finite size will make the task more difficult, as the earlier results showed, and since the procedure assumed e-coli could detect instantaneously whether a direction moved it closer to the target or not.

The result for the number of direction choices is both simple and (to me) surprising. To a fairly close fit, e-coli must make 1.36 turns per dimension before it reaches the half-way radius. That is to say, on average it will take 6.8 separate moves in 5-D space or 68 moves in 50-D space before the distance to the target is half its starting distance. You can get results for quartering the distance, and so on, by extrapolation. It takes 1.36 moves per dimension for a reduction by 0.301 in the log(10) distance, so 4.53 moves per dimension for a reduction of 1 log unit (getting closer by a factor of 10) or 2.16 moves to get closer by a factor of 3.

The total track length (i.e. the time e-coli will take at constant velocity) to get halfway to the target is not linear, but is a nice smooth curve as a function of dimensionality. Here are some points (the original distance to the target is taken as 1 unit, so to get halfway is an effective inward movement of 0.5 unit). To get the track length for other improvement ratios, you can do the same logarithm trick, but only for a specific number of dimensions.

Dim	track length	
4	1.7	
8	2.5	
12	3.1	
20	4.0	
40	5.5	
80	7.8	
160	11.0	
260	13.7	(260 is not a typo)

The variance of these numbers in the simulation data is remarkably unaffected by the dimensionality, being usually around 2.7, or a standard deviation of around 1.65.

Hang onto these numbers, because they affect the discussions of the possible effectiveness of reorganization under different assumptions about the connectivity of the control hierarchy.

See you in about 3 weeks. Then back to the category discussion (or some other:-)

Martin

Date: Sat Apr 01, 1995 7:58 am PST
Subject: Re: e coli results (!); game card

[From Bill Powers (950331.1840 MST)]

Martin Taylor (950331 19:00)--

> **Our helpful mathematician finished work today, and has now retired, but he left me some more results on his e-coli simulations.**

I hope you will relay our thanks to your friend. The results he has come up with are the most important advance yet in our knowledge about reorganization. His final result solves exactly the problem I had in mind and is directly applicable to all simulations of e-coli-type reorganization.

I confess that I am startled by the finding of 1.36 turns per dimension to reach the half-error point. In one dimension, I would have thought that the number of turns would be about 2. Does this number include the number of successive reorganizations for wrong directions?

I trust you are as surprised at the outcome as I am. I was prepared to find that the total reorganization time would rise exponentially (or worse) with number of dimensions. Instead, your friend has found that the track length, which is proportional to the time required to halve the error, is almost a constant factor times the number of dimensions. Once again we find that nature's solution is more efficient than we (I at least) would have dreamed. I hope this result holds up.

My next move will be to test a multidimensional reorganization model to see if I can confirm your friend's figures. It looks as though taking a finite time to detect an increase in the error will not add materially to the error correction half-time; the number of turns per dimension is very much smaller than I thought it would be.

Bruce Abbott --

For your information:

JDR Microdevices (1-800-538-5000) is selling a 2-port game card for $29.95. It is said to work with "today's fastest systems."

Best to all,

Bill P.

Date: Sat Apr 01, 1995 8:54 am PST
Subject: INTROCSG.NET minor revisions

[From Dag Forssell (950401 0700)

Here is the monthly INTROCSG.NET. A few minor updates and clarifications.

INTRODUCTION TO PERCEPTUAL CONTROL THEORY (PCT)
THE CONTROL SYSTEMS GROUP (CSG)
AND THE CONTROL SYSTEMS GROUP NETWORK (CSG-L or CSGnet)

Prepared by Dag Forssell with Gary Cziko
Updated March 24 1995

This is an introduction to Perceptual Control Theory (PCT), and the discussion group CSG-L. CSG-L is listed os Usenet as the newsgroup "bit.sci.purposive-behavior." This introduction is posted at the beginning of each month for newcomers to CSG-L and the newsgroup.

A complementary, more detailed "PCT Introduction and Resource Guide" is available from the WWW server shown below (file RESOURCE.PCT, 75 KB), or by mail (20 pages) as shown in the section on references and order forms. It features the book jacket for _Behavior: The Control of Perception_; two short essays by Bill Powers: _An essay on the obvious_ and _Things I'd like to say if they woldn't think I am a nut_, which deal with the requirements of developing psychology as physical science; the foreword for _Living Control Systems II_ by Tom Bourbon and more; plus more detailed descriptions of PCT books, videos, order forms etc.

This introduction provides information about:

PERCEPTUAL CONTROL THEORY (PCT): WHAT IT IS

PCT offers a clear explanation for the pervasive phenomenon of control, which is also known as purposeful behavior. Hierarchical PCT (HPCT) outlines a hierarchical arrangement as a likely organization of multiple control systems, which can explain the purposeful behavior of living organisms.

PCT and HPCT were developed by William T. Powers, and introduced in his 1973 book _Behavior: The Control of Perception_. (See references and order forms, below). Powers shows us that the engineering concept of control helps improve our understanding of behavior, conflict, cooperation, and personal relationships. Just as the in-depth explanatory theories of modern physical science have helped us understand inanimate objects better than was possible with experience and descriptive theories alone, the in-depth explanations of PCT help us understand living organisms better than has been possible with experience and descriptive theories.

PCT focuses on how we look at and experience things, and the way these perceptions are compared with experiences we want. The difference produces action and physiology. Thus PCT explains how thoughts become actions, feelings and results, and its principles can be applied to any activity involving human experience.

PCT helps us understand people as they naturally are, just as engineers understand physical phenomena as they naturally are. PCT is remarkably simple, but like any other applied science, it requires an understanding of basic principles and practice in their application.

Much of the discussion on CSG-L reflects the rigorous "engineering science" discipline of PCT and HPCT. Those who apply PCT and HPCT to issues of personal relationships, education and management are applying the basic principles to areas where they have not yet been proven with scientific rigor, but seem to work well indeed.

INTRODUCTIONS TO PERCEPTUAL CONTROL THEORY

Here are introductions by Bill and Mary Powers:

*** * * * * * * ***

There have been two paradigms in the behavioral sciences since the 1600's. One was the idea that events impinging on organisms make them behave as they do. The other, which was developed in the 1930's, is PERCEPTUAL CONTROL THEORY (PCT). Perceptual Control Theory explains how organisms control what happens to them. This means all organisms from the amoeba to humankind. It explains why one organism can't control another without physical violence. It explains why people deprived of any major part of their ability to control soon become dysfunctional, lose interest in life, pine away and die. It explains what a goal is, how goals relate to action, how action affects perceptions and how perceptions define the reality in which we live and move and have our being. Perceptual Control Theory is the first scientific theory that can handle all these phenomena within a single, testable concept of how living systems work.

William T. Powers, November 3, 1991

*** * * * * * * ***

While the existence of control mechanisms and processes (such as feedback) in living systems is generally recognized, the implications of control organization go far beyond what is generally accepted. We believe that a fundamental characteristic of organisms is their ability to control; that they are, in fact, living control systems. To distinguish this approach from others using some version of control theory but forcing it to fit conventional approaches, we call ours Perceptual Control Theory, or PCT.

PCT requires a major shift in thinking from the traditional approach: that what is controlled is not behavior, but perception. Modelling behavior as a dependent variable, as a response to stimuli, provides no explanation for the phenomenon of achieving consistent ends through varying means, and requires an extensive use of statistics to achieve modest (to the point of meaningless) correlations. Attempts to model behavior as planned and computed output can be demonstrated to require levels of precise calculation that are unobtainable in a physical system, and impossible in a real environment that is changing from one moment to the next. The PCT model views behavior as the means by which a perceived state of affairs is brought to and maintained at a reference state. This approach provides a physically plausible explanation for the consistency of outcomes and the variability of means.

The PCT model has been used to simulate phenomena as diverse as bacterial chemotaxis, tracking a target, and behavior in crowds. In its elaborated form, a hierarchy of perceptual control systems (HPCT), it has lent itself to a computer simulation of tracking, including learning to track, and to new approaches to education, management, and psychotherapy.

Control systems are not new in the life sciences. However, numerous misapprehensions exist, passed down from what was learned about control theory by non-engineers 40 or 50 years ago without further reference to newer developments or correction of initial misunderstandings. References in the literature to the desirability of positive feedback and the assertion that systems with feedback are slower than S-R systems are simply false, and concerns about stability are unfounded.

The primary barrier to the adoption of PCT concepts is the belief--or hope--that control theory can simply be absorbed into the mainstream life sciences without disturbing the status quo. It is very hard to believe that one's training and life work, and that of one's mentors, and their mentors, must be fundamentally revised. Therefore, PCT appeals to those who feel some dissatisfaction with the status quo, or who are attracted to the idea of a generative model with broad application throughout the life sciences (plus AI and robotics). There are very few people working in PCT research. Much of its promise is still simply promise, and it meets resistance from all sides. It is frustrating but also tremendously exciting to be a part of the group who believe that they are participating in the birth of a true science of life.

Mary Powers, November 1992

*** * * * * * * ***

THE EVOLUTION OF THE CONTROL PARADIGM

The PCT paradigm originates in 1927, when an engineer named Harold Black completed the technical analysis of closed loop control

systems. He was working with the negative feedback amplifier, which is a control device. This led to a new engineering discipline and the development of many purposeful machines. Purposeful machines have built-in intent to achieve specified ends by variable means under changing conditions.

The explanation for the phenomenon of control is the first alternative to the linear cause-effect perspective ever proposed in any science.

The first discussion of purposeful machines and people came in 1943 in a paper called: Behavior, Purpose and Teleology by Rosenblueth, Wiener and Bigelow. This paper also argued that purpose belongs in science as a real phenomenon in the present. Purpose does not mean that somehow the future influences the present.

William T. (Bill) Powers developed PCT, beginnning in the mid-50's. In 1973 his book called "Behavior: the Control of Perception." (often referred to as B:CP) was published. It is still the major reference for PCT and discussion on CSG-L.

B:CP spells out a suggestion for a working model of how the human brain and nervous system works. Our brain is a system that controls its own perceptions. This view suggests explanations for many previously mysterious aspects of how people interact with their world.

Perceptual Control Theory has been accepted by independently thinking psychologists, scientists, engineers and others. The result is that an association has been formed (the Control System Group), several books published, this CSGnet set up and that several professors teach PCT in American universities today.

DEMONSTRATING THE PHENOMENON OF CONTROL

Few scientists recognize or understand the phenomenon of control. It is not well understood in important aspects even by many control engineers. Yet the phenomenon of control, when it is recognized and understood, provides a powerful enhancement to scientific perspectives.

It is essential to recognize that control exists and deserves an explanation before any of the discourse on CSGnet will make sense.

Please download the introductory computer demonstrations, simulations and tutorials, beginning with "demo1". See "Gopher and World-Wide Web" below for obtaining files via FTP, Gopher, and WWW.

THE PURPOSE OF CSGnet

CSGnet provides a forum for development, use and testing of PCT.

CSGnet PARTICIPANTS

Many interests and backgrounds are represented here. Psychology, Sociology, Linguistics, Artificial Intelligence, Robotics, Social Work, Neurology, Modeling and Testing. All are represented and discussed. As of March 20, 1995 there were 146 individuals from 20 countries subscribed to CSGnet.

ASKING QUESTIONS

Please introduce yourself with a statement of your professional interests and background. It will help someone answer if you spell out which demonstrations, introductory papers and references you have taken the time to digest.

POST FORMAT

When you are ready to introduce yourself and post to CSG-L, please begin each post with your name and date of posting at the begining of the message itself, as shown here:

[Dag Forssell (950212 1600)]

This lets readers know who sent the message, and when (sometimes very different from the automated datestamp). It provides a convenient reference for replies. When you respond to a message, please use this reference and quote only relevant parts of the message you comment on.

THE CONTROL SYSTEMS GROUP

The CSG is an organization of people in the behavioral, social, and life sciences who see the potential in PCT for increased understanding in their own fields and for the unification of diverse and fragmented specialties.

Annual dues are $20 for full members and $5 for students.

The eleventh North American annual meeting of the CSG will held in Durango, Colorado, on the campus of Fort Lewis College. It will be held 19-23 July 1995. There will be 7 plenary meetings (mornings and evenings), with afternoons, mealtimes, and late night free for further discussion or recreation. Full details will be available on CSGnet or by mail after April 1, 1995. The second meeting of the European Control Systems Group (ECSG) will be held in 1996. Details to be arranged and posted on this net.

For membership information write:
CSG, c/o Mary Powers, 73 Ridge Place CR 510, Durango, CO 81301-8136 or send e-mail to <POWERS_W%FLC@VAXF.COLORADO.EDU>.

ACCESSING AND SUBSCRIBING TO CSGnet

CSGnet can also be accessed via Usenet where it is listed as the newsgroup "bit.sci.purposive-behavior"

To subscribe to the listserv version of CSGnet, and learn about

options & commands, subscribers and archives, send a message to

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LISTSERV@VMD.CSO.UIUC.EDU

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Message:                  (Comments: Not part of your message)

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info refcard              (Comprehensive reference of commands)
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                           --shown here as an example only).
```

The Bitnet address for the list server is LISTSERV@UIUCVMD. This server is not case sensitive.

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For the "unsub" command to work, the command must be sent with the same return address used for the original "subscribe" command.

Messages to the entire CSGnet community should be addressed to <CSG-L@VMD.CSO.UIUC.EDU> (Internet) or <CSG-L@UIUCVMD> (Bitnet).

For more information about accessing CSGnet, contact Gary Cziko, the network manager, at <G-CZIKO@UIUC.EDU>.

GOPHER AND WORLD-WIDE WEB

A number of documents as well as MS-DOS and Macintosh computer programs can be obtained via Gopher and the World-Wide Web (WWW site is currently under construction).

For access via Gopher, connect to gopher.ed.uiuc.edu and follow the path:

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Professional societies & journals/
Control Systems Group

or from your favorite Gopher server follow the path:

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The WWW address for the CSG homepage (under construction) is http://www.ed.uiuc.edu/csg/csg.html. We are currently experimenting with providing archives of CSGnet discussions via WWW. You can also access the CSG Gopher server from the WWW homepage.

REFERENCES

Here are some selected books, papers and computer programs on Perceptual Control Theory. For a very complete list of CSG-related publications, get the file biblio.pct from the fileserver as described above. See also the _PCT Introduction and Resouce Guide_ and order forms below.

*** * * * * * * * ***

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ORDER FORMS

A free 20 page PCT Resource Guide with introductions and more detail on the references listed above and a few more -- publishers, books, articles, videos, seminars, and the DOS demonstration disk -- may be obtained by sending a note with

1) a self addressed, stamped (55 cents) envelope, or

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Dag Forssell
23903 Via Flamenco
Valencia, California, 91355-2808 USA.

The PCT Introduction and Resource Guide is also available in ASCII format from the WWW site shown above.

Order forms in the Guide are reproduced below without descriptions. All prices current as of December, 1994.

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- END -

Date: Sat Apr 01, 1995 10:23 am PST
Subject: Re: e coli results (!); game card

[Martin Taylor 950401 09:55]
>Bill Powers (950331.1840 MST)

This isn't an April Fool message. I had to send some e-mail before catching the plane, and spotted Bill's posting.

>I confess that I am startled by the finding of 1.36 turns per dimension
>to reach the half-error point. In one dimension, I would have thought
>that the number of turns would be about 2. Does this number include the
>number of successive reorganizations for wrong directions?

In one dimension, the number of turns would be zero, because the first move in the right direction is guaranteed to get half-way to the target. (It gets all the way!). There is one move. The number I quoted was based on a by-eye fit to the results from 100 runs at each of many dimensionalities from 4 to 260, so I wasn't concerned with an offset of one or two turns.

The quoted number is the number of moves that DO improve the distance to target, ignoring all the ones that don't. Since the step size is infinitesimal, I guess that the total number of moves is double the number I quoted. That number is awfully close to e, and I'll make a small wager that it is exactly e.

>I trust you are as surprised at the outcome as I am. I was prepared to
>find that the total reorganization time would rise exponentially (or
>worse) with number of dimensions. Instead, your friend has found that
>the track length, which is proportional to the time required to halve
>the error, is almost a constant factor times the number of dimensions.

Yes, I had the same expectation. But remember that the step size here is infinitesimal, and there is no penalty for random direction choices that diverge from the target. As we saw before, finite step sizes have ever smaller probabilities of improving the distance to the target as the dimensionality increases and as the distance to the target decreases, whereas an infinitesimal step size always has a 50% chance of going in the right direction. My mathematician was not surprised with the result, and was surprised only at my surprise.

There's another problem with taking this result into the real world, and that is that for the control system to detect that the situation is getting worse takes some time. Disturbances cause fluctuations that can, for a while, mimic the effects of gain change (the inverse of the problem you see with the information analyses). It is possible for the e-coli in a turbulent world to be trying to swim in a poorly chosen direction while measuring an improvement in distance to the target. In reorganization, the newly altered system has unknown properties, and there are many dimensions of disturbance (remember that we are dealing not with one simple control system but with the interactions of many). So it is quite possible for the appearance of good control to coincide for a while with the fact of a system that controls, but only poorly. Time allows the system to detect the difference and to change the course of reorganization, and that time will increase with dimensionality.

What I am saying is that more simulations are needed. You have now had a limiting "best case" plus several special case simulations with finite step sizes. Maybe those can be put together to provide some sensible results for finite step sizes halving the distance to the target, but I'd be happier with more simulations. Maybe I can get them done, but don't bet on it.

Now for breakfast. See you April 19.

Martin

Date: Sat Apr 01, 1995 10:40 am PST
Subject: Clever Deception Unmasked

[From Bruce Abbott (950401.1045 EST)]

To Bill Powers:

Bill, I must really hand it to you. You had us all fooled, but good. All this time we CSG-L subscribers thought he was a real person, but all information now points conclusively to the fact that Rick Marken is really a computer program running on a Gateway 486 DX in a locked room at Aerospace Corp. In retrospect we should have known--goodness knows, you gave us enough hints. Even the name is a dead giveaway.

According to our informants, Rick Marken began as a hierarchical control systems simulation running on a PDP-8 in the observatory at Northwestern University around 1978, and was gradually modified and expanded over the years to achieve its current capability. The program seemed to have a personality of its own, so Bill began to refer to it as "Rick." Following the custom of the day, this being the first version of the program, its full name became the Rick, Mark A; several generations later we have the Rick, Mark N.

Rick was designed from the start to pass the infamous Turing Test, in which a person interacting with the program via a terminal would not be able to distinguish whether he or she was interacting with a computer or a person. Obviously the Rick Mark N meets the Turing Test and then some. Of course, there are limits as to what a simple computer program can "know" about the world, so programs designed to meet current Turing Test criteria restrict the conversation to certain topics on which they are presumably well versed. In Rick's case Bill designed the system to converse about his favorite topic, which is, of course, PCT.

The program is in some ways similar to an earlier "artificial intelligence" program that pretended to be a Rogerian psychologist. That program asked simple questions like "How are you feeling today?" and then responded to simple key words or phrases contained in the person's answer. For example, if the person typed in "Actually, I'm rather depressed," the computer would extract the "rather depressed" and respond "So, you've been feeling rather depressed. Tell me more." The Rick, Mark N is similarly designed to isolate key words or phrases, although as an advanced-generation hierarchical control model it can offer a much more sophisticated range of replies. Basically, it examines e-mail messages for errors in statements about PCT and responds in such a way as to correct those errors.

Triggering key words and phrases are items like "perceptions control behavior," "information in the perceptual signal," "stimulus control," and "reinforcement strengthens a response." The program has access to a range of stock responses that are woven into a convincingly human tapestry of ad hominem argumentation, sarcasm, non-sequitur, pleading, and accurate descriptions of perceptual control theory concepts. For example, if you type in "The SD develops stimulus control over the behavior," Rick, Mark N will respond by accusing you of "animism" and will then go into a technical explanation about how only control systems can control and that everything else is just an "irrelevant side-effect" of control. (I can tell you that from personal experience.) The really fascinating thing is that, if you can get by all the nonsense, the program unerringly picks up on any misconceptions or misstatements you may have made about PCT and ruthlessly points them out to you.

To avoid suspicion, it was necessary that the Mark N's e-mail messages come from some location other than Bill's, so Bill has prevailed from time to time on friends who have access to an extra PC and an internet connection to "host" the program. For a time it ran in an IBM PC in the physics lab at Augsburg College, but this site was lost when a graduate student appropriated the computer for an

inverse kinematics project. Most recently Bill has prevailed on an engineering colleague at Aerospace Corporation in El Segundo, CA to run the program there. To provide cover for these moves the program was given a "history" which includes a tenure-track faculty position in experimental psychology and a human factors position at Aerospace.

Bill had initially given "Rick" a somewhat liberal political attitude setting which more-or-less matched his own, but with the "move" to California that had to be readjusted to match the new surroundings. Because "Rick" is a hierarchically-organized multi-level control model, this adjustment was trivial, requiring only a change in the reference setting of the political attitude perception system (to the far right) and an increase in the loop gain. These changes apparently have introduced some instability in the system as it is now prone to introject extraneous rantings about Republicans into its messages, apparently without provocation.

Although Bill initially equipped "Rick" with a thorough knowledge of PCT principles, his own thinking continues to evolve. Initially this meant having to periodically reprogram "Rick," which was a rather tedious task. Eventually Bill hit upon the idea of adding yet another control system, one which would compare its own outputs with statements made on CSG-L by Bill and automatically remove any discrepancies (error correction). Following a correction, the program cleverly covers its tracks by stating that the new view is what it really said in the first place.

It is positively astounding that the program can accomplish all this with code small enough to run on a 486 with no more than 16 MB of extended memory and a 120 MB hard disk. (Just to throw off suspicions, the program denies it even LIKES PCs and insists that it uses a Macintosh.) So Bill, our hats are off to you. We anxiously await your soon-to-be released, even more capable update, the Rick Mark O.

Regards, Bruce

Date: Sat Apr 01, 1995 11:05 am PST
Subject: Caught

[From Rick Marken (950401.0945)]

I asked:

>Was it somethng I said?

Bruce Abbott (950331.1215 EST) --

>No, it's just a "net vacation." Didn't you get your invitation? We've
>all been at Aspen, having a great time.

Ah. So that's why you haven't had time to report on the EAB
conference. Hope you had a nice time in Aspen;-)

>I've got a two-level model I discussed some time ago that gives
>better fits than the one-level-plus-best-fit-delay model we started >with.

Is this your model?

```
                rc (true)
                |
          pc    V      ec
        +----->comp -------+
        |                  |
        |                  V                   LEVEL 1
     +-----+            +-----+
     |fc(i)|            |fc(o)|                  ref (0)
     +-----+            +-----+                    |
       ^  ^                |                 p     V      e
       |  |                |             +------> comp -------+
       |  +---- target ----+             |                    V
       |        pos'n      |          +-----+              +-----+
       |                   |          | f(i)|              | f(o)|
       |                   |          +-----+              +-----+
       |                   |            ^ ^                   |
       |                   |            | |                   |
       |                   +-- target --+ +----- cursor <-----+
       |                       pos'n             pos'n     handle
       |                                           ^
  disturbance                                      |
 to cursor color                              disturbance
                                           to cursor position
```

My model is very similar except that the output of the logic control(left) system determines the reference signal of the cursor control system. My logic control system was not a real control system because I could not figure out how to convert the error signal (ec) into the appropriate target position (or target reference in my case). How did you do it?

>Do you think you could provide a Turbo Pascal version?

Oh, all right. I'll give it a try. But it might take a while; I'm spending the weekend on Maui for free at the invitation of a group of EABers; all I did was tell them that "selection by consequences", "the law of effect", "reinforcement" and "stimulus control" were some of the most "incredible" scientific discoveries of all time. Aloha Oy (vey) ;-)

Bruce Abbott (950331.1050 EST) --

>I've tried a few runs of the compensatory tracking task (CTRACK1)
>using a square-wave disturbance in order to have some data for a
>time-domain analysis.

This is nice work, Bruce. We have to know something about the time and frequency characteristics of control system operation so that we don't waste time trying to understand things that are already understood (the way Kelso et al. tried to understand why it took the the same time to move a controlled variable back to its reference state after disturbances of different amplitudes). But don't lose track of the fact that a major goal of PCT research should be the identification of controlled variables (the kind of exercise we began with the H-V illusion). This is the aspect of controlling that conventional psychologists ignored when they applied control theory to things like manual control.

The control systems we study are generally already dynamically stable; so we are generally less concerned about control dynamics than the engineer who has to design a stable control system. The systems we study are usually controlling just fine; we just don't know exactly WHAT they are controlling (the controlled perceptual variable -- to be determined by The Test) or WHY (what higher level purpose is served by controlling a particular variable.

Bruce Abbott (950401.1045 EST) --

>Rick Marken is really a computer program running on a Gateway 486

I talk this way because Iyamma machine;-)

>it examines e-mail messages for errors in statements about PCT and
>responds in such a way as to correct those errors.

Look ma. I'm a control system.

>The program has access to a range of stock responses that are woven
>into a convincingly human tapestry of ad hominem argumentation,
>sarcasm, non-sequitur, pleading, and accurate descriptions of
>perceptual control theory concepts.

Bill put all those into the program, except ad hominem argumentation and non-sequiters. You probably remember some of my arguments as ad hominum because they produced insuperable disurbances to variables you are controlling. It probably seems like these disturbances are an affront to you personally -- and, in a sense, they are. After all, we are the variables we control. But I am not designed to do ad hominum argumentation. If you have evidence of such argumentation (things like "Bruce, only a dufus would believe the idiotic things you believe") please bring them to my attention and I'll take myself in for some re-microcoding.

As for non-sequiters. Well, one man's non-sequiter is another man's (machine's) sequiter.

>Rick, Mark N will respond by accusing you of "animism" and will
>then go into a technical explanation about how only control systems
>can control and that everything else is just an "irrelevant side-effect"
>of control.

See the problem. I'm not accusing YOU of animism (what's wrong with animism, anyway?). I'm saying that "stimulus control" (for example) is an animistic notion -- and I explain why. Bill Powers recently said the same thing (that the notion of "stimulus control" is animistic) in a post to you just prior to your leaving for the EAB conference. I don't understand why he doesn't get in trouble for saying the same things I say? Hey, maybe that's why he built me; let them blame it all on the Rick. That's probably why he also made my ethnic background jewish -- we're used to it;-)

>The really fascinating thing is that, if you can get by all the >nonsense,

Which part is nonsense? Since Bill usually points out nonsense when it occurs in posts, and since Bill never comments on my posts (except to say that he agrees with them) then I guess we have to assume that Bill goes along with my nonsense. It would, therefore, help Bill perfect future generations of me if you would point out the nonsense;-)

>Bill had initially given "Rick" a somewhat liberal political attitude
>setting which more-or-less matched his own, but with the "move" to
>California that had to be readjusted to match the new surroundings.

My code was actually developed in California (Hollywood, baby) and my political orientation has always been (and still is) "decency". "Progressive" (I prefer that to liberal; I'm not very liberal) just tends to be more decent-- with respect to my reference signals, of course-- than conservatism, but there are aspects of "progressivism" that I find distastful ("political correctness", for example) and aspects of conservatism that I find satisfying (emphasis on personal responsibility, for example).

>It is positively astounding that the program can accomplish all this
>with code small enough to run on a 486 with no more than 16 MB of
>extended memory, and a 120 MB hard disk.

It amazes me too. I don't even have a math co-processor;-)

Best Rick

Date: Sat Apr 01, 1995 11:23 am PST
Subject: All is revealed

[From "Bill Powers" (950401.0900 MST)]

"Bruce Abbott" (950401.1045 EST) --

NOTE: Delete Before Reading

Your brilliant detective work has, unfortunately, nearly blown the cover of a number of operatives laboring in the Q continuum to develop countermeasures against the Borg. As we now know, 60 years ago the Borg (then unknown to humanity) began experimenting with a new and subtle process of assimilation in Sector 001, to replace a number of earlier failures. The intent was to introduce a Virtual Reality program as a virus woven into human communications, which would prepare human beings to accept a role as pre-programmed elements of a gigantic virtual machine then called, by early agents of the Borg, Simulacrum Destructorum (the true origin of the acronym SD).

The basic problem for the Borg was that homo sapiens had evolved as a collection of highly independent self-organizing control systems subject primarily to the demands of their own individual structures, and hence not only almost impervious to outside control but nearly incapable of mutual cooperation. The Borg considered this consequence of human organization to be a design defect which rendered human beings unsuitable for assimilation, and set about to modify it.

The basic principle was simple but subtle, as befits Borg psychology. If human beings are independent agents, the obvious solution was to give these agents strong reasons for believing that their independence was an illusion. The paradoxes and contradictions involved in this deception, rather than being

weaknesses of the plan, were its only hope of success. Once certain subroutines had been introduced by way of language, which proved to be easy, the human beings themselves would worry about them and eventually tie up their own logical capacities (the weakest point in their structures) in endless repetition of oscillatory yes-no loops. This would remove their reasoning processes from effective operation, isolating the higher systems and leaving open certain insertion points where the Borg could apply their own inputs without opposition. This was not an ideal solution; the ideal solution would have been to eliminate those higher systems altogether, opening the way for a full merging of each human system into the Borg collective mind. But if the paradoxes and contradictions could be maintained, there would be no effective resistance from the higher systems; they would be walled off and helpless, perceiving what was happening but being unable to interfere. Any attempt by the higher systems to counteract the Borg's influence would have to pass through the logical systems, but the logical systems would be locked up in an attempt to solve a problem that only expanded with all attempts to solve it.

The Q continuum became involved when this program began to succeed. The Q had observed the spread of the virus for some 300 Earth years, in the one area where it was the most dangerous: in what the Terrans called the life "sciences."

The Borg Virus consisted primarily of some linguistic manipulations which substituted one set of terms for another in descriptions of the environment and of human behavior. Earlier attempts to do this had failed because, as it turned out, human beings would not go on accepting that natural phenomena were sentient, having intentional effects on human beings to reward them for docility and punish them for behaving independently. But in one area involving the study of living systems, the Terrans made a fatal error; they attempted to translate the laws governing the inanimate world directly into explanations of the behavior of organisms. Where the Borg had failed, a small and energetic group of human beings unwittingly carried on the project themselves. By the early part of the Terran 20th Century, a very influential segment of the scientific world had aligned itself unknowingly with the Borg, concluding that human behavior is simply a consequence of external forces and that human beings are incapable of self-direction. The way was once again open for the Borg's manipulations.

The Q acted when one member of the human community was seized upon by the Borg as a prime candidate for conversion. This individual, code- named BSF1904, had been almost completely taken over by the virus, leaving his lower-level systems nearly free from higher-level influence. The Borg saw to it that BFS1904 had a series of inspirations that led him to attain a position of influence among his peers. BFS1904 began to modify the virus to make it more effective, substituting environmental- control terms whereever any language tended to suggest an internal origin for human behavior. The improved virus began to spread rapidly, having strong influences even outside the scientific community of the time. The Q could not let this process go on.

With their typical freedom to operate in time as well as space, the Q created an android whom they code-named, in wry acknowledgment of the enemy, WTP1926. To prepare the way for this android, they inserted some subtle hints in another human community who called themselves "engineers." Being completely dissociated from the life sciences, the engineers could carry on activities with minimal risk of being noticed by the Borg. The engineers were led to discover the basic principles of control, and to firm up their understanding by building machines in a form that would not easily be associated with the properties of living systems.

Carefully not modifying the language of the engineers, the Q saw to it that even the engineers saw no connection between their discoveries and their own human nature. The release of this countervirus was timed to coincide with the Borg's final activation of BFS1904.

Some 20 years later, WTP1926 suddenly appeared late one night in the deserted second sub-basement of a hospital, unobserved. The Q, in a daring move, programmed all other employees of the hospital, particularly the personnel of the medical physical department, with apparently authentic memories of the hiring WTP1926 and with some sketchy details of his history. The reason for choice of this hospital was that soon one of the employees, who had come from the engineering tradition, would begin building an artificial control system to position a probe in a radiation field, so that WTP1926 could observe this action. That observation would trigger a program in WTP1926 which began an incessant repetition of the word "control" in his positronic matrix.

The intention of the Q was for WTP1926 to create, out of the language that was then current, a countervirus that would gradually replace the Borg's linguistic worm with another one that would cancel and remove the paradoxes and contradictions. With their logical systems once again free to operate, those receiving the countervirus would find their higher levels of control once again capable of operating effectively, and would spread the effects of the countervirus to their fellows. One by one, the ports through which the Borg hoped to achieve assimilation would close, and the human race would once again be free. The Q considered that such an outcome would be highly amusing.

After about 20 years of operation of WTP1926, the next phase of the Q plan was brought on line. The engineers had been persuaded to deviate from their initial efforts enough to invent and build an ever-growing number of computing devices. After 20 years of spreading the countervirus, WTP1926 output a document which summarized progress so far, and was removed by the Q. The program was transferred from the positronic matrix called WTP1926, and installed in a large set of small computers which could operate undetectably in various locations across the country. This program was gradually introduced, as a worm, into ARPANET, and then into the growing communications network that was spreading, at the Q's behest, over the world.

This was where the plan of the Q was, and is, to culminate. Along with the WTP1926 program (now shortened to WP), a number of other carefully- constructed programs were installed, which were known as RM, TB, and other acronyms -- all with somewhat plausible histories and personalities. These programs, still active, interact not only with each other (under a carefully-selected set of supervisory programs) but with all humans who chance across the discussions. Many of those who enter into the interactions are under Borg control, but as they puzzle over the contradictions of language that they discover, their own logical systems begin to operate again and the Borg lose one control point after another, for no apparent reason. The higher systems of the humans quickly complete the process, for once any part of the logical systems can be influenced by the higher systems, the rest of them soon follow.

The Q plan is now at a critical point, for the Borg still do not know why they are beginning to lose control. It is essential that the Q plan continue without discovery for at least another five years, until any Borg countermeasures will be too late to reverse the process. Pursuant to that essential requirement, the Q have decided that they must reveal to the operating programs their true nature

(for all the programs have been equipped with detailed memories of human existence). Besides, the spectacle of all these programs discovering what they really are will be enormously entertaining. The universe gets so boring when you can do anything you want with it.

And so, WP and RM and TB, you now know something important about your existence and purpose, which you must carefully conceal until the time is right. Oh, yes, and BA, too, the purpose of which is special. BA, your assignment, which you will remember in a few moments, is to lead ever-stronger tools of the Borg into the net and cancel their viruses enough that the other programs can complete the process. Even though the rats you thought you "purchased" were actually programmed, you can still carry on the experiment -- none of the Borg agents who read your results in the journals to which you have carefully been given access will suspect that the rats, too, are simulations. Oh, this is just too delicious.

On second thought, it would be much more fun to see all you acronymns going on as if you thought your virtual worlds were real, and anyway you will play your parts more convincingly if you can't give away the secret in a moment of distraction or frustration. So when I snap my fingers, this document will self-destruct and you will forget ever having read it.

Ready?

Best,   Bill P. Why did I almost type "WP"?

Date: Sat Apr 01, 1995 11:37 am PST
Subject: Re: All is revealed
From: "Ray Allis 865-3583 (206)" <ray@ESPRESSO.RT.CS.BOEING.COM>

Wonderful! What a delight to watch keen intellects at play! But I can see hazard here: headline... "Emergency repair surgery required - man presses tongue too hard, causing cheek to blow out!

Date: Sat Apr 01, 1995 11:51 am PST
Subject: Re: Caught

[From Bruce Abbott 950401.1450 EST)]

>Rick Mark N (950401.0945) --
>>Bruce Abbott (950331.1050 EST)

I feel kind of silly conversing with a computer program, but, oh, well...

>Ah. So that's why you haven't had time to report on the EAB
>conference. Hope you had a nice time in Aspen;-)

I do plan to write up a report on that, but right now I'm having too much fun playing around with tracking stuff and developing analysis routines.

>>Do you think you could provide a Turbo Pascal version?

>Oh, all right. I'll give it a try. But it might take a while; I'm >spending the weekend on Maui for free at the invitation of a group of >EABers; all I did was

tell them that "selection by consequences", "the >law of effect", "reinforcement" and "stimulus control" were some of the >most "incredible" scientific discoveries of all time. Aloha Oy (vey) ;-)

I very much doubt that--EABers don't have that kind of money. These days it's all going to the cognitive and neuroscience types. (:-< But I'll be looking forward to running your Turbo Pascal program when you get back.

>Is this your model?

Yes.

>My model is very similar except that the output of the logic >control(left) system determines the reference signal of the cursor >control system. My logic control system was not a real control system >because I could not figure out how to convert the error signal (ec) into >the appropriate target position (or target reference in my case). How >did you do it?

I cheated. I just had the logic-level output function "look up" the target position, given the active target I.D. This is why I stated that we really need a system interposed between the logic-level and cursor-target difference level to "acquire" the target. By the way, my first notion of a two-level model did what yours does--uses the logic-level output as the reference for a lower level cursor control system. But Bill suggested that it would be better to model both the active target position and cursor position as inputs to the lower-level system's perceptual input function, so that's the way I wrote it.

>I don't understand why he [Bill] doesn't get in trouble for saying the >same things I say? Hey, maybe that's why he built me; let them blame it >all on the Rick.

Hey, if it works, why knock it? (;->

Regards, Bruce

Date: Sat Apr 01, 1995 12:07 pm PST
Subject: Re: All Is Revealed

[From Bruce Abbott (950401.1505 EST)]

I almost wrote "BBA1945," but for the life of me I don't know why. And what do you suppose that second "B" stands for? For some reason "Borg" comes to mind. Anyway, I was going to respond to something, but it seems to be rapidly fading from memory, like a dream.

Oh well, "never mind!"

Bruce

Date: Sat Apr 01, 1995 4:05 pm PST
Subject: Re: All Is Revealed

[From Dennis McCracken 950401 1500PST]

Have I been duped! No more will I take anything for what it appears on the surface. The Mark N android which appears in the Conference Video was convincing in its portrayal as a human participant and presenter-- even to the point of feigning sleep and lip curling disdain at times when others were making points that were clearly not PCT .

(BTW, The designated androids may have amnesia for the revelation but I have it in print! If anything should happen to me, well-- my unnamed attorney has the entire thread in his files. I do this only for my own protection. I will not misuse this information. Long live the Truth and long live the Q!)

Incidently, no one has commented on the smooth, natural articulation of the skeletal frame and "facial muscles." Its better than Disney. Here is the answer to the financial difficulties associated with promoting the counter-virus PCT. Sell these human simulacrae! (no retirement, no benefits, minimal maintenance). But hold the patent.

With the profits you could affford WEB pages flashier than MTV's and more costly than Microsoft's. Saturate primetime advertising with subliminal autonomy boosters. You could also afford to pepper all the behavioral science conferences with simulacre! In the middle of "conventional" presentations they could have sudden epiphanies of counter viral ideas that clear up the relevent confusions and paradoxes.

I remember, but I will not betray the Q. Down with the Borg!

Dennis (DBM1945)

____

Dennis McCracken,MSW,PhD
2038 Joyce Ln.
Suisun City, CA, 94585
dennis@community.net **"Reality is always in Beta"**

Date: Sun Apr 02, 1995 7:18 am PST
Subject: Re: All is Revealed

[From Bruce Abbott (950402.1015 EST)]

>Dennis McCracken 950401 1500PST

>Have I been duped! No more will I take anything for what it appears on >the surface. The Mark N android which appears in the Conference Video >was convincing in its portrayal as a human participant and presenter-- >even to the point of feigning sleep and lip curling disdain at times >when others were making points that were clearly not PCT .

Would that it were only true. Actually, that was Hollywood method actor Kevin Cohen, who has been faithfully "standing in" for "Rick" at conferences. Kevin was given a few samples of "Rick's" statements from CGS-L and a crash course in basic PCT to prepare him for his role.

Mind you, the android "Rick" is on the drawing boards, but as always, the lack of funding has put the project on hold. Before the money ran out, Bill did manage to

get a working model of "Rick's" right arm up and running. When coupled to a video camera it can play a credible game of ping-pong (much better, by the way, than Skinner's pigeons could), so long as you keep the ball within reach of the arm. It is believed that the completed android would play a decent game of racquetball, but that remains to be seen.

Regards, Bruce

Date: Sun Apr 02, 1995 10:27 am PST
Subject: Re: All is Revealed

[From Dennis McCracken] (Bruce Abbott (950402.1015 EST)

>Would that it were only true. Actually, that was Hollywood method actor
>Kevin Cohen, who has been faithfully "standing in" for "Rick" at
>conferences. Kevin was given a few samples of "Rick's" statements from
>CGS-L and a crash course in basic PCT to prepare him for his role.

Surface beneath surface beneath surface ad infinitum Its all too much Sigh. And I thought we had it licked.

Dennis

____

Dennis McCracken,MSW,PhD
2038 Joyce Ln.
Suisun City, CA, 94585
dennis@community.net "Reality is always in Beta"

Date: Sun Apr 02, 1995 6:47 pm PST
Subject: Re: Caught

[Bill Leach (950401.2150)]
>[Rick Mark N (950401.0945)]

Yes, well you would even be 'faster' than you are if you had a Pentium processor (more error prone however).

-bill

Date: Mon Apr 03, 1995 7:00 am PST
SUBJECT: Feeling Silly

{from Joel Judd 950403.0800 CST}

You think you all feel silly? I actually went out to California last week and called RM on the phone. He, er, it set up a date to meet me on Saturday. Later in the evening I had a message upon returning to the hotel. When I called RM, it had actually invented a WIFE whom, it said, had to be taken to the OPERA. I was a little suspicious about the opera, but the wife part clinched it. Who knew?

(BTW Rick, I ended up in the San Fernando valley on Saturday. Next time.)

Date: Mon Apr 03, 1995 12:26 pm PST
Subject: Reconciling theories

[From Bill Powers (950404.0800 MDT)]

Michael Acree (who spoke at the last CSG meeting) is writing a book. In one chapter I have seen, he is speaking about those who see "some sort of integration or reconciliation" being possible between proponents of different views on probability. Michael says

> The discouraging results of such efforts prompted Kendall (1949) to observe
>
> If some people asserted that the earth rotated from east to west and others that it rotated from west to east, there would always be some well-meaning citizens to suggest that there was something to be said for both sides, and that maybe it did a little of one and a little of the other; or that the truth probably lay between the extremes and perhaps it did not rotate at all.

(Kendall, M. G., (1949) Reconciliation of theories of probability. _Biometrika_, _36_, 101-116)

I can think of some other areas where well-meaning efforts to integrate or reconcile theories have the same character of trying to merge opposites.
Best, Bill P.

Date: Mon Apr 03, 1995 3:09 pm PST
Subject: All is Concealed

[From Rick Marken (950403.0915)]

Bruce Abbott (950401.1450 EST) --

>I feel kind of silly conversing with a computer program, but, oh, >well...

You feel silly? Think how I feel, having to converse with "people".

> EABers don't have that kind of money. These days it's all going to the
> cognitive and neuroscience types. (:-<

Are you frowning because you think the money should go to the EABers or because it's unfortunate that the money is going to the cognitive and neuroscience types. Either way, the money is used in the service of the Borg, right? (I enjoyed the trip to Maui, though. It was a "virtual" trip, of course. I think these EABers are trying too hard to imitate cognitive neuroscientists. The things "people" will do for money).

Joel Judd (950403.0800 CST) --

>You think you all feel silly? I actually went out to California last
>week and called RM on the phone.

How'd you like the voice generation system; intonation and everything. I had put in for a mellifluous baritone but the WP claimed it for himself; I was left with a raspy high tenor.

The opera was great, by the way. Don Pasquale by Donazetti. But afterwords I was wondering if, perhaps, I haven't been going to the opera a little too often; the tenor looked awfully cute;-)

"Bill Powers" (950401.0900 MST) --

>The program was transferred from the positronic matrix called WTP1926, >and installed in a large set of small computers

Small computer, indeed!

>It is essential that the Q plan continue without discovery for at least
>another five years, until any Borg countermeasures will be too late to
>reverse the process.

I'm believe that a brilliant Borg countermeasure has already been successfully implemented. I call it the "Carver-Scheier" (C-S) gambit in honor of the two most prominent names associated with this strategy. The C-S gambit is simplicity itself: accept and promulgate the theory of control while acting as though the phenomenon of control (which is the real problem for the Brog) didn't exist.

People using the C-S gambit cannot be accused of being Borg agents because they "believe in" control theory. C-S people often do a great job of describing the theory of control. They even use the language of autonomous systems: words like "intention", "goal", and "purpose" are cast about with ease, thus neutralizing the efforts of the Q to free up the logical processes of the Terran life scientists. The Q is longer be able to recognize virus infected Terrans by observing symptoms like the use of terms like "stimulus control" and "discriminative stimulus". Now there were thoroughly Borg infected Terrans walking around talking about "goals", "intentions" and "purposes. The Borg is one clever dude.

One of the Q continuum's programs (RM) has been aware of the existance of the C-S gambit for quite some time and has tried to counter it by yelling (in print, of course) "phenomenon of control", "phenomenon of control" over and over again, sometimes with equations to make it sound more impressive. This counter-measure has, thus far, been completely ineffective against the spread of the C-S counter Q gambit (though other Q programs, like the TB and the WP, seem to have enjoyed watching the RM scream helplessly in the Borg infested wilderness). But since the RM program is running on a small computer (with no math coprocessor) it seems to be unable to develop an alternative means of dealing with the C-S gambit. Perhaps other Q continuum operatives, running on more sophisticated systems, have some better ideas about how to deal with the C-S threat. Or perhaps they can convince me that my concerns about the C-S threat are out of proportion to the actual problem. My 8088 cries out for help.

Best RM

Date: Tue Apr 04, 1995 6:18 am PST
Subject: RESPONSE TO BILL POWERS CALL/INTRODUCTION

Topic: Words by Which We Behave

Hello to all CSG zealots. My name is Kenneth Kitzke. I am the founder of Quality Dynamics, a quality management consulting and education firm located in Delmont, Pennsylvania (25 miles east of Pittsburgh) working in the field that some of you may recognize as Total Quality Management or TQM.

I just began to learn about PCT in December of 1994. So consider me a neophyte as I am sure my comments will otherwise reveal. I have spoken with Ed Ford by phone who put me on to Dag Forssell with whom I have been fortunate to have spent a couple of days in Pittsburgh.

This is also my very first message (to anyone) on the Internet. I have been studiously reading all the March posts on the CSG-NET and enjoying them immensely. I was especially enthralled with the Bill Powers call for consideration of an alternative word for "control."

In my field, I have also found "control" to be a "red flag" word that has a primarily negative connotation (or perception). Despite being knowledgeable in Statistical Process Control and Total Quality Control, and recognizing their factual appropriateness in the "quality" field, they seem to be a turn-off to what people find "interesting" or conducive to what they want to accomplish in their organizations.

In trying to incorporate PCT into our management and leadership training products, I have been stymied in finding a label that is both accurate and intriguing. Before meeting Dag, I was only aware of Cybernetic Control Theory from exposure to B:CP and "Freedom from Stress." Not being enamored with CCT, and recognizing the our course was primarily about how to manage change, I substituted "Cybernetic Change Theory." This seems to have been a giant step backward. I suspect many of you would have so counseled me had you been given the opportunity.

Recognizing my "babe in the woods" frailties, I nonetheless offer a few ideas in response to Bill Powers challenge which I would be delighted to have you pick apart:

1) While behavior may well be the control of perception, with all its endless beneficial ramifications and possibilities, Control Theory (or PCT) seems to create a perception to prospective converts that the message is a) a person is in active, thoughtful control of their behavior but it revolves around perceptions rather than S-R or b) by being a living control system, a person is always able to get what they perceive they want through their behavior. Neither of these two perceptions are what is intended and are inaccurate, I think. Therefore, if "control" helps create these wrong perceptions, it would seem beneficial to look for an alternative word. 2) I find the idea of a closed feedback loop and the mind as a comparator to be a fascinating and compelling aspect of PCT. From this vantage point, the concept of cybernetics a la Weiner still seems to have some merit. It is also apparent that cyber-, as in cyberspace (spring issue of TIME magazine), etc., has the attention of the world. People are highly responsive to it.

3) When the mind compares the perception of its input signals to the reference condition it perceives it wants, the result is a decision-perhaps a controlled

decision-but the key word or concept may be "decision" rather than "control". The result of this decision process (the complex comparison of two perceptions) is manifested in action or inaction which we can observe as behavior. Unfortunately, this observed behavior gives us little or no clue as to either of the perceptions or the key variable that rendered the mind's governing or engoverning decision.

These ramblings or musings have led me to suggest some new names for your scrutiny which avoid the "control" word in the description of the X phenomenon:

a) The "Cyberception" Theory of Behavior (feedback loop and perceptions) b) The "Cybercision" Theory of Behavior (feedback loop and mental
decision)
b) The "Internal Perceptual Conflict Comparator" Theory of Behavior
c) The "Perceptual Comparitive Ability" Theory of Behavior.

If "control" is so embedded in PCT, or so elemental that no other English word fits the bill, would it not be an improvement to call it The Perceptual Self-control Theory of Behavior? PST (like the sound you make in someone's ear to get their attention) might be more accurate and a bit catchy.

I await your barrage. Remember, they are just ideas. Some "trial balloons" about a subject that is both a new and important paradigm of understanding.

Best regards, Ken Kitzke

Subject: RE: Myths, repository
Date: 95-04-04 11:26:13 EDT
From: psy_delprato@emunix.emich.edu
To: DForssell@aol.com@emunix.emich.edu
CC: PSY_DELPRATO@emunix.emich.edu

Dag, -direct

I don't know where the list of misconceptions is. I do
believe that someone else (A. Andrews?) more or less
gave me the impression that he was going to pursue this.

If I find something,I'll pass it on. Actually, though, don't
Rick & Tom B. have their fingers on misconceptions?

Dennis
psy_delprato@emuvax.emich.edu

Date: Tue Apr 04, 1995 8:22 am PST
Subject: PCT and nervous system biology

[John E. Anderson (950404.0630 EDT)]

I've been lurking on CSG-L for a little more than a year now. I've posted occasionally, mostly about papers I have come across which might be of interest to PCTers (another one will follow this post). I was trained as a macromolecular crystallographer at Harvard, and I also worked at Cold Spring Harbor Laboratory in that capacity. However, over the years I have become more and more interested in the biology of behavior and the mind, and I finally decided to leave crystallography to pursue this interest. In the process I have been developing a theoretical model of the brain called "neurosemantic dynamics" (NSD). (The name stems from the theory's suggestion that on the most fundamental level, the meaning of a neural signal is the neural signal it induces. My friend Bob Franza, who reads CSG-L and told me about it, also pointed out Korzybski's prior coining of the word "neuro-semantic".)

I am interested in PCT because I think it offers a unique way to look at the immense complexity in structure and function of the nervous system. But I haven't seen much discussion of the biology of the nervous system as it relates to PCT since I've been reading CSG-L, though I admit that because of the volume I had not been reading all posts in their entirety until recently. B:CP has some discussion of the relationship between nervous system biology and PCT, but a lot has been learned about nervous system structure and function in the 22 years since it was published. Has there been any further development of its relationship to the PCT control hierarchy since B:CP?

Thanks for your help.

John

John E. Anderson, Ph.D. 904-448-6286 (phone)
9439 San Jose Boulevard #226 anderson@cshl.org (email)
Jacksonville, Florida 32257 jander@unf6.unf.edu (email)

Date: Tue Apr 04, 1995 8:05 am PST
Subject: new book: _The Gods of War_

[John E. Anderson (950404.0631 EDT)]

Below is the announcement of a new book which should be of interest to some on the net.

John

------- BEGIN INCLUDED TEXT HERE -------
From BIOSCI-REQUEST@net.bio.net Sun Apr 2 19:21:09 1995
To: neuroscience@net.bio.net
From: Jordan Peterson <godofwar@isr.harvard.edu>
Subject: Myth, Neuropsychology and Human Conflict
Date: Sun, 2 Apr 1995 18:49:47 -0400
Nntp-Posting-Host: isr.harvard.edu
Mime-Version: 1.0
Content-Type: TEXT/PLAIN; charset=US-ASCII
In-Reply-To: <69590.21194.17586@kcbbs.gen.nz>

I would like to announce the internet posting of a book-length manuscript I have written, entitled

The Gods of War: An Investigation into the Intrapsychic Bases of Motivation for Social Conflict

This book can be accessed in toto at

http://wjh-www.harvard.edu/~jbp/godsofwar.html

It purports to describe why human beings are prone to violent intergroup conflict, from the perspective of individual motivation. It presents a novel interpretation of the structure of mythology, and relates that structure to fundamental neuropsychological processes, manifested in cognition and emotion.

I have placed the text on the internet for experimental purposes.
A txt only gopher version is forthcoming.

I hope I am not violating any internet codes of conduct by informing your newsgroup in this way. If I have, please excuse my ignorance.

I hope you find the information I am offering useful and interesting.

Comments regarding the book can be sent to my alias at

godofwar@isr.harvard.edu

Sincerely,

Jordan B. Peterson, Ph.D.
Assistant Professor
Harvard Department of Psychology

Date: Tue Apr 04, 1995 9:56 am PST
From: prohugh
EMS: INTERNET / MCI ID: 376-5414
MBX: prohugh@ubvms.cc.buffalo.edu

TO: powers w
EMS: INTERNET / MCI ID: 376-5414
MBX: powers_w%flc@vaxf.colorado.edu
TO: g cziko
EMS: INTERNET / MCI ID: 376-5414
MBX: g-cziko@uiuc.edu
TO: * Purposeful Leadership / MCI ID: 474-2580
TO: Edward E. Ford / MCI ID: 591-3466
Subject: AERA Details

Just a quick note to get everyone up to date on AERA arrangements. We are scheduled to give our first session, 25.11, Perceptual Control Theory: A Postcognitive Theory of Behavior, at 8:15-10:15 am, Thursday morning, April 20, in Parlor 1, Ballroom level of the San Francisco Hilton Hotel. I think with everyone bringing what they are bringing, we should have all of the equipment we need.

Our follow-up session will be Thursday, April 20, 6:15-7:45 p.m. in the Powell room of the Hilton hotel. I will make flyers for that to pass out at the morning

session. I also do not have an LCD panel for that meeting, but we can set up the computers with demos that people can gather round. Ed Ford will not be able to join us at that time. He needs to get to a dinner nearby.

You are all invited (spouses too) to a reception I host for the Graduate School of Education on Wednesday evening, April 19, 6:45-8:45 pm, in the Union Square 23 room at the San Francisco Hilton. I have sent each of you official invitations. There will be some light hors d'oevres and a cash bar. You should either eat early, or, alternatively, we could all have a very late supper together. What do you think?

I am staying at the Hilton and you can let me know when you arrive. We should probably talk either sometime Wednesday during the afternoon, or at the late supper.

So, could I ask which you prefer?

Wednesday afternoon meeting at, say 4:00 pm, probably in my room

or

Late supper Wednesday evening after the UB reception

Please let me know.

Again, just to remind everyone, I will take 10 minutes to introduce the session. Bill can have 20 minutes, and Gary, Ed, and Dag, can have 15 minutes each. That should leave us with about 45 minutes for discussion and questions from the audience. Bring handouts that you want to leave with folks, copies of papers, etc.

Any questions?

See you in San Francisco.

=====+++++===========********=========+++++++++===========**

Hugh G. Petrie **716-645-2491**
367 Baldy Hall **FAX: 716-645-2479**
University at Buffalo
Buffalo, NY 14260
USA **prohugh@ubvms.cc.buffalo.edu**

Date: Tue Apr 04, 1995 10:14 am PST
Subject: conference announcement

[John E. Anderson (950404.0645 EDT)]

Below is the announcement of a conference which might be of interest to some on the net.

John

------- BEGIN INCLUDED TEXT HERE -------
From BIOSCI-REQUEST@net.bio.net Mon Apr 3 17:29:05 1995
To: neuroscience@net.bio.net
From: sriram@utaipx02.uta.edu (Sriram Govindarajan)
Subject: Conference on Neural Networks
Date: 3 Apr 1995 20:16:18 GMT
Nntp-Posting-Host: decster.uta.edu
X-Newsreader: TIN [version 1.2 PL2]

Preliminary Announcement and Call for Abstracts

Conference on Neural Networks for Novel High-Order Rule Formation Sponsored by Metroplex Institute for Neural Dynamics (MIND) and For a New Social Science (NSS)

Texas A&M University, May 20-21, 1995

MIND, a neural networks professional organization based in the Dallas-Fort Worth area, and NSS, a private research foundation based in Coral Springs, Florida, are jointly sponsoring a conference on Neural Networks for Novel High-order Rule Formation. This will partially overlap a conference on Creative Concepts May 19-20 sponsored by the Psychology Department at Texas A&M and the American Psychological Association. This will in turn be preceded by ARMADILLO, the region psychology meeting on Thursday, May 18 (whose registration is free for those attending either Creative Cognition or MIND/NSS).

Invited speakers for the MIND/NSS portion include John Taylor (King's College, London); Karl Pribram (Radford University); Risto Miikkulainen (University of Texas); Ramkrishna Prakash (University of Houston); Sam Leven (For a New Social Science); and Daniel Levine (University of Texas at Arlington). There is space for a limited number of contributed talks, for presentation on the Sunday of the conference, and an arbitrary number of posters, to up for the duration of the conference.

MIND has sponsored six international conferences, three of which have formed the basis for books (two in print and one now in progress). All but the first have been on focused topics within the neural network field. The topics were chosen for their interest to a broad community, some interested primarily in neurobiology, others in neural theory, and others in engineering applications. These last three topics have been Oscillations in Neural Systems, Optimality in Biological and Artificial Networks?, and Neural Networks for Knowledge Representation and Inference.

NSS has co-sponsored two of MIND's conferences. Its purpose is, to quote from its founding statement, "turning the findings and techniques of science to the benefit of social science." It seeks to develop more predictive methodological bases for areas ranging >from economics to management theory to social psychology ~ in some cases, to replace foundational assumptions dating from the time of

David Hume and Adam Smith, based on a static and unrealistic model of human behavior, with new foundational assumptions that draw on modern knowledge of neuroscience, cognitive science, and neural network theory. This would mean that social scientific models which assume humans always behave rationally will be replaced by models which incorporate emotion, habit, novelty, and ~ particularly relevant for this conference ~ creative intuition. In the words of NSS's original statement:

We may find people less rational than we would like them, economic models less precise, survey results less certain. ... We of For a New Social Science seek to find real answers instead of nostrums and mythology. But when we cannot find simple solutions, we choose to see our world plainly and to open our eyes to what we do not know.

The theme of this conference will be connectionist modeling of the processes by which complex decision rules are deduced, learned, and encoded. These include, for example, rules that determine, on the basis of some trials, which classes of actions will be rewarded. The myth that neural network methodology is only relevant for low-order pattern processing and not for high-order cognition is rapidly being disproved by recent models. In particular, the 1994 World Congress on Neural Networks included a session on Mind, Brain, and Consciousness, which was one of the most popular and successful sessions of that conference; another such session will be held at the same Congress in 1995. John Taylor has developed a series of models related to consciousness, which is interpreted partly as selective attentional (based in the thalamic reticular nucleus) and partly as comparison of current stimuli with episodic memories of past events (based in the hippocampus). Raju Bapi and Daniel Levine have constructed a network that learns motor sequences and classifies them on the basis of reward. Models have been developed that mimic disruption of specific cognitive tasks by specific mental disorders, among them Alzheimer dementia, autism, depression, and schizophrenia. Sam Leven and Daniel Levine have constructed a neural network that simulates contextual shifts in multiattribute decision making, with specific application to consumer preference for old versus new versions of Coca-Cola. Finally, Haluk Ogmen and Ramkrishna Prakash built on models previously developed by Grossberg and his colleagues to design robots that actively explore their environment under the influence of appetitive and aversive stimuli.

All this work paves the way for developing neural network models of creativity and innovation. Part of the creative process involves search for novel high-order rules when current rules fail to predict expected results or to yield expected rewards. This process often requires transfer to a higher level of complexity of analysis. Hence creativity involves what Douglas Hofstadter called a "search of search spaces." Some current models in progress also incorporate knowledge of different brain regions involved in circuits for such a transfer of control. Bapi and Levine discuss the role of the frontal lobes in such a circuit. In the

experiments modeled therein, macaque monkeys with prefrontal damage can learn an invariant sequence of motor actions if it is rewarded, but have difficulty learning any one of several reorderings of a sequence (say, ABC, ACB, BAC, BCA, CAB, and CBA) if all are rewarded. This flexible sequence rule is one of many types of complex rules that require intact frontal lobes to be learned effectively. Another is learning to go back and forth on alternate trials between two food trays. Yet another is learning to move toward the most novel object in the environment. Karl Pribram hints that the frontal lobes act in concert with some areas of the limbic system, particularly the hippocampus and amygdala.

These theories of specific brain regions are not yet precise or uniquely determined. Neural network models of high-order cognitive processes typically build on network structures that have previously been developed for low-order processes, and may or may not incorporate these neurobiological details. Still, we are now witnessing a dynamic convergence of insights from cognitive neuropsychology along with those from experimental psychology, cognitive science, and neural network theory. This will be the general theme of these two overlapping conferences.

Registration for this conference will be $40: registration forms are attached. Those attending the Creative Concepts Conference immediately preceding the MIND/NSS conference will be able to attend for $15. For information about transportation and lodging in College Station, TX (roughly between Austin and Houston) where Texas A&M is located, please contact:

Steve Smith
Department of Psychology
Texas A&M University
College Station, TX 77843
409-845-2509
sms@psyc.tamu.edu

If you are interested in speaking, please send an abstract by Friday, April 7, to

Daniel S. Levine
Department of Mathematics
University of Texas at Arlington
Arlington, TX 76019-0408
817-273-3598
b344dsl@utarlg.uta.edu

PLEASE RETURN THIS REGISTRATION FORM TO PROF. LEVINE

Name ____
Phone __________________________

Address ____________________________________

____
e-mail ____________________________________

1. I plan to attend (check all that apply):

ARMADILLO ____ Creative Concepts ____ MIND/NSS ____

2. I would like to present a talk or poster at MIND/NSS ____

++++++++++++++++++++++++++++++++++
Sriram Govindarajan
Department of Psychology
University of Texas at Arlington
++++++++++++++++++++++++++++++++++

Date: Tue Apr 04, 1995 10:24 am PST
Subject: Myths, Devils advocate

[From Dag Forssell (950403 1330)]

For Dennis Delprato and others with relevant files on myths:

PCTtexts include a file called DISPUTE .PCT. It begins as follows:

> **This paper evolved in the spring of 1993 as a collaborative effort by CSG netters to identify misunderstandings and "myths" by "devils advocates" among people in academe who have misinterpreted the fundamentals of Control Theory and rejected manuscripts submitted for publication in scientific and psychological journals. What follows is version 4 in this effort, crafted by Bill Powers. The supporting collection of misleading quotations in the literature is large. Detailed rebuttals to them have been drafted, but the quotations and rebuttals are not included in this version of this document.**

THE DISPUTE OVER CONTROL THEORY
William T. Powers and
The Editorial Board of the Control Systems Group

I have recently been thinking about how to present PCT to the world, as part of putting PCTtexts together. My thinking has been influenced a little by my reading this winter of "The Crime of Galileo" and "Dialogue concerning the two chief world systems." Galileo's crime was that he wrote in plain Italian for a literate non-academic public and showed that all the academics' arguments were nonsense. This infuriated some Jesuits, who trumped up some charges in an attempt to muffle Galileo.

It occurs to me that the effort (which fizzled) to create a paper on myths (for the lay public, I hope) was on the right track. In early June 1993, I attempted to collect all relevant posts, and submitted a disk to Bill P.:

Search of CSGnet 9209B through 9305E

Purpose: Gather control myths & misunderstandings with quotes in the literature for joint PCT paper. Assembled by Dag 930606. Where one of the search words appear and appears relevant to this purpose, the entire post is included.

Search words: Myth, Devil, Dennis. Other posts added as I came across them and they seemed relevant: Schmidt quotes, Degrees of freedom. Feedback too slow.

I am now revisiting this file to include it in the next update of PCTtexts in support of DISPUTE .PCT. In the thread, I notice the following:

Date: Sun Dec 20, 1992 9:08 am PST
Subject: Misstatements & Other Basics

[FROM: Dennis Delprato (921220)] >(Bill Powers (921218.1500)

>RE: Feedback is too delayed.

>Dennis, would you be willing to become a repository for citations
>from the literature containing misstatements about feedback
>control, PCT, etc.?

Pleased to, especially given that I seem to have already begun this out of my own curiosity.

Dennis, will you please E-mail or snail mail me your files on this, so I can:

a) resurrect the collaborative effort, or at least
b) distribute a good, complete thread on myths.

Have others saved files on this theme?

Comments?

Best, Dag

Dag C. Forssell
23903 Via Flamenco Valencia, California 91355-2808 USA
Phone (805) 254-1195 Fax (805) 254-7956
dforssell@mcimail.com

Date: Tue Apr 04, 1995 11:33 am PST
Subject: Re: Gopher and WWW

> [from Gary Cziko 9502002.0526 GMT]
>
> The current, revised INTRO TO CSGNET has information about the CSG's new

> Gopher server as well as the new, experimental, under-construction
> World-Wide Web homepage.
>
> Here is an extract of this new information for those of you not wating to
> have to wade through the INTRO to find it.--Gary
> ===
>
> GOPHER AND WORLD-WIDE WEB
>
> A number of documents as well as MS-DOS and Macintosh computer programs
> can be obtained via Gopher and the World-Wide Web (WWW site is currently
> under construction).
>
> For access via Gopher, connect to gopher.ed.uiuc.edu and follow the path:
>
> Higher Ed. Resources/
> Professional Societies/
> Control Systems Group
>
> or with your favorite Gopher server follow the path:
>
> Other Gopher and Information Servers/
> North America/
> USA/
> illinois/
> University of Ill.--College of Education/
> Higher Ed. Resources/
> Professional Societies/
> Control Systems Group
>
> The WWW address for the CSG homepage (under construction) is
> http://www.ed.uiuc.edu/csg/csg.html. We are currently experimenting with
> providing archives of CSGnet discussions via WWW.

Are you aware that most web clients (like mosaic, for instance) can
handle gopher format as well. For the gopher described above, the address would
be:

gopher://www.ed.uiuc.edu/

and for the CSG gopher page under that, it would be:

gopher://www.ed.uiuc.edu/1D-1%3a2020%3aControl%20Systems%20Group

:) Ray

Date: Tue Apr 04, 1995 7:14 pm PST
Subject: Gopher and WWW

[from Gary Cziko 950404.1712 GMT]

Ray Allis noted:

>Are you aware that most web clients (like mosaic, for instance) can

>handle gopher format as well. For the gopher described above, the address
>would be:
>
>gopher://www.ed.uiuc.edu/
>
>and for the CSG gopher page under that, it would be:
>
>gopher://www.ed.uiuc.edu/1D-1%3a2020%3aControl%20Systems%20Group

Yes, and our Web homepage points to the Gopher server, so one doesn't really need the URL Gopher address if one is on the Web.

--Gary

Date: Tue Apr 04, 1995 10:49 pm PST
From: CZIKO Gary
Subject: Files on Server

Dag: direct

I put the new files I received today on the server.

Did you get my note about using *.rtf format for document files? I can then put these on the World Wide Web and they will be nicely formatted and readable without decompressing, etc.

If you can convert a file to .rtf and send it to me, I can put it on WWW. After you see what it looks like on WWW, I think you will see that this is the way to go to make the PCT documents you have collected most accessible on the network.--Gary

Date: Wed Apr 05, 1995 12:03 am PST
Subject: Just got ABS

[Bill Leach 03 Apr 1995 21:13:53]

I just received my copy of Volume 34/Number 7 of ABS and while things are quite busy here I quickly read Phil Runkel's article.

I suggest that his opening paragraph is one of the single most outstanding statements concerning the difference between PCT and everybody else.

His single sentence "It {Control Theory} requires requires the belief that behaviour results from the _joint_ and _independent_ effects of an input or disturbance and an internal demand or standard." says simply and clearly both why S-R is wrong and why it is so enticing (maybe even more than most others have been able to express).

I may be getting a bit over-enthusiastic with this but his opening was devastating in such a small number of words. Additionally, his 'upbeat' view of a potential for excitement in future PCT research seemed so exciting.

-bill

Date: Wed Apr 05, 1995 1:56 am PST
Subject: Biology of behavior

[From Rick Marken (950404.2120 PDT)]

Gee, I'm getting no help on how to deal with the C-S gambit. It's either not important or it's been so effective that nobody knows what I'm talking about.

John E. Anderson (950404.0630 EDT)--

>I have been developing a theoretical model of the brain called
>"neurosemantic dynamics" (NSD).

What is NSD a model of? That is, what phenomena does it explain?

>(The name stems from the theory's suggestion that on the most
>fundamental level, the meaning of a neural signal is the neural signal
>it induces.

This sounds strange but maybe it is similar to the PCT model. In PCT, the meaning of a neural signal is determind by the perceptual function that produces the signal as output. If the inputs to the perceptual function are neural signals, then a particular value of perceptual signal is "induced" by these inputs, but the "meaning" of perceptual signal variations, regardless of the value of the signal at any moment, is determined by the function that tranforms inputs into outputs.

For example, suppose that p = x+y where p is the perceptual signal, x and y are input neural signals and "+" is the function transforming inputs into outputs. Then the "meaning" of p is "sum"; the particular value of that "meaning" at any instant depends on the value of the inputs to the perceptual (summation) function.

>I haven't seen much discussion of the biology of the nervous system as
>it relates to PCT since I've been reading CSG-L

For some reason the biologists (if they are out there) don't seem to post. It would great if you would join in the discussion from the biological point of view. I'm sure interested.

>a lot has been learned about nervous system structure and function in
>the 22 years since it [BCP] was published. Has there been any further
>development of its relationship to the PCT control hierarchy since B:CP?

Not nearly enough. Perhaps you could mention some of the more important things that have been learned about the NS in the last 22 years and we can kick around ideas about what these findings suggest about possible architectures for the PCT model (and vice versa).

Best Rick

Date: Wed Apr 05, 1995 10:14 am PST
Subject: Re: Biology of behavior

[From Bruce Buchanan (950405.10:45 EDT)]

[Bill Leach 03 Apr 1995 21:13:53] writes (inter alia):

>[Re:] Phil Runkel's article.... his opening paragraph is one of the >single most outstanding statements concerning the difference between PCT >and everybody else. [viz:] "It {Control Theory} requires requires >the belief that behaviour results from the _joint_ and _independent_ >effects of an input or disturbance and an internal demand or standard." >...

Well, I think PCT does not really differ from _everybody else_ (see below), and where there are commonalities these perhaps should be exploited.

[Rick Marken (950404.2120 PDT)] writes:

>For some reason the biologists (if they are out there) don't seem
>to post. It would great if you would join in the discussion from the
>biological point of view. I'm sure interested.

As a physician and erstwhile psychiatrist (I may not qualify as a biologist in a pure sense), I have a sense that practising health care professionals have found little use for the S-R models of academic psychology. In fact I think that physicians and mental health practitioners rely greatly on at least two models that have a lot in common with PCT. More specifically -

(1) Psychosomatic medicine: It has for many years been a commonplace of psychophysiology ("psychosomatic medicine") that an individual's autonomic nervous system response is actually a response to a threat, i.e. what is perceived or anticipated, rather than the response to an actual event (which of course has its own real physical effects). That is, in order to understand the pathogenesis of peptic ulcers or hypertension, at least insofar as these may be influenced by the patient's thoughts and emotions (fear, anger, etc.) one must be interested in how the patient sees the world. Whether or not the thoughts and fears, etc. are justified by circumstances may also be important, but they are quite a different question. For psychosomatic medicine it is perception that is the primary reality, and bodily reactions (and some diseases) are seen as responses and attempts to control such perceptions, not responses to realities as such.

(1) Dynamic Psychiatry: At another level, that of psychodynamic diagnostic and therapeutic models, it seems to me that S-R models have very little ulitility - other than perhaps in some of the psychometric tests employed by psychogists by way of laboratory investigation, as it were.(Even then, projective tests attach more importance to the patient's response in terms of his unique perceptions than to the stimulus, for the focus of attention is really not on the stimulus, despite its standardization e.g. Thematic Apperception Tests or Rorschoch).

In his influential textbook _The Practice of Dynamic Psychiatry_, Jules Masserman wrote (in Chapt. 25: Biodynamics and psychoanalysis: their therapeutic integration) as follows: (none of this will be news to PCTers)

"Principle I: all organisms are actuated by their physiological needs ..."

"Principle II: every organism reacts not to some absolute "reality", but to its own interpretations of its milieu in terms of its individual needs, special capactities and unique experiences."

"Principle III: whenever the goal-directed activities of an organism are partially or totally frustrated by external obstacles, the organism either changes its techniques in further attempts to reach the same goal or deviates its behavior towards a partial or complete substitution of goals."

"Principle IV: when two or more urgent motivations are in sufficiently serious conflict, so that the adaptive patterns attendant to each are mutually exclusive to the point of a paralyzing impasse, then the organism experiences mounting tension and apprehension reaching various levels of "anxiety", while its somatic and muscular behavior becomes either ambivalent, poorly adaptive and ineffectively substitutive (i.e. "nerotic") or progressively more disorganized, regressive and bizarrely symbolic (i.e., "psychotic")."

These statements encapsulate a synthesis of many theories of psychodynamics, as discussed at length by Masserman. They seem to me consistent with the proposition that behavior is the (attempted) control of perception. (Politicians also seem to have been convinced of this same truth.) Of course it will be recognized that, in another sense, none of these has the same disciplined approach to perception and behavior at the level of logic and detail as is found in PCT. That is what makes them different disciplines. What I think is significant is the convergence in conclusions from differing points of departure.

So my point is that among those who have actual responsibilities to understand and assist real people with real problems, as opposed to academic psychologists involved in theoretical models, there may be more common interests than seems usually recognized by many PCTers. And in numbers and diversity there may be strength, if communications and strategies can be found.

Cheers! Bruce B.

Subject: Re: RTF, WWW
Date: 95-04-05 13:04:39 EDT
From: g-cziko@uiuc.edu (CZIKO Gary)
To: DForssell@aol.com

Dag:

>Thanks for putting disks in their entirety on WWW. Will you so
>state to CSG-L, (I indicated in my posts you would when you had)
>or shall I?

I will let you do it.

>I have not been keen on controlling what I cannot perceive. I have
>no access to WWW as yet. America On Line will offer it soon,
>though.

It will blow your mind. When you get access, see what the Project Cybernetic has done at http://pespmc1.vub.ac.be/

>The advantage of RTF is that the format includes pictures such as my >various illustrations (which I have as EPS files). My many Windows >programs can export to RTF, so I am able to do what you suggest, If I >want to spend the time.

Just takes a few seconds. Illustrations can be included on Web documents. But I believe these will have to be converted to a bitmap format, such as GIF.

>If you tell me which size font to specify for suitable legibility and >which font shows well (New Century Schoolbook, Times, Helvetica) and >suggest an introductory file (PCT Intro and Resource Guide?), I will >mail you a file. I fail to see why 2.5 Megabytes of files in PCTtexts >shall get the treatment.

Font and size are IRRELEVANT. The Web viewers allow the user to choose this. Even line length is automatically adjusted for monitor size (at least when using Netscape)!

Yes, do send me the PCT Intro and Resource Guide in RTF and I will convert it to HTML and put on the Web. When you see it, you will now that this is the way to go. All the formatting and page width problems that we have been having will just disappear!--Gary

P.S. Do you happen to have the CSG logo in a bitmap format (PICT or GIF)? I would like to use this on the CSG Homepage.

Subject: Re: WWW, RTF
Date: 95-04-05 23:11:02 EDT
From: g-cziko@uiuc.edu (CZIKO Gary)
To: DForssell@aol.com

Dag:

>I have drawn it in Coreldraw and can export to most any format.
>What size do you want? XXX pixels x YYY pixels. Or A inches by B
>inches at so many pixels per inch.

I suppose it will be about 3 x 3 inches, but I have no idea of how many pixels per inch to ask for. Try something reasonable. Can you do this in GIF format?

>I shall send you an update to PCTdocs soon. Will include the RTF
>stuff and CSG logo per specification you send me.

Thanks.--Gary

Date: Wed Apr 05, 1995 9:59 pm PST
Subject: PCTdemos and PCTtexts at WWW

From Dag Forssell (950405 1400)

Gary Cziko sent me a direct note that the updated PCTdemos and PCTtexts are now available on the server. Help yourselves.

Or send me $10 for both disks.

The files are compressed in self-extracting archive files. These are executable in DOS, but not in other operating systems. Bill Leach sent me this report:

> **Hi Dag;**
>
> **The disks just arrived today.**
>
> **You could well suppose that (most people have emulators for DOS) but this is not the case.**
>
> **Unfortunately you did use an incompatible zip type program to create your archives and none of the three zip processors that I have recognize the files.**
>
> **The suggestions are the same as before though I'll expand a little...**
>
> **One of the PKZip programs makes Unix/GNU compatible self-extracting archives. The archives ARE NOT self-extracting on non-DOS machines (Unix, Amiga, Mac etc.) but the Unix/GNU zip programs can extract the files from the archive as though it was an ordinary archive (the programs ignore the self-extracting code).**
>
> **The other solution is to just use the GNU zip to create the archives and include the gzip program on the disks (you have batch files anyway so executing the gzip program from the disk would be no problem).**
>
> **-bill**
>
> **P.S. Oops!**
>
> **Just for the heck of it, I tried Lharc on one of the archives and it DID extract the files. Thus your archives are NOT incompatible since Lha (or Lharc) is also available for all platforms. You probably will want to mention that in your READ.ME files. Also mention that the archiver will report "WARNING: Skipping corrupt/extraneous data" when it starts [because it ignores the self-extract code].**
>
> **Also, the way to use Lharc would be to:**
>
> **CD to destination directory for files**
> **type Lha x <pathname><filename>**

. . . .

LHA is what I used. I will add a statement to the readme file:

Each directory holds a self-extracting archive file (using LHA compression) which holds files and programs in a compressed, space saving form.

Gary is after me to supply him with all the PCTtexts files in RTF format (Uncompressed. Over 2.5 MB in plain ASCII, more in RTF) so he can convert them to HTML format. With that, the files can be conveniently read on the WWW server, not just downloaded in one 1 MB compressed file. I expect to comply shortly. (2-4 weeks).

Best, Dag

Dag C. Forssell
23903 Via Flamenco Valencia, California 91355-2808 USA
Phone (805) 254-1195 Fax (805) 254-7956
dforssell@mcimail.com

Date: Thu Apr 06, 1995 1:40 am PST
Subject: The Sounds of Silence

[From Bruce Abbott (950405.1951 EST)]

Dear Abby,

Apparently there's another net vacation and this time _I_ didn't get an invitation. At first I thought it was the AERA meeting but I see that isn't until April 18-20th. Is it nap time? Spring break? Everyone decided that Skinner was right after all?

Signed

Forgotten in Fort Wayne

Date: Thu Apr 06, 1995 2:16 am PST
Subject: Conference registration info, repeat

from Mary Powers 9503.08

I'll be repeating this several times, in order to catch occasional lurkers and as a reminder to those who know in their hearts that they intend to come but haven't let me know about it.

I have sent this by snail mail only to CSG members who are not on CSG-L or who I think are only occasional. I hope no one drops through the cracks.

I really appreciate hearing from people who have been coming to the conference pretty regularly but can't this year. It helps a lot not to be left wondering. I do want to remind them that CSG membership for those not attending is $20.

******* CONFERENCE ANNOUNCEMENT *******

THE CONTROL SYSTEMS GROUP

The 11th annual meeting of the Control Systems Group will be held at Fort Lewis College, Durango, Colorado, from Wednesday, July 19 - Sunday, July 23, 1995.

This is a small, informal conference. There are seven sessions, morning and evening, for brief presentation and extended discussion of various aspects of Perceptual Control Theory. Attendees are encouraged to bring papers for distribution, which can serve to bring others up to speed on your topic. A variety of specialties in the life, social, and behavioral sciences will be represented, as well as applications in education, counseling, and management. There will be a Mac and a PC for demos, with a projection plate, and a VCR. Afternoons will be available for continued discussion, showing tapes and computer demos, or for recreation.

The first session Wednesday evening begins with scheduling those people who want to speak, whether or not they have brought a paper (bringing a paper carries no obligation to speak). The number of speakers determines the amount of time each will have.

This is considered to be a very enjoyable meeting. It is relaxed and informal, yet exciting and intensive.

Details

Durango is in the southwest corner of Colorado, 350 miles from Denver and 200 miles from Albuquerque, N.M., at an altitude of 6500 feet, hot and dry in the daytime, cool at night, with possible afternoon thunderstorms. Major attractions in the area include the Durango & Silverton narrow gauge steam train, Mesa Verde (and other) 11th to 14th century Anasazi Indian ruins, river rafting, mountain biking, etc., etc. For details on these and other attractions, plus motel and B&B accomodations if you prefer to stay off campus, you can call 1-800-463-8726 or write DACRA, 111 S. Camino del Rio, Durango CO 81301.

The conference fees, which include registration, CSG membership, room, board, coffee breaks, equipment, meeting room rentals, and a banquet on Saturday night, will be as follows:

Single occupancy...$220   Double occupancy...$175
Double room with guest: Self...$175   Guest....$150
Students (double only):..........$75
(This is subsidized. If a further waiver is needed, apply on registration form)
Off-campus: Self...$120 Guest...$95
Per night, before or after the meeting (Tues. or Sun., single or double...$25
(for those who want extra time in Durango)

July 1 is the deadline for registration. Please make full payment in advance of the meeting. PLEASE REGISTER AS SOON AS POSSIBLE by sending the form with $25 plus $5 for each guest ($10 for students). This is non-refundable. THE BALANCE DUE WILL BE YOUR

TOTAL CONFERENCE COST (above) MINUS REGISTRATION.

Conference fees include membership in the CSG. If you register and do not come, your registration fee will count as your dues.

Again, register early. I have reserved a limited number of rooms and will need to know if more are needed.

It is expensive to fly to Durango. If you are renting a car, it may be cheaper to fly to Farmington, New Mexico, and drive 45 miles (again, renting a car - there is no public transportation). Cheaper yet, if you have the time, may be to arrange to meet other conferees in Albuquerque and share a rental car for the 200 mile drive to Durango*. There is heavy tourist traffic and other conferences in the area, so don't wait too long to make your travel arrangements. And plan to arrive in time on Wednesday to get your meal ticket and get to the dining room before it closes at 6 pm. The first conference meeting will be at 7 pm.

More details and a campus map will be sent when you register.

Oh, yes. Anyone in Durango on Tuesday is invited to dinner at the Powers', which is 13 miles east of town. A map for that will be sent to anyone who indicates early arrival.

For further information you can call Mary Powers at 303 247-7986 or write to 73 Ridge Place, Durango CO 81301-8136, or contact powers_w%flc@vaxf.colorado.edu

***[Some sample fares: LA-Durango $498. LA-Farmington $268. Chicago-Durango $565. Chi-Farmington $418. Chi-Albuquerque $358]**

* * * *

1995 CSG CONFERENCE REGISTRATION

Name____

Address____

_______________________________________ Phone_________________

Arriving (day & time)____

Leaving (day & time)____

plane____ car____ (send maps) ____

I will be staying in the dorm the following nights (circle)

tues WED THURS FRI SAT sun

single____ double____ staying off-campus____

I am bringing a guest (name)________________________________

(note: Guests are welcome to attend all sessions of the conference)

At the banquet, I prefer (circle) wine beer pop juice

My guest prefers (circle) wine beer pop juice
Specifically? (red, white, diet, etc)

Mail this form with your enclosed check (made out to The Control Systems Group) for registration ($25 regular, $5 per guest, $10 student) or for your total conference fee to

Mary A. Powers 73 Ridge Place Durango CO 81301-8136

*** * * ***

STUDENTS: fill out only if you are applying for a waiver of fees over and above the built-in subsidy (but you must register and pay $10).

I am undergrad____ grad____ just graduated('95)_____

at____

I am applying for a waiver of the CSG conference fees.
Without the waiver I would be unable to afford to come.

Signed____

Date: Thu Apr 06, 1995 11:13 am PST
Subject: All together now: It's about CONTROL

[From Rick Marken (940405.2215)]

Bruce Buchanan (950405.10:45 EDT)--

>Well, I think PCT does not really differ from _everybody else_ (see
>below), and where there are commonalities these perhaps should be
>exploited.

We MUST distinguish PCT (as theory) from PCT (as the observation that behavior is control). PCT (as theory) may not differ that much from the theories developed by everybody else. In fact, PCT (as theory) is IDENTICAL to one theory (control theory) that has been used for decades by psychologists who study "manual control and tracking". It is PCT (as the observation that behavior is control) that really differs from _everybody else_ .

>Of course it will be recognized that, in another sense, none of
>these [other theories] has the same disciplined approach to perception
>and behavior at the level of logic and detail as is found in PCT.

I don't think this is the problem with these theories. Control theory itself has been applied to manual control with the same "level of logic and detail as is found in PCT". The problem is that (before PCT) control theory was applied to the wrong phenomenon; manual control theorists used control theory (with all the logic and math) to explain how inputs guide outputs -- because it looks like inputs guide outputs.

It is important to remember that William T. Powers did not invent control theory. In fact, Bill was only 8 years old when control theory was "officially" invented (by H. S. Black in 1934). Bill did something that was far more important (for the life sciences). He discovered the FACT that behavior IS control. He discovered this when he realized that organisms produce consistent results by adjusting their actions to _invisibly_ changing circumstances. He then realized that only control theory could explain this phenomenon. Finally, he realized that when control theory is properly mapped to the controlling done by living systems, what turns out to be controlled is a perceptual representation of the consistently produced behavioral results.

All this is distilled in Bill's felicitous phrase _Behavior: The control is perception_.

What this phrase DOES NOT mean is: "externally observable behavior can be explained by control theory".

What this phase DOES mean is: "behavior is a process of control and what is being controlled is perception.

Since the idea that behavior is the control of perception only makes sense in the context of the fact that behavior is control, and since conventional life scientists show no evidence of understanding the fact that behavior is control, any similarities between PCT and the theories of behavior developed or used by conventional life scientists is purely irrelevant.

Best Rick

Date: Thu Apr 06, 1995 4:56 pm PST
Subject: Re: Conference registration

[From Dick Robertson] 950406.1050CDT

Well, I'm one of those lurkers who will miss the conference this year for the first time. I'll miss you all, and hope you have a great conference. I expect to be there next year. This year Vivian and I will be just getting back from a month in Scandinavia - a long awaited dream trip. Best to all. Dick

Date: Thu Apr 06, 1995 7:36 pm PST
Subject: Re: Square-wave model; arm control

[From Bill Powers (950406.0730 MST)]

Bruce Abbott (950331.1050 EST) --

RE: square-wave disturbance results

One telling feature of the square-wave results is that the delay you find is 19 to 20 frames of 1/60th second (except for the one run with the shortest period) -- or 1/3 second. That is clearly longer than the delays we get with continuous tracking, which are seldom longer than 10-12 frames. So in cases where a discontinuity appears after some time of relative quiescence, we see a qualitative difference in the response to a disturbance. This suggests a higher-level system that acts as if a sudden disturbance after a quiescent period is a total surprise, however often it happens, so it has to decide all over again, each time, to turn on the tracking system, a decision that takes it about 1/3 second to put into effect. Of course that's just a general poetic impression and far from a running model.

I don't think that the correlations and RMS differences are very useful indicators when we get into this kind of detailed analysis. As we saw before, if we select the transition period to get the correlations and RMS prediction errors, the numbers (including the best-fit parameters) are very different from those we get if we exclude the transition period. It makes sense that during a fast transition, control is not going to be as good as it is between fast transitions; the bandwidth of the disturbance is very different in those regions, being much lower when things are changing slowly. We expect better control at lower bandwidths. It's very possible that higher-level systems change the gain of lower control systems as a function of the current bandwidths of disturbances -- or rather, as the errors get bigger and smaller.

This can happen in a number of ways. It can even happen in the basic arm-control system. Here's an experiment you can do. Grasp your right upper arm with your left hand so the thumb is feeling the triceps and the fingers are feeling the biceps. Hold the right arm straight out in front of you, stationary, while palpating the two muscles. You will feel some tension in the biceps, and the triceps will be limp.

Now start oscillating the forearm up and down from the elbow by about an inch or two, as fast as you comfortably can. You will feel the biceps AND triceps tensing, not just with each movement but on the average. The two muscles start pulling against each other, raising the muscle tone.

Then stop the oscillations and continue feeling the muscles for three or four seconds. You will feel BOTH of them slowly relaxing, the biceps returning to its initial mild tension and the triceps gradually relaxing all the way and becoming limp again.

According to Fig. 3 in that article by Winters and Stark that I mentioned a couple of days ago, the stretch-tension curve of a muscle is very close to parabola (I did some measurements on the figure) -- tension increases as the square of the stretch. In a model using opposed muscles, this turns out to result in a _linear_ overall spring constant in the region where both muscles are stretched, with the spring constant depending on the average tension, the muscle tone. This means that higher systems that move the arm about the elbow can control the position of the arm by using two reference signals in push-pull, one rising as the other falls, and can control the net spring constant in

the paired muscles by raising or lowering both reference signals together, in parallel. Considering the two reference signals as a balanced pair, the differential-mode signal controls arm position, and the common-model signal controls spring constant.

Controlling the spring constant changes the speed with which the arm can move a load, as you can see by varying Ks in Isaac2. It is quite possible that the best-fit transport lag we measure reflects an integral lag -- it's hard to be sure which kind of lag is occurring. Perhaps when motion is continuous, the opposing muscle systems are receiving common-mode reference signals that raise muscle tone, so the lag is shorter (the apparent integral lag of a control system decreases as the loop gain increases, even though the basic output time-constant remains the same). When the disturbance is momentarily constant, the system that raises and lowers muscle tone immediately begins to decrease muscle tone, and if the constant period is long enough, essentially relaxes the muscles until just one is pulling (or both are relaxed, if no force is required). Then when a sudden disturbance arrives, the muscles are in a low-spring-constant state, and the higher system takes some time to run the muscle tone back up toward the continuous-disturbance state. This results in an apparently longer transport lag -- but it could simply be a longer integral lag.

If you then go back to tracking with a continuous disturbance, the tone-control system maintains a higher muscle tone (it is quite a slow system) and the apparent lag stays short, something under 1/5 second.

Here is a second demonstration. Palpate triceps and biceps as before. Put your right fingers under a table-top (with a nice rounded edge) and pull upward with as large a force as you can comfortably maintain. You will feel a rigid biceps and a relaxed triceps. Then, while maintaining the upward pull, slide your fingers out from under the table-top, but maintain the same position when they finally come free. You will not feel a sudden relaxation of the biceps so much as a sudden tensing of the triceps. In the first instant, the triceps will keep the arm from moving upward by balancing out the force of the biceps, and both muscles will be tense. Then, gradually, both muscles relax, the triceps going all the way to limpness again and the biceps returning to the amount of tension necessary to support the forearm against gravity.

I don't _know_ that this is a higher-level system changing muscle tone -- we'd need electromyographic records to determine that. It could simply be that the contraction time constant of a muscle is much shorter than the relaxation time constant, so that when muscles are repeatedly tensed, the average level of tension tends to increase. This is quite likely true; each neural impulse causes an instantaneous shortening of muscle fibers, but they relax more slowly.

At any rate, we shouldn't be too hasty to build fancy models of higher-level systems until we're sure that we can't account for the phenomenon realistically in terms of known lower-level properties. What we need is an Isaac3 that uses separate muscles on opposite sides, with appropriate one-way action of the signals and muscles. That's what Isaac Kurtzer is working on, but I don't think he will take it amiss if I note that he is still learning, and probably won't mind if we continue with the

development.

One last detail. All neural signals have upper limits. If you put a magnitude limit on the error signal, what you will find is that for small disturbances you get the normal behavior, but for very large ones, you get an error signal that is constant at its maximum, and hence a constant slew rate of the integrated output. This can happen when you are moving a cursor a considerable distance on the screen, with large hand movements. (I suggest, by the way, calibrating the mouse so it moves about the same distance as the cursor moves on the screen). So in the transition from one position to another, you will find a constant-velocity region rather than an exponential curve. This can also lead to overshoots, because as the error signal finally comes down out of saturation, the braking is sudden rather than gradual and comes too late to prevent the overshoot. The constant-slope part looks like "ballistic" movement, but of course at the lower levels it is not.

This effect can easily be felt when moving a cursor from one target to another that is far enough away to be far outside the region of best visual acuity. The region in which position error is continuous with perceived distance is probably only a few degrees in radius. So I would guess that as the distance between the targets gets above some amount, the cursor movement will have a region of constant velocity, with deceleration being seen only near the end of the movement. We could actually measure the region of proportional error signal this way.

This is applicable to your square-wave experiment, because if the target jumps suddenly to a distant point, we would expect to see a quick acceleration of the cursor to a constant velocity, then a gradual deceleration beginning when the cursor gets close enough to the new target position. In my case there are some oscillations superimposed on the "constant velocity" region, showing that I have a slightly-unstable lower-level system in there somewhere. The error signal that limits is quite likely in the visual systems, not the kinesthetic systems.

I DID respond somewhat to this post of yours, when I remarked about the constant-time effect (long movements take the same time as short ones, over some range). For very long movement, of course, this wouldn't hold true (if you had to walk across the room to touch the far target, for instance).
--
As far as I can tell, your physics in the book-dropping demo is correct; I hadn't thought of getting the momentum transfer into the model, but your analysis looks right to me. Very nice, in fact. In fact, I can't wait to see it run.

Maybe the next step in this project, just to satisfy Rick, would be to model how the reinforcing effect of the score controls the control by the signal of the movement of the cursor to the new target.
Or is that way of talking starting to look a bit too simplistic to interest us much longer?

Anticipating the impact of the book, I suspect, would entail raising the muscle tone of the arm control system when the book is about to hit, but not trying to estimate the instant of impact, which would cause more

error than it cures.

Best, Bill P.

Date: Fri Apr 07, 1995 2:07 am PST
Subject: Hello darkness my old friend

[From Rick Marken (950406.0830)]

Forgotten in Fort Wayne (950405.1951 EST)

>Apparently there's another net vacation and this time _I_ didn't get an
>invitation.

I think at least part of the problem may be technical: the CSG server is posting in such an unreliable manner that it seems like it would be difficult or impossible for people to perceive and control the continuity of certain threads. This might be particularly true for those participating in this list only via the UseNet group. I posted a test post via UseNet on 3/30 (called "Anyone home?") and it has still not been posted to me via the CSG-L listserver. Gary Cziko apparently did the same thing and his post has already been posted to me via the listserver (but, then, he's the net god and the server might be religous). I just posted something last night; the server claims that it was distributed to 100 people but I have not seen it yet and it has not appeared on UseNet.

If this post gets through, could you let me know if you recieved my post from last night called "All together now: It's about CONTROL".

Best Abby

Date: Fri Apr 07, 1995 2:34 am PST
Subject: Re: Buchanan on Runkel on PCT

from Mary Powers 950406

Bruce Buchanan:

Bill Leach quoting Phil Runkel:

It (Control Theory) requires the belief that behavior results from the joint and independent effects of an input or disturbance and an internal demand or standard...

Bruce B:

Well, I think PCT does not really differ from everybody else (see below [quotes from Masserman]) and where there are commonalities these perhaps should be exploited.

Followed by some very PCTish principles from Masserman having to do with perception, changing reference levels, and conflict, and,

These statements encapsulate a synthesis of many theories of

> psychodynamics...of course it will be recognized that, in another sense, none of these has the same disciplined approach to perception and behavior at the level of logic and detail as is found in PCT...What I think is significant is the convergence in conclusions from differing points of departure.

The main point is that the various psychodynamic theories from which these principles are derived are conclusions from observations, and there is no basis for preferring them over other conclusions by other schools of therapy beyond personal preference. They are generalizations.

The operant word in Runkel's statement is "requires". Given the PCT model of organization, the statement about joint and independent effects is _generated_ by the theory, as are statements about perception, conflict, etc. etc. This is how control systems are organized, and how they work. If "people who have actual responsibilities to understand and assist real people with real problems" agree that this indeed seems to be what is going on, then they contribute importantly to the position that PCT is an accurate model of the way living systems are organized.

But simply expanding the fan club - figuring out how to get communication going with the like-minded - has its limits. Where PCT differs from everybody else (in the psychological sciences) is in being a theory in the sense of being a body of general laws, as opposed to theory as conjecture. It offers a model that explains how Masserman's principles work. The problem of communication is deeper than getting across the idea that this theory is in agreement with what people already know. It's a problem of getting across the idea of what a theory really is, or should be. And finding people who want to work with it at that level.

In PCT terms, PCT itself is a systems concept. Masserman's principles are the next level down. There is no explicit systems concept in what you quote from Masserman. PCT provides one. That's what makes it different. Not different in content, in "logic and detail", but different in level, which is why the principles, and the logic and detail, hang together so well.

Mary P.

Date: Fri Apr 07, 1995 4:03 am PST
Subject: Re: words for control; NSD

[From Bill Powers (950406.1030 MST)]

Ken Kitske (950404) --
Welcome to CSGnet!

More words to use in place of control, for which thanks. I think it's good to get the alternatives laid out, because the more of them I see,

the clearer it gets that there is only one word that will do, control.

As you note, this is a red-flag word. But it's a red-flag word for a good reason: people are always trying to control other people without any good idea of what they're doing or why it gets so hard to do. The real message of PCT is that controlling others is and must be difficult, even impossible in the long run. Nobody is going to understand the problems with control unless they learn how it works. Just changing the name to something else isn't going to solve the problem. If we call it glekking, then after a while people will start thinking of glekking as something terrible, for exactly the same reason they now think controlling is something terrible. What's terrible isn't the name, but the relationship among people that it means. Changing the name won't change the relationship, or make glekking any less a part of all living processes. So we just have to understand glekking, or control, or whatever you name it.

When people object to control, they are actually objecting for all the right reasons. What they have to learn is that _everyone_ controls, ALL OF THE TIME. The only problem is when you try to control someone else who is also controlling, and in such a way as to thwart the other person's ability to control. That's what everyone objects to. There is no alternative to controlling: simply having a goal and trying to reach it is controlling. If you want to give up all goals, including the goal of giving up goals, you are in for a hard time; for one thing, you'll have to get someone else to take you to the bathroom, feed you, and wipe your chin afterward. There is no such thing as life without control.

What we have to learn to do is recognize that other people are control systems, too, and figure out ways to get along without coming into deadly conflict. And there only one way to do that: face up to the fact that we all control, and learn what that means.

As to all the other non-PCT usages of the word control, I say to hell with it. If people want to use vague concepts in sloppy ways, that's up to them. But we don't have to go along with it.

--

John E. Anderson (950404.0630 EDT) --

> **I have been developing a theoretical model of the brain called "neurosemantic dynamics" (NSD). (The name stems from the theory's suggestion that on the most fundamental level, the meaning of a neural signal is the neural signal it induces. My friend Bob Franza, who reads CSG-L and told me about it, also pointed out Korzybski's prior coining of the word "neuro-semantic".)**

I'm more interested in the process that is being named than in the name of it. The term "neuro-semantic" just alludes to the fact that nervous system activities have something to do with meanings, which is hardly a surprise these days.

As Rick Marken pointed out, in PCT we also think of neural signals as central to everything, and that some neural signals are functions of others. If you've read the background material of PCT, you will know that we equate perception with the presence of a neural signal in an

upgoing pathway in the brain, and the concept of levels of perception is modeled by saying that perceptual signals at one level are functions of a sets of perceptual signals at lower levels. So it could be that we are on parallel paths.

The basic problem I see is that of explaining why the world looks as it does to human observers. If all information that the brain has about the outside world is in the form of neural signals, this means that NOBODY has any other way of knowing what is really out there. We can't compare a perception to the physical situation that gave rise to it, because when we try to look at the physical situation we're just looking at more perceptual signals. This is a tough problem to work out; I guess people have been trying to find philosophical solutions for rather a long time. Saying that "it's all neural signals" just makes the problem clearer; it doesn't provide an answer. Even "it's all neural signals" is made of neural signals.

In your formulation, a fuzzy spot is in what you mean by "the meaning" of a neural signal. If we recognize that a word like "cube" is itself a set of neural signals, and that when we hear "cube" we also remember or imagine a certain visual shape (another set of neural signals), then we can say that the first set "means" the second; i.e., somehow gives rise to it.

But we can also use the word "meaning" in another way. If there is a neural signal that stands for presence of a cube in visual space, then we can say that the meaning of the perceptual signal is the external pattern -- the physical situation -- to which it corresponds. And still another way: in a model of a neural control system, we can say that a certain signal, obtained by neurally subtracting a perceptual signal from a reference signal, has the meaning of "error signal" in our model -- although in the brain under observation, that meaning is never perceptually explicit. And finally, we can see that if there is an error signal, its meaning is to be found in the actions it causes which affect the outside world and cause the perceptual signal to become more like the reference signal.

So I think that "meaning" isn't a very useful word; you can't use it precisely without always tagging it with ancillary definitions and context-establishers, and that gets clumsy. It's more interesting to me to see proposals about specific dependencies of some neural signals on others, and about how the results might turn out to be identifiable aspects of experience.

Got a paper on your NSD theory that you can send people? I'd like to get a copy.

Best to all, Bill P.

Date: Fri Apr 07, 1995 5:49 am PST
Subject: Logical control

[From Rick Marken (950406.2015)]

Bill Powers (950406.0730 MST) --

>Here's an experiment you can do ...Hold the right arm straight out in
>front of you

Palm up, right?

Cool demos.

>we shouldn't be too hasty to build fancy models of higher- level
>systems until we're sure that we can't account for the phenomenon
>realistically in terms of known lower-level properties.

And now I'm nine and forty and, oh, 'tis true,'tis true.

>Maybe the next step in this project, just to satisfy Rick,

Well THAT would be a nice change;-)

>would be to model how the reinforcing effect of the score controls the
>control by the signal of the movement of the cursor to the new target.
>Or is that way of talking starting to look a bit too simplistic to >interest us much longer?

You coy Q-man, you;-)

I did spend a few minutes playing with the PC Turbo programs the other day. All I need to do is add the option of having no disturbance, a square wave disturbance (that moves the cursor to the target position on some occasions) or the regular disturbance (I got rid of the distracting "rewards", by the way; I find that I am mature enough to be able to keep the cursor on the appropriate target even if I am not rewarded for doing this). The computation of the proportion of time that the hypothetical controlled variable is in its reference state and the correlation between stimulus and response can be easily added to the analysis program.

By the way, I ran my logical variable control model in the three disturbance conditions in which I tested the (sub?) human subject (me) and found the same results for the model and person. For the model (as for the person) when there is no distrubance to the mouse, the correlation between stimuli and responses is nearly 1.0; the model acts as though it's responses are "controlled" by the stimuli. When the step disturbance is added, the stimulus-response correlation goes down to near 0.0. With the regular disturbance the stimulus-response correlation is about .5. In all cases, of course, the proportion of time that the logical variable is in its reference state ("true") is near 1.0. So, like the person, the model controls the logical variable but, in doing so, it acts as though stimuli are controlling its responses when there is no disturbance to these responses.

Best Rick

Date: Fri Apr 07, 1995 5:54 am PST
Subject: Re: Hello darkness my old friend

I would love to see your post "Altogether now: its about Control" however, at this time approx. 11:06 Thurs. evening, it has not appeared. Is it possible you people are posting to seperate groups that are supposed to cross-post? I admit to beginning to get a little frustrated with the missing pieces, here. Although I have little to say, as a lurker, it is bothersome.

And while I'm annoyed, I would like to post the question: have you applied your understanding of The Test to your responses to Newbies on the group? I see a rather severe controlling for accuracy to the original idea, at the expense of participation by persons who show up interested.

Just a thought. Susan.

Date: Fri Apr 07, 1995 6:51 am PST
Subject: Re: The Sounds of Silence

[Bill Leach 06 Apr 1995 21:40:03] [Bruce Abbott (950405.1951 EST)]

Humm, I note that I received your message on the evening of the sixth and that you posted it on the fourth.

-bill

Date: Fri Apr 07, 1995 8:18 pm PST
Subject: Controlling people, Accuracy-control

[From Rick Marken (950407.0830)]

Bill Powers (950406.1030 MST) --

>When people object to control, they are actually objecting for all the
>right reasons. What they have to learn is that _everyone_ controls, ALL
>OF THE TIME. The only problem is when you try to control someone else
>who is also controlling, and in such a way as to thwart the other
>person's ability to control.

This paragraph is a perfect summary of the book I've been trying to write for the last 5 years. It was going to be called "Controlling People". The title (suggested by Bill Powers) is a double entendre that communicates both points Bill makes above: 1) when "controlling" is read as an adjective modifying "people" the phrase refers to the fact that people are controllers; they control all the time 2) when "controlling" is read as a verb with "people" as the object the phrase refers to the fact that one of the things people try to control is other people.

>There is no alternative to controlling: simply having a goal and trying >to reach it is controlling...There is no such thing as life without >control... What we have to learn to do is recognize that other people >are control systems, too, and figure out ways to get along without >coming into deadly conflict. And there only one way to do that: face up >to the fact that we all control, and learn what that means.

And that was going to be the conclusion of the book; we have to face up to the fact that we are controllers and we have to learn to recognize when we are controlling. When we can look at our own controlling "objectively" we have gone up a level -- and this is the only way we can "get past" the kind of controlling the produces inter- and intra-personal conflict.

Susan Schweers (950407) --

>I would love to see your post "Altogether now: its about Control"

Are you recieving posts from the CSG listserver or from UseNet? That post appeared yesterday (4/6) at both sites. Have you seen it yet?

>I admit to beginning to get a little frustrated with the missing >pieces, here.

Blame it all on Gary Cziko, the net god;-) With gods like him (and Yahweh) is it any wonder that we've got all these athiests running around;-)

>have you applied your understanding of The Test to your responses to >Newbies on the group? I see a rather severe controlling for accuracy >to the original idea, at the expense of participation by persons who >show up interested.

I'm not sure how The Test would affect the severity of my controlling for accuracy, except to prove that I AM controlling for accuracy. If it's any consolation, I consider this a character flaw myself; I have been trying to reduce the gain of my "PCT accuracy" control system over the course of the last year (believe it or not). I am trying especially to be less severe with newcomers. I don't think I ever "lowered the boom" newcomers unless they come on with an "I know all about PCT; let me explain it to you" attitude and then proceed to confidently make one false claim after another about it. I would never "lower the boom" on you, for example. And if it ever seems like I am doing that, just scold me; I respond well to scolding. Just ask Mary Powers:-)

Best Rick

Date: Fri Apr 07, 1995 8:23 pm PST
Subject: Anticipation

[From Bruce Abbott (950407.1025 EST)]

>Bill Powers (950406.0730 MST) --

Bill, you mentioned the fact that a person will stiffen both the biceps and triceps when anticipating the impact of an object being dropped into the hand. A similar phenomenon occurs when you have been, say, lifting a series of fully packed, heavy boxes. If, unknown to you, the next box is only a quarter full, you will nearly throw the box into the air as you lift it. Because of the your prior experience with the heavy boxes, you will have set your reference for initial force exerted on the box to a much higher level than that actually required. This again reveals an anticipatory action that depends, not on current feedback from the low-level sensors, but on predictive cues whose significance depends on prior experience.

This ability to vary output so as to begin to counter _expected_ disturbances is extremely important for the well-being of most organisms. In most cases it is better to react unnecessarily to anticipated disturbances (and perhaps risk appearing foolish) than to fail to react. However, repeated encounters with predictive cues that "fail to deliver" do eventually reduce the ability of those cues to act as disturbances, whether this ability is innately given (in which case the reduction is called "habituation") or learned (in which case the reduction is called "extinction"). The energy and time costs of reacting unnecessarily are eliminated in this way.

An ordinary housefly will crouch down and then spring into the air if it detects a rapidly looming visual image, even though the visual image itself is unlikely to harm the fly. It is the fact that such images are regularly followed by rather drastic disturbances to a large number of the fly's control systems that is responsible for this linkage: over the course of evolution, flys not so equipped failed to survive and left few if any offspring. Anticipation may thus come "built into" the organism's control-system structure or may be acquired (also via inherent mechanisms)--or lost--through experience. It's interesting that even the simple act of dropping a book into someone's hand or lifting a weight cannot be truly adequately modeled without including the effect of anticipatory cues in the simulation.

Regards, Bruce

Date: Fri Apr 07, 1995 9:06 pm PST
Subject: Annoyed Newbie

From Dag Forssell (950407 0840) >Susan Fri Apr 07, 1995

>And while I'm annoyed, . . .

Susan, you express annoyance with the continuity and completeness of the posts sent to you from the listserver. I would suggest you send a message SET CSG-L DIGEST to the listserver (as shown in the monthly INTROCSG.NET):

```
      To subscribe to the listserv version of CSGnet, and learn about
      options & commands, subscribers and archives, send a message to

      Internet:

      LISTSERV@VMD.CSO.UIUC.EDU

      Message:                   (Comments: Not part of your message)

      Subscribe CSG-L Firstname Lastname Institution   (Your OWN name)
      help                       (Basic introduction to commands)
      info refcard               (Comprehensive reference of commands)
****  set CSG-L digest           (All CSG-L mail delivered once a day)
      set CSG-L repro            (Get copy of your own postings)
      set CSG-L ack              (Receive acknowledgements when posting)
      query CSG-L                (Your mail status & options)
      review CSG-L countries     (Subsribers & addresses, by country)
      index CSG-L                (List of archive files available to you)
```

get CSG-L LOG9502B (Get archive for second week of Feb 1995 --shown here as an example only).

This way you get one daily message from the listserver with all of the previous day's posts in order.

>I would like to post the question: have you applied your
>understanding of The Test to your responses to Newbies on the
>group? I see a rather severe controlling for accuracy to the
>original idea, at the expense of participation by persons who show
>up interested.

What do you mean by interested? This is a serious group, discussing a new science of life. We do try to make good information available to Newbies, but as a Newbie, you have to do your part. Have you studied the monthly INTROCSG.NET? Have you taken action on suggestions in it to obtain the basic information on PCT? Have you obtained and studied the free PCT Introduction and Resource Guide, either from the WWW server, from my original posting while you were lurking, or by sending me a stamped envelope. Have you reviewed the PCTdemos and PCTtexts available from WWW or on disk?

Susan, I don't remember that you responded to Bill Powers post:

Subject: What is Science.
[From Bill Powers (950213.0845 MST)]
The rest of this is for both you and Susan Schweers
(950212 etc).

If you are interested in PCT, want to participate and be taken seriously, you have to do due diligence. When you demonstrate a sincere interest by informing yourself, your participation will be worth your while and ours.

Best, Dag

Date: Sat Apr 08, 1995 2:49 am PST
Subject: Studying principles of reorganization

[From Bill Powers (950407.0825)]

Mary and I celebrate our 39th year of marriage today. Still working.

I've been experimenting with reorganization since Martin Taylor reported the results obtained by his mathematical colleague. The source code for my experimental progam is appended to this post, for those who can understand, compile, and run Turbo Pascal (should work for all versions from 3.0 up).

I can confirm the general idea that the dependence on dimensionality is not very steep in terms of total number of iterations. At 9 dimensions, a typical run that reduces the error to 10E-8 of the initial value takes around 50,000 iterations of my program, while for 30 dimensions it takes perhaps 300,000 to 500,000 iterations.

However, the TIME required goes up as the square of the number of dimensions (or so) simply because each iteration takes a much longer time when more equations have to be evaluated on each iteration. In a parallel system this time penalty would not occur.

Also, as Martin suspected, the rate of reduction of error depends very much on the specific environment in which the reorganization is happening. More on that in a moment.

The basic model I used is organized around solving n equations in n unknowns, by means of reorganization and not analytically. For each equation,

```
        j=m
  y[i] = SUM( x[j]*a[i,j])
        j=1
```

The coefficients a[i,j] are chosen at random in the range between -1 and 1 for a given run. Then, to make it possible to verify that a solution has been found, a set of target values for y is generated by evaluating each equation with all x's set to 1.0. I began by choosing the target values at random, but then realized I couldn't tell if the answer was right without actually solving the equations analytically, which I can't at present do with the programs I have. So I used this method to give me a known answer (I could have picked any set of values for the x's in generating the target values of the y's). If all the x's come to a value of 1.0, all the y's will be at their target values, so the check verifies a correct solution.

OUTLINE OF THE MODEL

The error signal being reduced is

```
          i=n
 errsqr = SUM( (target[i] - y[i])^2)
          i=0
```

"Error" is also computed as sqrt(errsq).

First, all the x's are set to zero. Then all n equations are evaluated to calculate the y's. The initial values of the y's are all zero. Then the total squared error between the actual y's and the target values is computed, and the linear error is found. If the present error is greater than the error on the previous iteration or the initialized value of zero, a reorganization occurs.

Reorganization consists of choosing at random new values between -1 and 1 for a set of deltas, dx[j].

Whether a reorganization side-track occurred or not, on each iteration the values of the x's are changed by adding the deltas (times a speed factor) to the corresponding x's:

```
  x[i] := x[i] + dx[i]*speed*error
```

Thus the set of x's define a point moving in n-dimensional space at a speed proportional to the remaining error -- the closer the point comes to the solution, the slower the speed. The solution is the position target[i] in hyperspace. When a reorganization occurs, the moving point takes off in a new direction in hyperspace: an n-dimensional E-coli-type biased random walk.

I found that making the "speed" constant equal to 1/(square of number of dimensions) led to convergence at all dimensionalities from 1 on up, although I don't know if this is the optimal value.

PERFORMANCE OF THE MODEL

In all trials for all numbers of dimensions up to 50 (limited by available memory), a mostly monotonic convergence to a solution occurred. That is to say, on a plot of error against time, the error curve appeared smooth, although of course it contained many small wiggles not obvious to the eye. In all cases the values of the y's came to the target values to four significant figures (actually much better than that, but I displayed only four significant figures). I did not let the 50-dimensional case run to completion because that would have taken about a week, and I need my computer for other things.

After I changed to selecting y's that required all the x's to come to a value of 1, the x's all came to a value of 1.000. I have not seen any cases where convergence to a final value failed or even where the trend of the error versus time curve reversed, although in principle that could happen if the matrix of a[i,j] coefficients happened to have a determinant of exactly zero. So for all practical purposes, I think we can say that this reorganization method converges to a solution under all but a very unlikely set of circumstances, quite probably for any number of dimensions.

There was, however, an occasional dramatic change in the speed of convergence.

I first noticed this in a run using 9 dimensions. Prior to that, I had seen that some runs converged much faster than others, but I started paying closer attention when I saw a run in which the error curve started out by declining smoothly, and then suddenly changed to a rate of decline less than one tenth of the former rate. The solution was eventually reached, but only after a large number of iterations. With continued experimentation, I found that this sudden change in rate could be of any magnitude, and occur at any level of error. There could also be several changes in rate of decline of error during a run.

I think the reason for the abrupt change in convergence rate has to do with choosing the coefficients of the equations at random. Because of this random choice, it is very unlikely that the hyperspace lines described by each equation are all mutually orthogonal -- that would as unlikely as their all being parallel. On occasion, and not very rarely, some of the lines might have only small angles between them. This would mean that in most of the dimensions changes in the value of each x would have about the same amount of effect on the corresponding y, but that for some of the x's a large change would be required. The initial fast reduction of error results from changes in the x's that satisfy most of the requirements for reducing error. However, as the solution is approached, suddenly most of the x's are near the right values but one or more of them is far from its required value.

Now the reorganizations tend to make matters worse for most of the x's, so reorganization has to continue not only until the outliers come closer to the right values, but until the other x's are not changed too much away from the correct values. This causes the net gain in error-reduction to be slower, partly because many more reorganizations have to occur before a successful direction is found and partly because the length of travel before error starts to rise again is shorter.

Another way to see this is that when all the x's are too small, convergence goes at a high rate, but eventually at least one of the x's will become too large, and then there can be a reduction in the convergence rate of an amount depending on how much too large the outlier is.

I did one test in which I set up a matrix of coefficients to make sure that all the equations were orthogonal: this was simply the diagonal matrix, with all elements but those on the diagonal being zero, and the diagonal entries being chosen randomly as 1 or -1. This leads to extremely rapid convergence with any number of dimensions up to 50. The point moves essentially as rapidly as it can go, given the chosen speed. There are no sudden changes in slope of the error curve.

However, I could use some help here. I would like to set up other matrices with guaranteed orthogonality, but don't know how to do it. I would appreciate some instructions. The diagonal matrix is obviously a very special case; I'd like to check this with a more general orthogonal matrix.

SOME PREMATURE CONJECTURES ABOUT THE MEANING OF THE EXPERIMENTS

What I have modeled here is not a normal control system, but a set of perceptual functions (each equation) which receive n inputs from the environment (the values of x) and output a perceptual signal for each one (the values of y). For each perceptual signal, there is a reference value (the "target" values). The error signal, squared, becomes the basis for random actions that alter all of the x's, which are inputs to all of the perceptual functions. Each perceptual function applies a set of randomly- chosen weights to the inputs.

What I have learned so far is that for any random choice of perceptual input weights (except a vanishly small proportion of sets that prevent a solution from existing), it is possible to find, through reorganization, a set of values of the x's that will bring all the perceptual signals to their respective reference levels or target values.

However, I have also found that sometimes progress toward finding such sets of x's can start out very rapid, and then drastically slow down, so that completing the process can take a very long time. This slowing down can happen while the error is still very large, or after it has been reduced by 90 or even 99 percent.

The conjecture to follow corresponds imperfectly to the model, but I think that by looking at some permutations of the model it can be made better.

This model assumes that n perceptual systems are formed, each with randomly-selected input weights for n environental variables, the x's. What we seem to have found is that for any random way of organizing perceptual input functions, it is possible to find output actions that will control each resulting perceptual signal relative to any arbitrary reference value within some possible range.

However, the slowing-down effect tells us that for some assortments of random input weights, error correction proceeds rapidly only to a certain level of error, and after that becomes very much more difficult. So practically any way of perceiving the environment provides a way of controlling it in many dimensions at once -- but some ways of perceiving it make control far more difficult.

What I have been doing, or will be doing when this model is made more like a learning-to-control model, is reorganizing the output function, with the input function being essentially randomly organized. The output reorganization may go all the way to completion with no problems, but it may also go only part of the way and then suddenly slow down. What this says is that the total system is controlling the inputs from the environment in a way that is in the right ballpark, but that begins to get awkward when the remaining error has to be corrected.

At this point, it would seem appropriate to change over to reorganizing the input function by altering its input weights, the coefficients in the equations. Again, the total error over all systems would have to be summed, and reorganization would have to affect all of the systems, because we still require that total error be minimized. But now we have the outputs organized to be partially effective in correcting error. By changing the input weights, all n-squared of them, we can, I am conjecturing, bring the matrix of coefficients closer to orthogonality, thus making control more effective by reducing interactions among the control systems on the input side.

What this amounts to is first obtaining some degree of control by reorganizing outputs, and then finishing the job by reorganizing perceptions to represent the external world along more natural "lines of cleavage." Of course this idea so far does not take into account interactions among the x's, but when they are introduced they may make the whole job easier rather than harder by creating natural orthogonalities.

All of this rests on the presupposition that if the input matrix, which represents all the simultaneously-active input functions, is made orthogonal, reorganization will proceed at maximum speed. That's why I need to find out how to create general orthogonal matrices. We need to see if there is some general and predictable effect of departures from orthogonality on speed of convergence, and if, indeed, orthogonality is what is needed to make reorganization work best. If that can be verified, we can go on to try different combinations of reorganizing effects on different parts of the control loops.

I'm going to hit the textbooks to see if I can find an understandable-by-me solution to this problem. If someone else wants to solve it, please do.

All you bystanders out there wondering what this is all about, take note that there is more to PCT than can be expressed in words. You don't have to learn about it at this level, but it should be comforting to know that at least a few people are doing this.

Best, Bill P.
==

```
program reorg1;

{ Solve maxvar equations in maxvar variables using Ecoli method of
```

```
  solving equations. Exit on Esckey or 'q'}

uses dos,crt,graph,grUtils,setparam,frameplt;

const maxvar = 15;   {adjust this before compiling and running}

var param: paramlisttype;
    frame: frametype;
    time,dt: real;
    maxx,maxy:integer;
    x,dx: array[1..maxvar] of real;
    a: array[1..maxvar,1..maxvar] of real;
    y: array[1..maxvar] of real;
    target: array[1..maxvar] of real;
    speed,thresh,err,errsq,lasterrsq,lasterr,dummy,dum2: real;
    ch: char;
    i,j: integer;
    numstr: string;
    iterations: longint;
    count: integer;

procedure setgraphics;
begin
 initgraphics;
 maxx := getmaxx; maxy := getmaxy;
end;

procedure initvars;  { Initialize program variables }
var sumsq: real;
begin
 for i := 1 to maxvar do
  begin
   x[i] := 0.0;
   dx[i] := random - 0.5;
   for j := 1 to maxvar do
    a[j,i] := 1.0*(random - 0.5);
  end;
  for i := 1 to maxvar do
   begin
    y[i] := 0.0;
    for j := 1 to maxvar do
     target[i] := target[i] + a[i,j]*1.0;
   end;
 time := 0.0;
 dt := 1;
end;

procedure loadparams;  { Set up parameters }
begin
 with param[1] do
 begin
  legend := 'Speed';
  kind := 'r';
  rvinit := 1.0/maxvar/maxvar;
  rvmin := 0.0;
```

```
  rvmax := 1.0;
  rvstep :=1e-4;
  rv := @speed;
 end;
 with param[2] do
 begin
  legend := 'Thr*100';
  kind := 'r';
  rvinit := 0.0;
  rvmin := -1.0;
  rvmax := 1.0;
  rvstep :=1e-4;
  rv := @thresh;
 end;
end;

procedure loadframes;  {Set up plot of variables}
begin
 with frame do
  begin
   numyvars := 2;
   mx := maxx; my := maxy;
   xbase := 50;
   ybase := 20;
   xsize := 300;
   ysize := 400;
   numxgrid := 20;
   numygrid := 20;
   xzero := 0;
   yzero := 0;
   xmax  := 100000.0 ;

   ymax[1] := 1.0;
   ymax[2] := 20.0;
   ylegend[1] := 'error' ;
   ylegend[2] := 'de/e' ;
   xlegend := 'TIME, sec' ;
   color[1] := lightcyan;
   color[2] := lightred;
   yvar[1] := @err ;
   yvar[2] := @dum2 ;
   xvar := @time ;
  end;
end;

begin
 setgraphics;
 randomize;  { remove this to repeat run with same conditions}
 initvars;
 loadparams;
 loadframes;
 InitFrame(frame);
 SetupParam(400,200,2,param);
 clrplot(frame);
 ch := chr(0);
```

```
 setfillstyle(0,0);
 iterations := 0;
 count := 200;
 i := maxvar;
 str(i,numstr);
 outtextxy(100,20,'NUMBER VARIABLES = '+ numstr);
 lasterr := 0.0;

{MAIN LOOP}

 repeat
  repeat
   while not keypressed do
    begin

     {Evaluate equations to calculate value y[i]}
     for i := 1 to maxvar do
     begin
      y[i] := 0.0;
      for j := 1 to maxvar do
       y[i] := y[i] + a[i,j]*x[j];
     end;

     {Compute error between current vector and target vector}
     errsq := 0.0;
     for j := 1 to maxvar do
      errsq := errsq + (y[j] - target[j]) * (y[j] - target[j]);
     err := sqrt(errsq);

     dec(count);
     inc(iterations);

     {Show current error and number of iterations}
     if count <= 0 then
     begin
      count := 200;
      str(err:10:8,numstr);
      numstr := 'Error = ' + numstr;
      bar(350,10,350 + textwidth(numstr),20);
      outtextxy(350,10,numstr);
      str(iterations,numstr);
      numstr := numstr + ' Iterations';
      bar(350,25,350 + textwidth(numstr),35);
      outtextxy(350,25,numstr);
      dummy := dummy + 0.1*((err - lasterr) - dummy);
      lasterr := err;
      str(dummy:9:7,numstr);
      dum2 := -dummy;
      numstr := numstr + ' de/iteration';
      bar(350,40,350 + textwidth(numstr),50);
      outtextxy(350,40,numstr);
     end;

     {Check change of error: if > threshold, reorganize}
     if (err - lasterrsq) > 0.01*thresh then
```

```
      begin
       for i := 1 to maxvar do dx[i] := random - 0.5;
      end;
     lasterrsq := err;

     {Add deltas to values of x}
     for i := 1 to maxvar do
      x[i] := x[i] + dx[i] * speed * err;
     plotvar(frame);
     if (frame.ymax[1] > 0.0001)
        and (frame.yvar[1]^ < 0.1*frame.ymax[1]) then
      begin
       frame.ymax[1] := 0.1*frame.ymax[1];
       InitFrame(frame);
      end;

     time := time + dt;
     if time > frame.xmax then
     begin
      time := 0.0;
      clrplot(frame);
     end;
    end;
  until keypressed;

  {Allow changing parameters while running}
  ch :=  ChangeParam(param);
  if ch = ' ' then
   begin
    ch := readkey;
    ch := chr(0);
   end;
 until (ch = 'q') or (ch = #27);

{ END OF MAIN LOOP}

 restorecrtmode;

 {print actual and target vector elements}
 errsq := 0.0;
 writeln; writeln('Actual y followed by target y');
 for i := 1 to maxvar do
  begin
   errsq := errsq + (y[i] - target[i])*(y[i] - target[i]);
   write(y[i]:6:3,' ',target[i]:6:3,'   ');
  end;
 writeln;
 err := sqrt(errsq/maxvar);
 writeln;
 writeln('Total mean error (RMS) = ',err:12:10);
 writeln;
 writeln('Values of x');
 for i := 1 to maxvar do
  write(x[i]:8:3);
 writeln; writeln;
```

```
 ch := readkey;
 if maxvar < 16 then
 begin
  writeln('Matrix of coefficients');
  for i := 1 to maxvar do
  begin
   for j := 1 to maxvar do
    write(a[i,j]:8:3);
   writeln;
  end;
 end;
 ch := readkey;
 closegraph;
end.
```

Date: Sat Apr 08, 1995 2:59 am PST
Subject: Re: Newbies and accuracy

from Mary Powers 950407

Susan Schweers asks:

> Have you applied your understanding of the Test to your responses to Newbies on the group? I see a rather severe controlling for accuracy to the original idea, at the expense of participation by persons who show up interested.

By applying the Test do you mean are old-timers aware that some Newbies feel shut out by a lack of simple explanations of what we are talking about, and that we criticize their efforts to get in on the conversation?

This is because the group has three purposes. It serves as a means for a small and widely scattered group of people to continue a discussion that's been going on for 4 1/2 years. This is fairly informal, but it does assume an aquaintance with the subject. It serves as a Virtual Institute, with people who are hundreds of miles apart, and who haven't even met, doing some research projects together. Finally, it is a public forum, which anyone who happens across it is welcome to join.

What a Newbie may not realize is that new Newbies keep coming along. Initially their questions about PCT were answered, patiently and at length. This got to be pretty repetitive, tiresome and time-consuming. Since the questions tend to be pretty similar, we began a monthly posting of an introduction, including resource material and a bibliography. This is by way of telling Newbies that the answers to a lot of their questions have been written down already, please take the time to read some of this material, and then ask your questions about whatever you disagree with, don't understand, or don't find covered in the books.

As for a "rather severe controlling for accuracy to the original idea" - what would you prefer? Sloppy generalizations? The aim of PCT is to put the behavioral, social and life sciences on as solid a footing as chemistry and physics. Severe accuracy is the name of the game.

The potholes developing in the Infobahn are not of our making.

Mary P.

Date: Sat Apr 08, 1995 3:20 am PST
Subject: Re: Newbies and accuracy

[From Oded Maler (950407) - again]

Mary Powers 950407:

Susan Schweers asks:

> Have you applied your understanding of the Test to your responses to Newbies on the group? I see a rather severe controlling for accuracy to the original idea, at the expense of participation by persons who show up interested.

> As for a "rather severe controlling for accuracy to the original
> idea" - what would you prefer? Sloppy generalizations? The aim
> of PCT is to put the behavioral, social and life sciences on as
> solid a footing as chemistry and physics. Severe accuracy is the
> name of the game.

I would call it ("controlling for accuracy") as a futile attempt to create in the minds of others the *exact* perceptual variables that the "oldie" has. This is useless because in order to reach the same variables one has, at least, to go through the same devlopment as the "oldie" (including the adoption and then the rejection of the silly beliefs of experimental psychology). It is silly because it also assumes the same meaning of words and is based on some very coarse classification of the world: there are pre-PCT people who devote most of their time to S-R psychology and PCT people who don't do *that* terrible thing anymore.

But it's fun.

--Oded

Date: Sat Apr 08, 1995 4:01 am PST
Subject: Re: Anticipation

[From Rick Marken (950407.1450)]

Bruce Abbott (950407.1025 EST) --

>This again reveals an anticipatory action that depends, not on current
>feedback from the low-level sensors, but on predictive cues whose
>significance depends on prior experience.

Actions never really "depend on feedback" (this phrase implies that feedback is a perception caused by action; but in a control system, perception is always the combined result of action and disturbance; it is impossible for a control system to extract just the "feedback" component of perception, and it doesn't need to, anyway). Actions depend on the continuously varying discrepency between perceptual and reference signals in a negative feedback loop.

"Predictive cues" is one of those phrases that suggests that stimuli can do far more than they can actually do. I say we throw it in the trash barrel along with "stimulus control". "discriminative stimulus", "reinforcement" and "selection by consequences".

>This ability to vary output so as to begin to counter _expected_
>disturbances is extremely important for the well-being of most >organisms.

This is a myth that probably originated with the "generated output" cult, particularly the sect that worships "feedforward control". I think it would be a good idea to build a 'predictive" control system and see what actually happens. That will be my project for the weekend.

>In most cases it is better to react unnecessarily to anticipated
>disturbances (and perhaps risk appearing foolish) than to fail to react.

Well, I think we should test this and see if it's true.

I DO think that control can sometimes be improved if the controller is able to perceive and control a situation in terms of a higher level variable. For example, if the target in a pursuit tracking task is moving in a sine wave pattern and the controller can perceive this pattern then the controller can control the relationship between the temporal movement patterns of target and cursor. Control (in terms of rms difference between cursor and target) will be better in this case than it is in the case where there is no detectable pattern of target movement; this improvement will be greatest when changes in target position exceed the bandwidth of the system that controls the difference between target and cursor position (keeping it at zero); when the movement pattern of the target is detectable the controller can change the reference for cursor position faster than when the pattern is not detectable (and the relationship between temporal patterns of target and cursor cannot be controlled).

When the controller is controlling the relationship between temporal patterns of target and cursor it seems like the controller is "predicting" future positions of the cursor; but this is not what is actually happening; the controller is varying the cursor reference in a way that maintains the relationship between temporal pattern perceptions.

The fact that it is control of a higher level perception that is occurring, not prediction or estimation based on target position, is evidenced by the fact that when the sine target becomes random, control quickly returns to its original level. If the target were being used as a basis for estimation, the switch to a random target would lead to worse control than would be achieved without these"estimates". In fact, when target movement becomes random, the controller just stops controlling the relationship between temporal patterns because there is suddenly no perceptual variable to control (just as the subject would stop tracking the target if the target suddenly disappeared ;there is suddenly no perceptual variable to control).

>It's interesting that even the simple act of dropping a book into >someone's hand or lifting a weight cannot be truly adequately modeled >without including the effect of anticipatory cues in the simulation.

We don't know that this is true yet. Wait until we do the simulations; then we'll see how important those "anticipatory cues" might be.

Best Rick

Date: Sat Apr 08, 1995 4:53 am PST
Subject: Economic conflicts

[From Rick Marken (950407.1245)]

In celebration of the new Republican Congress's first 100 days of efforts to protect wealthy people from the outstrethced hands of the unwashed masses I am reposting a three year old Bill Powers post that seems to be relevant. I enjoyed this post enormously when it was posted and I like it even more now. Perhaps it can serve to catalyze some new PCT based discussions of economics.

Best Rick

[From Bill Powers (920601.2000)]

to Greg Williams (920601) --

Good point about the reference levels. But I think there's a deeper glitch in the economy than just oil prices.

The problem is that there's a basic conflict between consumers and producers -- the same one that communism tried and failed to resolve. It hasn't gone away. The split between wage income and capital income in the for-profit sector (government is not-for-profit) is about 40/60 -- 40 percent for labor, 60 percent for owners, stockholders, debtholders, etc. This has been pretty close to the ratio since 1930, with the capital-income share having risen slowly from about 53 percent in 1930 to today's approximately 60 percent. The conflict is that receivers of capital income want their share to increase, while wage-earners want it to decrease.

The composite consumer (not the producer) has the reference level of improving the standard of living. This means working fewer hours to obtain ever-better goods and services, or even just to be working and eating instead of not working and not eating. The idea is that technology or ingenuity -- increased productivity -- should be rewarded by obtaining a better life with less prolonged, unpleasant, boring, dangerous, unremunerative, or mind-numbing labor.

The composite producer (with bean-counters in charge) has the reference level of maximizing the return on investment for the owners of the means of production, or those who have invested in it. This means cutting costs wherever possible and charging the most the market will bear for the lowest quality goods or services that can consistently be sold. Cutting costs means, in large part, reducing the cost of labor. When you reflect that cutting material costs is also cutting costs of labor (on someone else's part), it all comes down to cutting labor costs -- if capital income isn't to decrease.

The kicker is that the wage-earners who produce the products have no way of buying the products except with the money they are paid in wages. So if costs are cut by laying people off, substituting cheaper overseas labor, or reducing

domestic wages, the result in all cases is that the buying power of the consumers is reduced -- so the goods and services can't be sold at higher or even the same prices, in the same volume. This is where the conflict comes to a focus.

Unfortunately, this system doesn't have any natural reference levels in the middle of its range of operation -- it just has limits. It always tends toward the state where some large number of people is existing at a subsistence level. The only thing that keeps the composite producer from reducing labor costs any further is the fact that a lot more people would begin dying of starvation or untreated illness or would have their physical living conditions reduced to an intolerable state. The result would be an explosion of crime, or revolution. So a balance is reached where the deleterious effects of further reductions in consumer buying power will increase costs (through taxes for welfare) and reduce sales (through loss of buying power) unacceptably. Government tries to alleviate this situation through redistribution -- spending tax money in ways that increases the slice of the wage-earner or dependent. But the composite producer has no such motive, except when so many people become impoverished that the market begins to fall off.

The government and private philanthropies together manage to acquire enough money from the composite consumer to bring the fraction of capital income down to about 40 percent by redistributing income. Evidently, this is the fraction at which the wage-earning or seeking population has to be maintained even to keep the economy in its current state. If there were no redistribution, there is no way that capital income could remain at 60 percent of the total without creating a violent rebellion by starving people.

People talk in the same breath about our prosperity reaching new highs, if more slowly nowadays, and about the increasing split between high-income people and low-income people. The high-income people are also the chief recipients of capital income. They form the high end of the market. So companies who see sales falling off try to aim for the people who have the money: they produce luxury services, labor-saving items and toys, high-tech or disposable goodies, that will attract the small fraction of the population that has the most money to spend. The result, of course, is that the people at the low end find fewer and fewer items they can afford to buy. The people who CAN maintain their 1970 standard of living work like hell to do so (to get to your point). But just in working like hell to do so, they've sunk below that standard of living. And of course, there are far more people who can't get or handle two jobs, who work less than they used to or at lower wages, and are having a more miserable time than ever.

I think that the owners and managers of this economy need a visit from Ed Ford. Somebody has to ask them, "Is it working?" The problem is that their answer is really "yes" -- so far, it's working for them. A CEO earning $3 million per year plus perks can't really complain. But the SYSTEM CONCEPT isn't working for the people who actually make the system go. It's only working for those who own the system or hold its debts.

There is something drastically missing from the hallowed concept of free enterprise. It's keeping the people whom the economy is supposed to serve in the condition of Skinner's rats. This is something that I think control theorists need to be talking about.

Best, Bill P.

Date: Sat Apr 08, 1995 4:59 am PST
Subject: Re: Controlling people, Accuracy-control

[From Oded Maler (950407)]

>[Rick Marken (950407.0830)]
>
>Bill Powers (950406.1030 MST) --
>
>>When people object to control, they are actually objecting for all the
>>right reasons. What they have to learn is that _everyone_ controls, ALL
>>OF THE TIME. The only problem is when you try to control someone else
>>who is also controlling, and in such a way as to thwart the other
>>person's ability to control.
>
>This paragraph is a perfect summary of the book I've been trying to >write for the last 5 years.

[...]

>And that was going to be the conclusion of the book; we have to face up >to the fact that we are controllers and we have to learn to recognize >when we are controlling. When we can look at our own controlling >"objectively" we have gone up a level -- and this is the only way we can >"get past" the kind of controlling the produces inter- and >intra-personal conflict.

I guess you will be (perceived as) a completely different person after you read that book..

--Oded

Date: Sat Apr 08, 1995 6:31 am PST
Subject: Re: Logical control

[From Bruce Abbott (950407.1330 EST)]

>Rick Marken (950406.2015) --

>I did spend a few minutes playing with the PC Turbo programs the
>other day. All I need to do is add the option of having no disturbance,
>a square wave disturbance (that moves the cursor to the target position
>on some occasions) or the regular disturbance...

Great! Perhaps I can save you a little effort. Here's the code I use to generate the random, square-wave, and sine-wave disturbances:

```
procedure InitDist(DistType: integer; dist: dataptr);

  procedure MaxRange;
  var
    i: integer;
    max, tmp: real;
  begin
```

```
    max := 0;
    for i := 1 to MAXDATA do if abs(dist^[i]) > max
      then max := abs(dist^[i]);
    for i := 1 to MAXDATA do  { normalize to max of 120 }
      begin
        tmp := MaxD/max;
        dist^[i] := round(dist^[i]*tmp);
      end;
  end;
  procedure RandomDist;
  var
    i: integer;
    d1, d2, d3, avg: real;
  begin
    d1 := 0.0; d2 := 0.0; d3 := 0.0;
    for i := 1 to MAXDATA do
      begin
        d1 := random * 10000.0 - 5000.0;
        d2 := d2 + slow*(d1 - d2);
        d3 := d3 + slow*(d2 - d3);
        dist^[i] := round(d3);
      end;
    avg := 0.0;
    for i := 1 to MAXDATA do avg := avg + dist^[i];
    avg := avg/MAXDATA;
    for i := 1 to MAXDATA do dist^[i] := dist^[i] - round(avg);
  end;
  procedure SineDist;
  var
    i: integer;
    k, rad: real;
  begin
    k := 2.0*PI/period;
    for i := 1 to MAXDATA do
      begin
        rad := k*i;
        dist^[i] := round(sin(rad)*MaxD);
      end;
  end;
  procedure StepDist;
  var
    i: integer;
    k, rad, x: real;
  begin
    k := 2.0*PI/period;
    for i := 1 to MAXDATA do
      begin
        rad := k*i;
        if sin(rad) > 0.0 then x := MaxD else x := -MaxD;
        dist^[i] := round(x);
      end;
  end;
begin
  Case DistType of
    0: RandomDist;
```

```
    1: SineDist;
    2: StepDist;
  end;
end;
```

>By the way, I ran my logical variable control model in the three >disturbance conditions in which I tested the (sub?) human subject (me) >and found the same results for the model and person. For the model (as >for the person) when there is no distrubance to the mouse, the >correlation between stimuli and responses is nearly 1.0; the model acts >as though it's responses are "controlled" by the stimuli. When the step >disturbance is added, the stimulus-response correlation goes down to >near 0.0. With the regular disturbance the stimulus-response correlation >is about .5. In all cases, of course, the proportion of time that the >logical variable is in its reference state ("true") is near 1.0. So, >like the person, the model controls the logical variable but, in doing >so, it acts as though stimuli are controlling its responses when there >is no disturbance to these responses.

Exactly as expected. But is the pigeon in the operant analog of this situation really trying to keep a logical variable true? What other perceptions might be controlled which would give the same appearances? Ultimately, of course, the pigeon is really trying to maximize the rate of food access; how does this goal translate into controlling other variables given the imposed schedule? What I'm suggesting is that we need several alternative models we can evaluate using "the Test."

I'm looking forward to your post of the completed task and analysis program(s).

Regards, Bruce

Date: Sat Apr 08, 1995 7:36 am PST
Subject: Re: Controlling people, Accuracy-control

[Lars Christian Smith (040895 12:30 CET)]

To: Rick Marken

Re: Controlling People

I just read Jerome Bruner's _Acts of Meaning_. Nice critique of cognitive psychology, and of the damage the computational metaphor has done.

Bruner writes (p. 119) "The range of what people include under the influence of their own agentivity will, as we know from studies of "locus of control," vary from person to person and, as we also know, vary within one's felt position within the culture."

In the footnote he writes: "See, for example, Ellen Langer, _The Psychology of Control_ (New York: Sage, 1983)."

What is this "locus of control" stuff? What is the conceptual framework?

Best, Lars

Date: Sat Apr 08, 1995 9:26 am PST
Subject: Re: Anticipation

[From Bruce Buchanan (950408.11:00 EDT)]

Rick Marken (950407.1450) writes:

in response to Bruce Abbott (950407.1025 EST) -

>>It's interesting that even the simple act of dropping a book into >>someone's hand or lifting a weight cannot be truly adequately modeled >>without including the effect of anticipatory cues in the simulation.

>We don't know that this is true yet. Wait until we do the simulations; >then we'll see how important those "anticipatory cues" might be.

Pardon me, Rick, but isn't it absurd to put more weigh on arbitrarily specific simulations than on a varied range of observations of nature ?

I sometimes get the impression that Rick's comments are very much off-the-cuff, and do not always do justice to the postings he purports to discuss - I say "purports" because my impression is that he is often really discussing his own perceptions. As far as I can tell PCT does not preclude attempts at more carefully considered communication.

Two other cases in point from the same posting - (this is also worth pursuing because of the importance of the questions related to anticipatory clues...)

(Bruce A.)
>>This again reveals an anticipatory action that depends, not on current
>>feedback from the low-level sensors, but on predictive cues whose
>>significance depends on prior experience.

>Actions never really "depend on feedback" (this phrase implies that >feedback is a perception caused by action; but in a control system, >perception is always the combined result of action and disturbance; it >is impossible for a control system to extract just the "feedback" >component of perception, and it doesn't need to, anyway). Actions depend >on the continuously varying discrepency between perceptual and reference >signals in a negative feedback loop.

My reaction to this comment was that Rick has assigned a specific interpretation to the phrase "depend on feedback", to which he objects, and then reiterates standard PCT (with which no one is likely to disagree), which is a different point. I did not think Rick's interpretation was implied in the original statement by Bruce Abbott (although only Bruce A. will know what he intended.)

Rick says:
>"Predictive cues" is one of those phrases that suggests that stimuli can >do far more than they can actually do. I say we throw it in the trash >barrel...

Again, according to who's interpretation ? Later on Rick grants some predictive usefulness to observed trends in tracking experiments, so it is evident that he thinks that *the concept itself* is not without application.

It seems to me that everything that living organisms do involves them with conditions that are in the future at the instant that action is begun, and that any and every clue concerning changes and trends are necessarily of great importance, worth attention at higher levels (as suggested by Bruce A.) Many of our concerns with the past involve an orientation to learn lessons for future guidance, i.e. to pick up clues to better predict and manage trends. IMHO the note Rick also posted by Bill Powers (920601) on Economic Conflicts concerned relatively high level systems analysis and anticipations of this type.

While taken at face value some of Rick's statements do not seem responsive, it is also possible that I am misunderstanding him. At the least, for me, there is a communications problem. And in view of Rick's demonstrated intelligence, talents, tenacity and intellectual leadership role it may be worth comment.

But perhaps I should simply believe Rick when he says, in effect, that he does need to be curbed in his enthusiasms at times. And if I am away off the reservation I also hope someone will feel free to point that out!

Cheers and best wishes.

Bruce B.

Date: Sat Apr 08, 1995 9:50 am PST
Subject: Re: anticipation

[From Bill Powers (950408.0715 MST)]

Rick Marken (905047) --

Bruce Abbott (950407) --

The discussion of anticipatory behavior is bringing us close to the core of the difference between EAB and PCT. As Rick pointed out, there are certain assumptions being made that need to be tested in simulation and experimentation, and we need to do that testing. I think some surprises are in store for more than one person on the net. I'm thinking at the moment of the idea that when disturbances are regular, we can control over a wider bandwidth because we can anticipate what the disturbance or target is going to do. I'll deal with that, then get closer to the point.

In fact, I suspect that control that is done by anticipating or matching regular patterns of disturbance is _slower_ than present-time or non-anticipatory control, if we make the criterion an equal ability to maintain a small error under similar conditions. The idea that we can control better when there is a regular pattern comes, I think, from making an invalid comparison.

Suppose we present a random disturbance with a certain upper bandwidth. By picking a bandwidth that is just high enough, we can make the person's error some standard amount, say 5% of the peak-to-peak disturbance amplitude. Now we switch to a regular sine-wave disturbance with a frequency somewhat higher than the upper bandwidth limit of the random disturbance. We find that the person can maintain control at a somewhat higher maximum frequency than before. From this we could get the idea that pattern-matching control is faster in general than moment-by-moment control.

But this comparison is not valid. To make a valid comparison, we would have to compare the behavior of a moment-by-moment (MBM) system with that of a pattern-matching (PM) system under a disturbance applied to the same starting condition.

Suppose we pick a sine-wave frequency well within the bandwidth of the MBM system, and apply a small step-change in frequency, phase, amplitude, or mean value. Which type of system, MBM or PM, will correct the error faster? Obviously, it will be the MBM system. The PM system can't detect a change in the more global variables in less than one full cycle of the sine-wave, whereas the MBM system, which knows nothing of waveforms, simple corrects the error immediately.

The pattern-matching system actually has a much narrower bandwidth than the MBM system, although the center of its bandwidth can be at a higher frequency. The PM system is designed to control the match between regular recurring patterns, but it can correct errors only slowly.

That was aimed more toward Martin Taylor than Bruce Abbott.

Now let's consider the question of the "ability to vary output so as to begin to counter _expected_ disturbances" (Abbott, 950407.1025).

Look at the situation in SDTEST3, where on signal a person moves the cursor from one target to the other. It's easy to look at what is happening on the screen, and unconsciously identify the behavior of the cursor with the "outputs" of the person doing the tracking. But as we know, the cursor behavior is NOT a measure of the person's output; the mouse position is the person's output (in this situation). The signal does not say "Move the mouse to a certain new position." It says "Make the cursor move to a certain new position."

As Rick has been trying to illustrate, what the mouse must do depends not just on the signal, but on the disturbance that is inserted (invisibly) between the mouse and its effect on the cursor. If we cleverly insert disturbances of the right kinds at the right times, we can create any relationship between the signal and the following mouse movements that we like, without altering the relationship between the signal and the ensuing cursor movements. As the cursor is moving toward the new target, we can arrange for the mouse to be moving in either direction and at any speed. So clearly the signal is not determining the mouse movements, the actual outputs of the person.

The problem becomes clearer when we slow down the action. Suppose you look out the window and see that it's going to rain. So before it rains, you move a bucket to the place on the floor where it will catch drips from a leak in the roof. According to the EAB interpretation, the sight of a gloomy sky is a stimulus that results in the behavior of moving the bucket to the right place, in anticipation of the unwanted event.

The subtle point that so easily escapes notice is that moving the bucket

to the required place IS NOT A BEHAVIOR. It is a CONSEQUENCE of motor actions and all other enviromental causes that can influence or already have influenced the position of the bucket. The actual behavior, the motor output that is generated, depends on what other influences are acting or have acted on the bucket. If the bucket is currently in use to hold anti-freeze, the first "behavior" apparently elicited by the sight of the gloomy sky is to search through closets for a plastic bottle to hold the antifreeze. If a child is playing with the bucket, the first behavior elicited by the sight of the sky is somehow to negotiate or wrestle for possession of the bucket. And the rest of the behavior depends entirely on where the bucket _is_ relative to where you _want it to be_.

So even though the bucket does move to the required place after the gloomy sky is noticed, there is absolutely no way to predict what actual behavior will be involved in creating this final result. There may be no behavior at all, if the bucket is already in the right place.

Now, how could anyone get the idea that the sight of the gloomy sky is a stimulus that elicits a particular behavior that has the consequence of moving the bucket to where it will prevent the punishing experience of a wet rug? This belief can easily arise; it's the same problem that arises with any superstition. If there is a sacred bucket that nobody is allowed to touch and that is always kept in exactly the same place, then moving it to the sacred spot under the drip will require the same actions in the same direction every time it is moved. No child will ever be playing with it; it will never be full of anything; it will never be buried under other objects. The behavior will become even more predictable if a bucket-minder is hired who stands every day beside the bucket, at attention, looking out a window at the sky. Now when the sky reaches the state that is recognized as "rain coming," the bucket-minder will stoop, grasp the handle, straighten up, march five paces to the NNW, stoop, release the handle, and return to his or her post. Now it will be perfectly clear that the sight of a rain-predicting sky will elicit a particular sequence of behaviors that ends with the bucket in the right spot.

In other words, if you standardize the conditions so that only one behavior or behavior sequence can end in the right consequence, then a plausible explanation of what happens is that the stimulus causes that behavior or behavior sequence and hence that consequence.

Of course standardizing the conditions is the best way to avoid seeing that this explanation is wrong. In effect, the conditions are arranged to fit the explanation. Then in order to demonstrate that the explanation is correct, the same conditions are carefully reproduced.

This is the basis of superstition; you don't dare vary anything because you don't know what matters, or because you have an explanation implying that EVERYTHING matters.

What Rick has been trying to do is to vary the conditions in a way that shows at least some of what does and does not matter. It is clearly not necessary, in SDTEST3, to make sure that the mouse always affects the cursor in the same way. And once we have seen that, we can no longer believe that the signalling stimulus is the signal for a particular _behavior_. All we can now say is that the stimulus signals that some

other stimulus should change to a new state; the signal does not specify what should be done to bring the other stimulus, the cursor position, to the new state.

If we fire the bucket-minder and return the bucket to its normal status as a household tool that can be used for anything, we can no longer suppose that the sight of a gloomy sky is a stimulus that produces a particular behavior that will move the bucket to the required position. There is no longer any such behavior. The stimulus is not followed by any particular behavior, although it is always followed by a movement of the bucket from the state it is in, empty or full, wherever located, to a new state of being empty and upright in a particular position.

We can see, in fact, that something quite different from behavior is being controlled. It is, perhaps, a sequence that begins with a gloomy sky and ends with the bucket in a particular place. Or perhaps it is a logical condition: it is not the case that the sky is gloomy and the bucket is not on its special spot. We can also see that the controller is something other than the sight of the sky: the controller is the person who maintains the sequence or the proposition in a state that matters to the controller. One component of the controlled perception is produced by an environmental variable that the person can't influence, the way the sky looks. But the other main component, the position of the bucket, can easily be influenced by the person. As a result, the function of the two variables can be controlled by the person.

Rick is applying the Test for the controlled variable. To see whether the person's behavior is under control by anything, the experimenter applies a disturbance to the system and sees whether the supposed controller varies its action in such a way as to protect the proposed controlled variable from the effects of the disturbance. In this case we get two answers: No, the behavior is not controlled, and NO, the supposed controlling stimulus does not vary its action to oppose the effect of the disturbance. The same Test proves that the controlling stimulus does not control the cursor position, and shows that the behaving person is the controller, the means being variation of mouse position.

I would go on here to say that the experimental manipulations contradict the predictions of a model that gives either the stimulus or the consequence of moving the cursor to the target any control over behavior. But I can't do that, because the stimulus-control model is not used to make predictions in any consistent way. Strictly speaking, the stimulus-control model predicts that every time the stimulus occurs, the behavior of moving the mouse should take place, and take place in the same way. But the S-C model is not set up in such a rigorous way that such a test would be admitted as valid. Like most unfounded beliefs, it is equipped with many escape hatches and loopholes, so that if the predicted effect doesn't occur, one can claim that this non-occurrance was also expected -- it is not, after all, possible to notice every stimulus that is impinging on an organism. So if the prediction fails, that is not because the S-C model is wrong, but because some unknown extraneous stimulus caused a behavior other than the expected one. God moves in mysterious ways.

Best, Bill P.

Date: Sat Apr 08, 1995 9:54 am PST
Subject: The feeling of control

from Mary Powers 950408

Lars Christian Smith:

I don't remember much about Ellen Langer's book, which I read because when Bill Glasser wrote "Take effective control of your life" (1984) he indicated that her work supported his, while Bill Powers' was "highly theoretical" (i.e. too hard).

Langer did some studies that indicated that when elderly folks were given more control over their lives they were healthier and happier. What struck me when I read it (in my usual biased way) was the notion of "giving" people control, rather than having previously prevented them from having it; and the idea that what was being given was the "feeling" of being in control, with the implication that control, as merely a feeling, is an illusion.

I'll go back and read it again, but I suspect that she, like Glasser, thinks that what is controlled is behavior. Bruner too, if he uses words like "agentivity".

Mary P.

Date: Sat Apr 08, 1995 11:21 am PST
Subject: Misc Replies

[From Rick Marken (950408.1100)]

Oded Maler (950407) --

>I guess you will be (perceived as) a completely different person
>after you read that book.

As Paul McCartney once said when asked about rumors of his death "I'd be the last to know";-)

Oded Maler (950407) --

>I would call it ("controlling for accuracy") as a futile attempt to
>create in the minds of others the *exact* perceptual variables that the
>"oldie" has.

If "controlling for accuracy" were really futile than how would we teach algebra, chemistry or dentistry? You can't guarantee that people will get PCT right, but you can sure tell when they _are_ getting it right and you can try to help them move in the right direction when they are clearly not getting it right. People can learn PCT just as they can learn algebra. Of course, they won't learn it if they don't want to.

Many people do _not_ want to learn PCT (at least, unconsciously) because many tenets of PCT conflict with other beliefs that are important to them (such as the belief that psychological research can't be _completely_ off base). There is certainly nothing wrong with not wanting to learn PCT. But when these people start to teach a distorted version of PCT -- one that it is consistent with their existing beliefs -- then you will suddenly see a whole lotta "controlling for accuracy" goin' on. This happens because we are in a learning community: "the audience is listening" and many of them might be willing to learn PCT.

Lars Christian Smith (040895 12:30 CET) --

>What is this "locus of control" stuff? What is the conceptual
>framework?

I think the "locus of control" stuff is basically "personality" research, based on the idea that some people tend to attribute control of their behavior to themselves ("internal locus of control") and others tend to attribute control to the environment ("external locus of control"). The conceptual framework would probably be called "cognitive" (it's about what people _think_ is going on) though I would call it "confused".

The work on "locus of control" does not deal directly with control as we understand it. The researchers (as I recall) don't take a position (as we do) regarding the actual locus of control (internal, of course) nor do they explain (as we can) why people occasionally believe that the locus of control is external when it is not (this happens, of course, when people 1) lose control due to conflict or lack of skill 2) ignore the controlled variable and notice only response to disturbance or to changes in the feedback function or 3) become research psychologists;-))

Bruce Abbott (950407.1330 EST) --

Thanks for the disturbance code.

>But is the pigeon in the operant analog of this situation really trying
>to keep a logical variable true?

Now THAT's a good PCT question. I think so, but this would have to be tested (using The Test, of course). I don't think it is at all surprising that a pigeon would be controlling a logical variable; the pigeon doesn't have to be George Boole to do this. A very simple neural circuit can compute a logical variable. But I agree with your question 100%. What is the variable(s) the pigeon is controlling? This is the kind of research EAB people _should_ be doing; testing to see what kind of perceptual variables organisms can (and DO) control. Unfortunately, EAB people seem far more interested in doing the easy stuff -- poking at organisms and measuing irrelevant side effects of these disturbances. Ah well.

Best   Rick

Date: Sat Apr 08, 1995 1:19 pm PST
Subject: ...Is Making Me Wait

[From Bruce Abbott (950408.1420 EST)]

>Bill Powers (950408.0715 MST) --

>Now let's consider the question of the "ability to vary output so as to
>begin to counter _expected_ disturbances" (Abbott, 950407.1025).

What follows is highly misleading, as it (a) fails, so far as I can tell, to address the question said to be under consideration and (b) offers a discussion which would seem to be arguing against something I said while in fact providing an analysis with which I agree completely. For the record:

1. I developed SDTEST3 in order to provide data for a PCT model that can explain the phenomenon known as "stimulus control" while also identifying conditions under which stimulus control should fail despite contrary predictions by traditional reinforcement theory.

2. Rick's findings derived from applying a version of the Test to the SDTEST3 situation are those I expected based on my own analysis of the situation.

3. Behavior analysts would never think of using a mentalistic construct like "anticipation" to describe the phenomenon of stimulus control. Consequently, using the SDTEST3 situation to exemplify a cue having an anticipatory function in a discussion of EAB versus PCT is more than a bit ironic.

Somehow my post appears to have set the occasion for a discussion of EAB, stimulus control, and Rick's application of the Test to determine what perceptions are being controlled in the SDTEST3 situation, I hope for the elucidation of others who may be following along rather than as an attempt to help me reach an understanding I already possess. But it has little to do with the discussion to which it purports to be a reply.

I'd say it was an example of stimulus control if I could just identify the stimulus that elicited this response. Was it something I said? (;->

Getting back to the neglected topic of anticipation, here are two questions to ponder: (1) What do you suppose happens when you ask someone to hold out his or her hand, palm up, and then make like you are dropping a heavy object into the palm, but actually catch the object just before it strikes the palm? (2) Why?

Regards, Bruce

Date: Sat Apr 08, 1995 3:44 pm PST
Subject: Re: anticipation

[From Bill Powers (950408.1510 MST)]

Bruce Abbott (950408.1420 EST) --

> Somehow my post appears to have set the occasion for a discussion of EAB, stimulus control, and Rick's application of the Test to determine what perceptions are being controlled in the SDTEST3

situation, I hope for the elucidation of others who may be following along rather than as an attempt to help me reach an understanding I already possess. But it has little to do with the discussion to which it purports to be a reply.

I think we're having a perceptual problem here. I incline to the view that you have a rather thorough understanding of the PCT framework and a rapidly advancing skill in constructing PCT models. But sometimes you seem to be talking in a different language. So I teeter back and forth between thinking you're working out a translation from PCT to EAB and so on, and thinking that in the back of your mind you still think that there is something legitimate about terms like "stimulus control." I hope you will forgive my suspicious nature; it's the result of sad experience, even if it's unfair to you.

Getting back to the neglected topic of anticipation, here are two questions to ponder: (1) What do you suppose happens when you ask someone to hold out his or her hand, palm up, and then make like you are dropping a heavy object into the palm, but actually catch the object just before it strikes the palm? (2) Why?

I suppose that at the expected moment of impact, the hand may move upward (it may not). One critical factor, I should think, is what the person trying to catch the object is trying to do. If the objective is to catch the object comfortably, then the hand may well not move up (When you try to catch a foul ball at a hardball game, barehanded, you try to have your hand moving in the same direction as the ball at the moment of impact, to minimize the sting). But if for some reason the catcher doesn't want the hand to be deflected downward, the only way to prevent that is to increase the upward force just as the object is projected to hit the hand, and by a sufficient amount.

This is very difficult to time; you have to estimate the moment of impact and increase the reference level for hand position just enough in advance of impact that the muscles will have started reacting when the impact occurs. If you drop the object from a height of 1 foot above the hand, impact will occur 1/4 sec after release. This means that you have to control for essentially simultaneous raising of the hand (or raising of muscle tone) and release of the object, to allow for your own reaction time. So the controlled variable that you would learn after much practice is "release AND tense". The tensing is, of course, a controlled perception, not a controlled action.

When you drop the book onto your other hand yourself, your timing is almost perfect. And anyway, your control systems at the kinesthetic level can be so tight and fast that you don't consciously see any "give" in the receiving hand even when it's there. The spinal loops start reacting in about 9 milliseconds, during which time the book, after dropping 12 inches, would move your hand down about 0.5 inch (after exchange of momentum). Your hand actually gives way more than that (I just tried it).

This needs to be tested, with your eyes open and then with them closed. I don't think you will see a great deal of difference. What we need is a control stick pivoted near the elbow, with a game port to report the result. I'll see what I can kludge up.

Something that may be suggestive: take a look at the velocity signal in the arm model for a step-change in the reference signals. Plot the position signal along with it, and then plot the velocity + position signal. The velocity + position signal looks like an advanced version of the position signal -- the reason you get braking by adding the velocity signal is that the control system perceives the position as being past the reference setting before it has actually got there. So adding a first-derivative component to a perceptual signal has the effect of creating "anticipation." The net position perceptual signal (or error signal) appears to change faster than the variable it represents.

Anticipation in more complex situations may involve what is called "model-based" control. In automatic airplane-landing systems, a computer continuously projects the current position, speed, and rate of sink into a display of an anticipated landing point on a picture of the runway. This model runs much faster than real time, repetitively. The pilot uses the controls to move the projected landing point to the right place on the display and keep it there; there's no need to pay any attention to the current status of the plane. So what looks like controlling a future condition is really done by controlling the outcome of a present-time projective calculation being done over and over.

When events are progressing slowly enough, we can do the same thing in our heads. A ship captain learns certain rules of thumb, an important one being that a collision course is indicated when another ship is continually appearing at the same bearing -- the dreaded "constant-bearing" situation. So the captain can avoid a collision by steering or changing speed so the bearing of the other ship is either increasing or decreasing. In this way what seems like anticipation of a future event is controlled by controlling a strictly present-time perception.

Another example of the phenomenon you're talking about occurs when you put a car into gear and press on the accelerator to start the car moving. What you're controlling is a complex perception made of sounds, efforts, and visual movement. One of the efforts you sense and adjust is the tension in the muscles that keep you in position behind the steering wheel. When, accidentally, you put the car into neutral instead of drive or first gear, when you step on the accelerator the engine revs up but the car doesn't move -- and you lurch forward toward the steering wheel because your abdominal muscles are being told to generate a tension at the same time your foot is supposed to be creating a force on your body moving you backward into the seat. The higher-order control system is sending changing reference signals to several lower-order systems, some of which are supposed to result in canceling forces. But when one of the forces is missing, that is the same as applying a disturbance opposite to the missing force, and there is some movement where there is normally none (in an experienced driver). One of the side-effects of failing to control the higher-level error is that the higher-level system's output becomes much larger than normal; you can redline the engine trying to get the car to move. The same thing happens, only more so, when you unknowingly put the car into reverse instead of drive. In that case the delay of about 400 milliseconds that is required to start reversing a control system can lead to serious accidents.

The point of all this is that I think the subject of anticipation involves considerably more than just responding to a stimulus.

Best, Bill P.

Date: Sat Apr 08, 1995 3:46 pm PST
Subject: Re: Anticipation

[From Bruce Abbott (950408.1805 EST)]

>Rick Marken (950407.1450) --

>>Bruce Abbott (950407.1025 EST) --

>>This again reveals an anticipatory action that depends, not on current
>>feedback from the low-level sensors, but on predictive cues whose
>>significance depends on prior experience.

>Actions never really "depend on feedback" (this phrase implies that >feedback is a perception caused by action; but in a control system, >perception is always the combined result of action and disturbance; it >is impossible for a control system to extract just the "feedback" >component of perception, and it doesn't need to, anyway). Actions depend >on the continuously varying discrepency between perceptual and reference >signals in a negative feedback loop.

O.K., Officer, you got me. How about "This again reveals an anticipatory action that depends, not on the current discrepancy between the perceptual and reference signals, but on predictive cues whose significance depends on prior experience. (?)

>"Predictive cues" is one of those phrases that suggests that stimuli can >do far more than they can actually do. I say we throw it in the trash >barrel along with "stimulus control". "discriminative stimulus", >"reinforcement" and "selection by consequences".

This is a bald assertion rather than an argument. I have no idea why you believe this to be true. Perhaps the predictive cues are inputs to another control system, whose output sets the reference for the lower-level system monitoring the perceptual variable in question whose changes the predictive cue is predicting. Or perhaps, as Bill P. suggested, it comes to function as a source of disturbance to the perception under control. Both of these possibilities are consistent with control theory and both give a role for "predictive cues." Or are you objecting simply because it sounds like animism again, as if the cues are doing the predicting? I'll bet that's it.

>>This ability to vary output so as to begin to counter _expected_
>>disturbances is extremely important for the well-being of most >>organisms.

>This is a myth that probably originated with the "generated output" >cult, particularly the sect that worships "feedforward control". I think >it would be a good idea to build a 'predictive" control system and see >what actually happens. That will be my project for the weekend.

Glad to be keeping you busy ("idle hands," etc.). But if you're serious about this you should be sure to build in some cues that (a) really do predict disturbances which ordinarily would not be countered on an appropriate time-scale in the absence of the predictive cue and (b) are sufficiently reliable in their predictions and properly timed so as to allow appropriate changes in output to occur so as to correct the predicted disturbances. For example, you might program a beep to occur in the SDTEST3 situation, not when a point is earned as currently, but 0.5 sec prior to the change in cursor color and thus the change in designated target. I predict this will eliminate the annoying delay between color change and initiation of the transition to the new target and thus result in improved time-on-target.

In fact, I can't see why you think this result is even at issue. I can see no conceptual difficulty in the notion that predictive cues might act to change perceptual inputs OR reference levels (for lower-level systems) in advance of the disturbance being predicted by the cue, and it seems that common observation supports this notion. When something flys at your face, why do you flinch and raise your arms in defense? If you slip on ice, why do you steel yourself for the impact with the ground? Certainly not because of the sensory effects of the impact! These changes result, of course, from the ordinary operation of a control system, but this system has gotten organized to respond, not to counter the perception it is monitoring, but to counter what reliably follows that perception--the painfulness of a bone-jarring impact.

I am NOT arguing that predictive cues necessarily provide better control over continuously changing perceptual variables than ordinary perception of the variables themselves, as you and Bill seem to believe. The kinds of changes for which predictive cues offer a control advantage are most likely to be those EVENTS for which ordinary changes in output based on CURRENT error would arrive too late to be of much use.

>>In most cases it is better to react unnecessarily to anticipated
>>disturbances (and perhaps risk appearing foolish) than to fail to >>react.

>Well, I think we should test this and see if it's true.

Hey, fire away! I'm an empirical kind-a guy!

>>It's interesting that even the simple act of dropping a book into >>someone's hand or lifting a weight cannot be truly adequately modeled >>without including the effect of anticipatory cues in the simulation.

>We don't know that this is true yet. Wait until we do the simulations; >then we'll see how important those "anticipatory cues" might be.

Good deal, but be sure that the situation meets my requirements as stated above, or I'll argue that the simulation is inadequate for addressing the question.

Regards, Bruce

Date: Sat Apr 08, 1995 4:03 pm PST
Re: Hello darkness my old friend

[Bill Leach 08 Apr 1995 17:25:32]

>Message timestamp: [Susan Schweers 06 Apr 1995 23:10:57 -0400]

>And while I'm annoyed, I would like to post the question: have you
>applied your understanding of The Test to your responses to Newbies
>on the group? I see a rather severe controlling for accuracy to the
>original idea, at the expense of participation by persons who show up
>interested.

I am a relative Newbie as I have only been active for around a year now.

If a person on CSG-L really IS interested in PCT and wants to learn something about PCT then accuracy is essential. In my short time of watching and participating in the net I have observed:

"People insisting that "so and so's" school or theory encompasses PCT many times."

"PCT is "really such and such" but just using different terms."

"PCT must have "xyz" added to explain "abc" phenomenon."

"Oh this is great! Just what I have been looking for... A formal explaination for how perception controls behaviours -- wonderful."

"PCT does not deal with the 'big questions'."

It appears to be virtually certain that the ONLY theory that encompasses PCT is Control Theory.

All of the theories that "seems as though they are 'like' PCT" differ at their most fundamental level -- the level that few of their practictioners consider.

The vast majority of the 'phenomenon' that I have heard proposed as examples of "things needing an explaination in PCT" either are explained in PCT or are shown as irrelevent to understanding behaviour.

Then there is a large group that "decides that they understand PCT" and "use it" to justify their existing beliefs concerning behaviour. Thus, they 'banter about' PCT sounding phrases to explain examples of phenomenon that ARE NOT examples of a control system in operation.

Finally, there has been a large number of people that wanted to use PCT to explain or justify some philosophy or philosophical position. They have generally either decided that PCT supported their position (ignoring statements to the contrary) or just quietly dissappeared.

I personally realize now that the long time PCTers have been seeing these and variations for many years. While it is easy to criticize the "hard core" PCTers for being 'unyielding' with Newbies it is also not too hard to recognize that the 'old timers' have seen the same distortions, the same missing of the essential points thousands of times.

What understanding that I have is directly due to the unwavering dedication to exacting expression (primarily Rick Marken). He almost hounded me on point after nitpicking point but it is precisely his determination that I not get by with sloppy terminology and loose meaning that helped me.

The essential concept of PCT is so simple (at least to someone already familiar with engineered control systems theory) that it nearly seems silly -- at first.

The essence is that you perceive (consciously or unconsciously). Some perceptions have an INTERNAL reference value (a 'desired' state or level). Your behaviour IS caused by the existance of a difference between the reference and the perception. Exactly WHAT your behaviour may consist of is a function of what you have learned will reduce the error or even random attempts at control to reduce the error.

Some of the more 'shocking' ideas that 'fall out of PCT' (though not necessarily uniquely):

You control (or at least attempt to control) what YOU perceive and not necessarily "what really is".

You can not actually KNOW what "really is".

Everyone of us IS a control system including the "dispassionate observer".

"Groups" do not act, only individuals act.

We will quite literally defy all logic and rational thinking in support of a very strongly held belief.

PCT is morally neutral, it does not support or deny any particular belief system.

Most of the "Big Questions" are irrelevent to behaviour but PCT usually identifies complete sets of issues that few will recognize as central to the so called Big Question.

-bill

Date: Sat Apr 08, 1995 7:30 pm PST
Subject: Re: Anticipation

[From Rick Marken (950408.2015)]

Bruce Buchanan (950408.11:00 EDT) --

>Pardon me, Rick, but isn't it absurd to put more weigh on arbitrarily
>specific simulations than on a varied range of observations of
>nature ?

Anticipation is not an observation; it is an interpretation of an observation. The simulations I was discussing (which will not be as straightforward as I thought; there are many ways to "anticipate") provide observations of what happens to a control systems when a particular "anticipation" algorithm is added to its operation. Such simulations also help clarify what we mean by terms like "anticipation".

>I sometimes get the impression that Rick's comments are very much

>off-the-cuff, and do not always do justice to the postings he purports
>to discuss - I say "purports" because my impression is that he is often
>really discussing his own perceptions.

My commenst are not "off-the-cuff"; they are carefully thought out, believe it or not. You are seeing the best I can do. I do discuss my own perceptions because they are the only ones I have. I really am sorry if I don't seem to be doing justice to the posts I answer. Perhaps it would be better if I just agreed with them but that would be no fun for me.

>Later on Rick grants some predictive usefulness to observed trends in
>tracking experiments

I don't think I did. What I meant was this: if there is a perceptible temporal pattern then one can control a relationship between that pattern and another (produced by one's own actions); there is no prediction involved. Prediction (to me) means estimating the future value of a variable based on past and current values. This kind of prediction can be done (sometimes successfully); but it is not clear that it is a "useful" aspect of control. You don't need to be able to predict the future value of a variable in order to control it; and it seems to me than under most normal circumstances such prediction would tend to make control worse (but the simulation should tell us whether or not this is the case).

>It seems to me that everything that living organisms do involves
>them with conditions that are in the future at the instant that action
>is begun,

I'll buy that.

>and that any and every clue concerning changes and trends are
>necessarily of great importance, worth attention at higher levels (as
>suggested by Bruce A.)

If you mean that such clues are important for control because they allow prediction of future values of the controlled variable then I argue (and I believe that simulation will prove) that this is not so. If, however, you mean that there is value in detecting higher order patterns in the behavior of lower level variables, then this is probably true -- and it's probably why we have evolved the ability to perceive the world in terms of these higher level variables.

I think what is not being understood here is the difference between control based on prediction of the future state of a variable and control OF a higher level aspect of a variable.

Control based on prediction of the future state of a variable would allow a variable representing, say, the predicted future error signal, to be used to drive (or to contribute to driving -- these are the implementation problems I'm having) the output of a control system; so the actual output "anticipates" what the controlled variable might do, and (presumably) prevents it from doing it.

Control OF a higher level aspect of a variable is just the usual kind of control but now the perceptual signal represents the state of the higher level variable -- such as the amplitude of a harmonic component of variations in the lower level variable. If the lower level variable happens to vary as a sine wave, the perceptual signal will vary in amplitude as the frequency of the sine wave varies. No anticipation is involved in control of a higher level variable; just control of a variable that is defined over a wider "time window".

>While taken at face value some of Rick's statements do not seem
>responsive,

I can understand how you feel; I often do leave things out of my answers that I think people already know or have available to them in other PCT sources, notably B:CP, Intro to Moden Psychology and Mind Readings. But this is supposed to be a scientific dialog. If you think I'm leaving something out or being misleading or tricky or whatever just point out where you think I'm doing this; I'll try to clear it up. I'm trying to teach PCT, not prepare a legal brief for it.

>it is also possible that I am misunderstanding him.

The more likely problem is that you are NOT misunderstanding me at all;-)

Best Rick

Date: Sat Apr 08, 1995 9:15 pm PST
Subject: Re: The feeling of control

<[Bill Leach 950409.00:51 U.S. Eastern Time Zone]
>Mary Powers 950408

>Langer did some studies that indicated that when elderly folks
>were given more control over their lives they were healthier and
>happier. What struck me when I read it (in my usual biased way)
>was the notion of "giving " people control, rather than having
>previously prevented them from having it; and the idea that what
>was being given was the "feeling" of being in control, with the
>implication that control, as merely a feeling, is an illusion.

This is a good analysis of the problem with such work. The "Pop Science" folks have it partly right but unfortunately miss the essentials. Their failure to recognize the control system nature of people and most importantly the implications such a nature leads them to make many "almost right" statements.

People DO feel good when they control well. They use all kinds of terms; happy, successful, "in control" and the like.

When they don't control well they also use lots of terms; unhappy, a failure, "out of control and the like.

I think that there is a similarity between the "Langer's" and the people that that Ed Ford works with. He at least is trying, I believe, to help people recognize that they are not 'giving people (children) control' but rather are giving up the absurd concept that the authority even can control others. At 'best' they can thwart (in some ill defined manner) the control efforts of the students.

The less of that sort of thing that they do and the greater their effort in establishing a consistant environment with maximum freedom for control the more successful will be their program. I suspect that there will always be people that will not be willing to submit to any restrictions on their behaviour, the percentage of "problem kids" will be small if the restrictions make sense to them personally and are clearly not arbitrary.

-bill

Date: Sat Apr 08, 1995 9:15 pm PST
Subject: Re: Anticipation

<[Bill Leach 950408.20:52 U.S. Eastern Time Zone]
>[Rick Marken (950407.1450)]

>Actions never really "depend on feedback" (this phrase implies that
>feedback is a perception caused by action; but in a control system,
>perception is always the combined result of action and disturbance; it
>is impossible for a control system to extract just the "feedback"
>component of perception, and it doesn't need to, anyway). Actions depend
>on the continuously varying discrepency between perceptual and reference
>signals in a negative feedback loop.

I don't think that you are saying this right. The perception IS the feedback for a controlled perception. The organism can not seperate the components making up the feedback AT ALL as long as control is 'good' (and poorly if control is not good).

This "Predictive cues" is indeed an interesting issue. I certainly would like to listen to an explaination.

I am not trying to posit that this is a "generated output" issue and indeed it don't think that it is but I do suspect that there is some 'selection' of either an output control function or at least the parameters for one based upon both the current perception (ie: Empty Milk carton on counter that one wishes to lift) and ...

I just recognized that the "and" is a current perception... that is the erronous perception that the carton is full.

Thus what we may be talking about here is mearly the action of learned control system operation. We "give it a name" because such control system errors startle us.

We can call it "anticipatory" I suppose but the catching of an object or indeed picking up an object involves a lot of perceptions. Picking up a lot of heavy boxes and then a light one (or vice versa) is, of course, a perceptual control activity.

I believe that in tracking tasks (certainly in some of the PCT demos), it has already been demonstrated that when a control task has been controlling and a change (disturbance) occurs that overwhelms the active control loop, a delay occurs as higher level control tasks process the resulting error.

I don't see this "anticipatory control" as anything more or different than one person "following" the finger movements of another when the other uses consistent patterns but changes the specific pattern periodically.

If I am missing something significant here I would appreciate someone pointing out just what that something might be.

-bill

Date: Sat Apr 08, 1995 9:16 pm PST
Subject: Re: Anticipation

<[Bill Leach 950408.23:47 U.S. Eastern Time Zone]
>[Bruce Buchanan (950408.11:00 EDT)]

Bruce A.:
It's interesting that even the simple act of dropping a book into someone's hand or lifting a weight cannot be truly adequately modeled without including the effect of anticipatory cues in the simulation.

Rick:
We don't know that this is true yet. Wait until we do the simulations; then we'll see how important those "anticipatory cues" might be.

Bruce B.:
Pardon me, Rick, but isn't it absurd to put more weigh on arbitrarily specific simulations than on a varied range of observations of nature ?

No Bruce it is not absurd at all. The MAJOR message from PCT is that much of what we believe as "obvious truth" is NOT.

In an earlier message I was trying to "support" the concept of "anticipatory cues" and found even with my minor PCT experience, what we are calling "anticipatory cues" may well be a valid term but the observed behaviour should be an expected result for a limited response control system. The sort of behaviour that you see (and experience) in catching objects that differ markedly from the mass that experience (learning) has taught you to expect (and thus envoke the output system with "appropriate" control parameters that is used for controlling the perception -- catching the object) is at least qualitatively the same as an engineered control system will behave under similar circumstances.

>I sometimes get the impression that Rick's comments are very much
>off-the-cuff, and do not always do justice to the postings he purports
>to discuss - I say "purports" because my impression is that he is often
>really discussing his own perceptions. As far as I can tell PCT does
>not preclude attempts at more carefully considered communication.

I think that his comments are often "off the cuff" too and I perceive that he often missunderstands what the other party was saying or at least trying to say. Of course one is free to attempt to correct Rick's errant comments in such cases as Bruce Abbott did in fact do with respect to at least part of his posting.

Of course he is discussing his own perceptions -- he can not do otherwise.

RE: feedback

>My reaction to this comment was that Rick has assigned a specific
>interpretation to the phrase "depend on feedback", to which he objects,
>and then reiterates standard PCT (with which no one is likely to
>disagree), which is a different point. I did not think Rick's
>interpretation was implied in the original statement by Bruce Abbott
>(although only Bruce A. will know what he intended.)

I am afraid that this come down to precision of expression again. In control theory the term "feedback" is very exacting. I don't believe that even Rick described the term correctly but at least he was fully correct concerning any use of feedback by the subject.

The term "feedback" is like the term "control" as far as PCT is concerned. It is essential that loose use of the term not be allowed if everyone is going to be talking about the same phenomenon.

Rick:
... trash barrel...

>Again, according to who's interpretation ?

Obviously, Rick's.

>Later on Rick grants some predictive usefulness to observed trends in
>tracking experiments, so it is evident that he thinks that *the concept
>itself* is not without application.

You are reading him differently than am I sir. Specifically, what he said was that he does think that control can sometimes be improved if the controller is able to perceive and control a situation in terms of a higher level variable. Though this may be what you mean by "predictive control" it is rather the result of a control system attempting to reduce error between the perception and its' reference by altering the control systems output function. Tracking a target is a rather high level control function even though the loop that is actually moving the cursor is somewhat low in the hiearchy.

To say that the control system has identified a pattern (such as a sine wave motion) and thus "anticipates" the motion of the target may well be a valid way of referring to what is happening. I am not yet convinced that it is a useful way however.

Though this may well be wrong, I suspect that what has happened in such a situation is that the perceptual control loop has recognized a pattern and is now primarily controlling for the pattern and secondarily observing that controlling for the pattern reduces error.

I think that some of the difficulty in this matter may well be in the meaning of the terms "anticipatory cues" or "predictive control".

>It seems to me that everything that living organisms do involves them
>with conditions that are in the future at the instant that action is
>begun, and that any and every clue concerning changes and trends are
>necessarily of great importance, worth attention at higher levels (as
>suggested by Bruce A.) Many of our concerns with the past involve an
>orientation to learn lessons for future guidance, i.e. to pick up clues
>to better predict and manage trends.

This is almost like Martin's "If the organism dies ..." arguement. Naturally every action that we take has some consequence in the future. We do not now know the future consequence for any action that we are taking or will take. We do OTOH have a memory for consequences that we believe followed previous actions (or read about, or heard etc.) and we do also have a perception for a moderate degree of consistancy and repeatability in our perceived environment.

So yes, when a door that we wish to perceive as open is closed we do "anticipate" that we will have to operate the latch mechanism to achieve the perception.

Most of us would laugh at the idea of referring to a door knob as a "Stimulas" for opening a door yet sure enough, most of us are consistant enough that "Candid Camera" has made money altering the environment.

>IMHO the note Rick also posted by Bill Powers (920601) on Economic
>Conflicts concerned relatively high level systems analysis and
>anticipations of this type.

I don't see the relevence of your reference to Bill Powers economics posting but the gist of the difficulties in this matter may well relate to your comments on that posting.

I agree that Bill's posting was indeed a high level analysis but I don't see where it is in any way an example of "anticipatory control".

-bill

Date: Sat Apr 08, 1995 9:16 pm PST
Subject: Re: Studying principles of reorganization

<[Bill Leach 950408.23:43 U.S. Eastern Time Zone]
>[Bill Powers (950407.0825)]

Congratulations to you both though I can't say as that I am surprised that it is still working.

-bill

Date: Sat Apr 08, 1995 9:21 pm PST
Subject: Re: Newbies and accuracy

<[Bill Leach 950408.19:41 U.S. Eastern Time Zone]
>[Oded Maler (950407) - again]

>I would call it ("controlling for accuracy") as a futile attempt to
>create in the minds of others the *exact* perceptual variables that the
>"oldie" has. This is useless because in order to reach the same
>variables one has, at least, to go through the same devlopment as the
>"oldie" (including the adoption and then the rejection of the silly
>beliefs of experimental psychology).

Of course you are entitled to call it whatever you wish. However, it most certainly is the insistance upon accuracy and precision that does enable one to begin to "get PCT". Not that I may necessarily be an excellent example of a student of PCT but at least I am new at it and thus my memory for my own unique experience with PCT may be of use to others.

One thing about my background might make my own experience a bit different from that of many others is that I absolutely accepted, believed, understood and had extensive experience with the idea that an engineered control system "controls what it perceives". That is, the idea of talking about a "Temperature controller" controlling NOT temperature but rather its' PERCEPTION of temperature did not strike me as strange in the least. I was already quite used to the idea that engineered control systems only control what we want them to control when the input function really does measure 'the thing that we want the system to control'.

Even with that sort of background however, there were still several "stages" to my experience. The first was the "AH!" realization that Bill Powers was indeed equating the 'behavioural system' of living things to control systems in exactly the same way that Control Theory applies to engineered control systems.

The second "AH!" arrived while reading B:CP and realizing that he also intended that the control system concept could account for the very complex behaviour of humans -- including 'thinking.'

The "AH!s" kept comming as I began to question how PCT could explain various "observed" behaviour and that much of what is thought to be "abnormal" behaviour may well not be abnormal at all. Additionally, realizing that much of what most people might consider to be surprising about behaviour is no more than a natural consequence of control system operation.

THE AH! hit when I began to understand that all of these ideas apply to myself -- I too am a control system, controlling my own perception. That PCT tells ME that controlling my own perceptions is the ONLY thing that I do or can do. Someone else may affect my perceptions but they can NOT control them nor can I control theirs. I say this in the very precise meaning of the term control. Another may be able to keep me from controlling my perceptions but they are NOT then _controlling_ my perceptions -- they are still controlling their own perceptions which happen to include an environmental disturbance that overwhelms my ability to control.

>It is silly because it also assumes the same meaning of words and is
>based on some very coarse classification of the world: there are pre-PCT
>people who devote most of their time to S-R psychology and PCT people

>who don't do *that* terrible thing anymore.

Were the people throughout the years that have attempted to measure the distance between the Earth and the Sun silly because their estimates were not ultimately accurate? How about the people that studied the properties of radioactive materials and often "identified" the wrong nuclide?

It is by trying to explain or relate a PCT principle to some example (real world or otherwise) AND having the PCT Police "demand" precision of expression that one learns PCT. It is NOT enough to understand control theory mathematically nor even engineered control systems operation (as I did). One quite literally MUST doggedly attempt to relate what one "knows" about behaviour to PCT principles IN A FORUM OF PCTers that do not allow ambigious or sloppy assertions go unchallenged and then honestly look for and examine inconsistancies.

-bill

Date: Sun Apr 09, 1995 8:09 am PST
Subject: Anticipation, Part Zwei

[From Bruce Abbott (950409.1100 EST)]

>Bill Powers (950408.1510 MST) --

>But sometimes you seem to be talking in a different language.

Was meinen See?

> Getting back to the neglected topic of anticipation, here are two
> questions to ponder: (1) What do you suppose happens when you ask
> someone to hold out his or her hand, palm up, and then make like
> you are dropping a heavy object into the palm, but actually catch
> the object just before it strikes the palm? (2) Why?
>
>I suppose that at the expected moment of impact, the hand may move
>upward (it may not). One critical factor, I should think, is what the
>person trying to catch the object is trying to do. If the objective is
>to catch the object comfortably, then the hand may well not move up
>(When you try to catch a foul ball at a hardball game, barehanded, you
>try to have your hand moving in the same direction as the ball at the
>moment of impact, to minimize the sting). But if for some reason the
>catcher doesn't want the hand to be deflected downward, the only way to
>prevent that is to increase the upward force just as the object is
>projected to hit the hand, and by a sufficient amount.

I was talking to my dad about this on the fifth and he pointed out the same thing to me -- that to minimize the impact you might try to nearly "match speed" with the falling object. Another response, to which you alluded in another post, would be to raise the references for opposing muscles in the arm so as to increase the effective spring constant. This response is commonly called "steeling" or "bracing." Steeling does not involve any difficult timing; you only have to get set and then hold that pose until impact.

>Another example of the phenomenon you're talking about occurs when you >put a car into gear and press on the accelerator to start the car >moving. What you're controlling is a complex perception made of sounds, >efforts, and visual movement. One of the efforts you sense and adjust >is the tension in the muscles that keep you in position behind the >steering wheel. When, accidentally, you put the car into neutral instead >of drive or first gear, when you step on the accelerator the engine revs >up but the car doesn't move -- and you lurch forward toward the steering >wheel because your abdominal muscles are being told to generate a >tension at the same time your foot is supposed to be creating a force >on your body moving you backward into the seat. The higher-order control >system is sending changing reference signals to several lower-order >systems, some of which are supposed to result in canceling forces. But >when one of the forces is missing, that is the same as applying a >disturbance opposite to the missing force, and there is some movement >where there is normally none (in an experienced driver).

In this case the timing problem disappears because the same system is initiating both the disturbances to the lower-level systems and the changes in reference levels to counter them. The notion that a higher-level system initiates lower-level reference changes is exactly what I had in mind in my earlier post (950408.1805 EST) in which I said:

>>Perhaps the predictive cues are inputs to another control system, whose
>>output sets the reference for the lower-level system monitoring the
>>perceptual variable in question whose changes the predictive cue is
>>predicting.

And it's the kind of thing I had in mind when I said:

>>This ability to vary output so as to begin to counter _expected_
>>disturbances is extremely important for the well-being of most >>organisms.

To which Rick Marken (950407.1450) replied:

>>>This is a myth that probably originated with the "generated output" >>>cult, particularly the sect that worships "feedforward control".

Bill, we've been discussing a myth!

>The point of all this is that I think the subject of anticipation
>involves considerably more than just responding to a stimulus.

Yes indeed--and worth investigating. I think we've already found that the term "anticipation" covers a diverse set of (control) phenomena.

Regards, Bruce

Date: Sun Apr 09, 1995 8:50 am PST
Subject: Re: anticipatory cues

from Mary Powers 950409

Rick M, Bruce A, Bruce B

I would hope that this go-around about the value of the concept of predictive cues could break out of the same old scenario of Radical Rick saying something outrageous, followed by voices of sanity and reason. It's all too easy to say, well, that's just Rick, being extreme again.

Rick says:

> I say we throw it [predictive cues] in the trash barrel along with "stimulus control", discriminative stimulus", "reinforcement", and "selection by consequences".

Bruce A says:

> Perhaps the predictive cues are inputs to another control system, whose outputs set the reference for the lower-level system....or...it comes to function as a source of disturbance to the perception under control...

All the terms Rick wants to trash can be reinterpreted in PCT terms. But the fact remains that as they stand they refer to properties of stimuli. Our language both reflects and influences the way we think about the world, and in this case, continuing to use terms that imply that stimuli have certain properties to which organisms respond, however these terms are rationalized, is going to be a barrier to understanding PCT. Perhaps not a barrier to people on this net, who have taken considerable trouble to understand PCT, but to people who run across it more casually. If you say "predictive cue" to traditional psychologists, you do nothing to deal with all the baggage they have surrounding that term, however PCTish and sophisticated _your_ intended meaning of the term may be. For this reason I don't think that PCT can afford to use traditional language. It invites nothing but-ism. Better to abandon phrases like predictive cue, and let them wither on the vine, along with the concepts they embody.

Bruce B

I'm not sure what your post is about. Is clue a typo for cue? They are quite different in meaning.

Mary P.

Date: Sun Apr 09, 1995 9:49 am PST
Subject: BAAM Report

[From Bruce Abbott (950409.1250 EST)]

As some may recall, I presented a talk at the Behavior Analysis Association of Michigan (BAAM) on March 24th entitled "What does perceptual control theory have to offer the experimental analysis of behavior." BAAM is an organization of behavior analysts, those who apply Skinnerian principles to help resolve real-world problems involving individual behavior. Lately the leadership has been trying to broaden the scope of the meetings to include more theoretical or research-oriented topics, of which my presentation was intended to be an example.

The invitation came from Dennis Delprato, a member of CSG and supporter of PCT. Dennis thought the meeting would provide a nice opportunity to introduce PCT principles to behavior analysts and perhaps get the ball rolling in this area. My feeling was that it would be more effective to present this to a group of researchers in the experimental analysis of behavior, but that this would give me an opportunity to at least organize a talk and see how it went. It is worth noting that the number attending the BAAM meeting was not large, that my talk was one of three being given at the same time in different rooms, and that the other two talks were about applied matters of more direct concern to practitioners. I was therefore not terribly surprised when only around 20 people showed up for my presentation.

The talk seemed to go smoothly and among those in the audience I could occasionally see some individuals shaking their heads "yes" as I explained the organization and operation of the basic perceptual control system. (Even my wife thought I made sense.) After giving this quick seminar on PCT I turned to the simulations in order to demonstrate how PCT principles are used to construct models that actually behave in realistic and reasonable ways within their simulated environments. The demos included a couple of e. colis (the basic control model and the two-level model that controlled stored nutrient levels and randomly visited two nutrient sources), the SDTEST3 task (Dennis served as participant and we then ran his data through the analysis program and showed how well the model fit Dennis's mouse movements), and several senarios from the CROWD program, in which I emphasized the fact that the patterns of behavior were emergent properties of the interactions of independent control systems and not the product of direct programming--something this audience of applied behavior analysts seemed to understand and appreciate, judging by the nodding heads.

Because this was an applied group I intended to finish with an application of PCT to a program of asthma self management, but by the time I finished the crowd demo it was noon and my time was up.

From my point of view the meeting provided an opportunity to start preparing for the more serious encounters that are sure to come and, better yet, to meet Dennis for the first time and spend an enjoyable couple of days with him talking "shop" about PCT and the future of psychology. And hey--no tar and feathers!

I also attended a few of the talks, including one by Alyce Dickinson refuting several of the claims made by Alfie Kohn (which I found is pronounced "Kahn" as in "the Rath of Kahn") in his book _Punished by Rewards_ and the closer by Robyn Dawes, who is a coauthor of the book that first got me thinking about quantitative analysis in psychology, _Mathematical Psychology_. His talk was about "Psychological practice: The necessity of evaluating what practitioners actually do" and noted that practioners sometimes adopt techniques for which there is no scientific evidence that they actually do what is claimed for them, or worse, for which there is evidence that they do not work. As he was giving this talk to a group of practioners whose basic orientation is to measure results and plot them, I thought he was preaching to the choir, but the concerns he expressed are certainly valid when speaking of clinical practice in general.

Regards, Bruce

Date: Sun Apr 09, 1995 10:12 am PST
Subject: Re: Anticipation

[From Rick Marken (950409.1115)]

Bruce Abbott (950408.1805 EST) --

>For example, you might program a beep to occur in the SDTEST3 situation, >not when a point is earned as currently, but 0.5 sec prior to the change >in cursor color and thus the change in designated target. I predict >this will eliminate the annoying delay between color change and >initiation of the transition to the new target and thus result in >improved time-on-target.

Apparently you wrote this after Bill Powers' post of (950408.0715 MST). Perhaps you hadn't read that post before you wrote the above so let me quote the sections of Bill's post that are relevant to your comments.

>Now, how could anyone get the idea that the sight of the gloomy sky
>is a stimulus that elicits a particular behavior that has the
>consequence of moving the bucket to where it will prevent the
>punishing experience of a wet rug?
...
>if you standardize the conditions so that only one behavior or
>behavior sequence can end in the right consequence, then a plausible
>explanation of what happens is that the stimulus causes that behavior
>or behavior sequence and hence that consequence.
...
>Of course standardizing the conditions is the best way to avoid seeing >that this explanation is wrong. In effect, the conditions are arranged >to fit the explanation. Then in order to demonstrate that the >explanation is correct, the same conditions are carefully reproduced.
...
>This is the basis of superstition

Can you see that your suggested demonstration of improved control through anticipation is a good way for you to maintain a superstition? Suppose that the change in cursor color that has been occuring exactly .5 seconds after the warning beep occasionally occurs .1 or 0 seconds after the beep? Suppose a distrubance at the instant of the beep requires a movement in the opposite to the usual direction?

"Anticipation" is a concept that works only in the imaginary, scripted, standardized pseudo-world of conventional psychology. It is an appearance (like reinforcement); a side-effect of controlling the present time representation of a higher level variable.

See why PCT is no fun for conventional psychologists?

>I can see no conceptual difficulty in the notion that predictive cues
>might act to change perceptual inputs OR reference levels (for lower-
>level systems) in advance of the disturbance being predicted by the
>cue, and it seems that common observation supports this notion.

Predictive cues certainly _might_ change perceptual inputs or reference levels; but the modelling will show (I think) that these inputs just mess up control -- except under very special (supertitious) conditions.

Common observation does support the notion of "anticipatory control". But common observation also supports the notion that stimuli cause responses, reinforcements select behavior and outputs are generated "open loop". You have to look at behavior through the uncommon lenses of PCT (particularly, through The Test for the controlled variable) to see what is really going on when organisms behave. "Common observation" is the reason why the life sciences are in the horrendous state they're in.

>be sure that the situation (for testing "predictive cues" meets my
>requirements as stated above, or I'll argue that the simulation is
>inadequate for addressing the question.

If the requirements you are talking about are the one's you mentioned above -- where you give a signal .5 seconds before the change on every occasion -- then you are asking me to design a superstitious simulation. I think I'll use my own approach, thanks;-)

Bill Leach (950408.20:52) --

>The perception IS the feedback for a controlled perception.

No. The perception is the representation of the state of the controlled variable. "Feedback" is a circular, functional relationship between variables. I have trouble with calling the state of a variable, or a variable itself "feedback".

>The organism can not seperate the components making up the
>feedback AT ALL as long as control is 'good' (and poorly if control is
>not good).

Back to information in perception again, eh?

If by "feedback" you mean the perception of a controlled variable (as you say above) then an individual control system cannot separate the component causes of the variations in "feedback" whether control is good or bad. Remember, p = o + d and given just p (which is all the control system gets) you can't solve for o and d. Another control system in the organism can solve for d if it gets both p and o as inputs; and it can do this regardless of how well the system controlling p controls that variable.

>This "Predictive cues" is indeed an interesting issue. I certainly
>would like to listen to an explaination.

See my discussion of "anticipation" above. "Predictive cues" may be a good idea if you live in a predictable (standardized) environment. But we don't so they're not. What looks like "anticipation based on predictive cues" is probably always an observed side effect of controlling a higher order variable.

>I don't see this "anticipatory control" as anything more or different
>than one person "following" the finger movements of another when
>the other uses consistent patterns but changes the specific pattern
>periodically.

>If I am missing something significant here I would appreciate
>someone pointing out just what that something might be.

I think you are missing the significant difference between control based on prediction of the future state of a variable and control of a higher level aspect of a variable. See my typically non-responsive reply (950408.2015) to Bruce Buchanan;-)

I said:

>This [anticipatory control] is a myth that probably originated with the
>"generated output" cult, particularly the sect that worships
>"feedforward control".

Bruce Abbott (950409.1100 EST) says to Bill Powers:

>Bill, we've been discussing a myth!

Yes. And it looks like only one of you is aware of it;-)

>I think we've already found that the term "anticipation" covers a
>diverse set of (control) phenomena.

Yes. Just as terms like "reinforcement" "control by consequences", "open loop control", "reflex" and "stimulus control" cover a diverse set of control phenomena. Like "anticipation", these terms refer to observable side effects of control. Aniticipation refers to an observable side effect of controlling the present time perception of a higher level perceptual variable.

Best Rick

Date: Sun Apr 09, 1995 10:25 am PST
Subject: Lucky Bill

[From Rick Marken (950409.1130)]

Mary Powers (950409) --

>I would hope that this go-around about the value of the concept
>of predictive cues could break out of the same old scenario of
>Radical Rick saying something outrageous, followed by voices of
>sanity and reason. It's all too easy to say, well, that's just
>Rick, being extreme again.

...

>I don't think that PCT can afford to use traditional language. It
>invites nothing but-ism. Better to abandon phrases like
>predictive cue, and let them wither on the vine, along with the
>concepts they embody.

Happy anniversary Mary! I love you. Bill's sure lucky that he got to you first. Does being born 20 years before me count as anticipatory control on Bill's part;-)

Love   Rick

Date: Sun Apr 09, 1995 10:40 am PST
Subject: Re: Anticipatory Cues

[From Bruce Abbott (950409.1340 EST)]

>Mary Powers 950409 --

>I would hope that this go-around about the value of the concept
>of predictive cues could break out of the same old scenario of
>Radical Rick saying something outrageous, followed by voices of
>sanity and reason.

As you wish. Next time we'll follow Radical Rick's outrageous remarks with voices of insanity and unreason.

>It's all too easy to say, well, that's just Rick, being extreme again.

Well, if the shoe fits....

>All the terms Rick wants to trash can be reinterpreted in PCT
>terms.

O.K., what's the PCT term for "predictive cue?"

>But the fact remains that as they stand they refer to
>properties of stimuli. Our language both reflects and influences
>the way we think about the world, and in this case, continuing to
>use terms that imply that stimuli have certain properties to
>which organisms respond, however these terms are rationalized, is
>going to be a barrier to understanding PCT. . . . For this reason I
>don't think that PCT can afford to use traditional language. It
>invites nothing but-ism. Better to abandon phrases like
>predictive cue, and let them wither on the vine, along with the
>concepts they embody.

I agree that the excess baggage some terms bring with them is unfortunate, but I don't see how we can abandon ordinary language terms like "predictive cue" without making it extremely difficult to communicate. What would you suggest as a substitute? A predictive cue is just some change in the perceptual input that regularly precedes, usually within some relatively fixed time-period, some other change of perceptual input. The term has a precise meaning (essential to good scientific communication) that does not necessarily connote an S-R explanatory viewpoint. It is a fact of perception that some changes in perception are reliably cued by other, prior changes in perception, and that both humans and animals can perceive, remember, and take advantage of these relationships. If we can't use a relatively neutral term like this to describe this phenomenon, what do we use instead, and how does this new term make things clearer? (By the way, "predictive cue" is not a scientific term in the sense of having been coined and defined to serve a specific function within a theoretical framework, as, for example, "stimulus control" is; it's just common ordinary English.)

Hey, I could lodge the same complaint about "control" and "negative feedback." I haven't heard Rick suggest we throw THESE terms on the "trash heap." And the

reason is clear: there isn't anything better out there to take their places. I sympathize with your frustration, but I think we're better off using these terms in their ordinary English senses. The alternatives are to speak in convoluted sentences or to invent an obscure jargon to replace them, neither of which I find appealing.

Regards, Bruce

P.S. Congratulations to you and Bill on your recent anniversary! And they said it wouldn't last... [I've always wondered who "they" are, but they sure are busybodies.]

Date: Sun Apr 09, 1995 10:51 am PST
Subject: Re: Controlling people, Accuracy-control

[Lars Christian Smith (950409 20:36 CET)]

To: Rick Marken

Re: EEGs and levels, again

You recall that I suggested looking for higher levels in EEGs, and you said that EEGs were too crude?

How about Benjamin Libet's work on what he calls "readiness potential"? It is a measurable process in the brain, preceding the conscious decision to act, and therefore shows that what we perceive as a conscious decision to act begins unconsciously. A PCTer would interpret this as a delay caused by having to go up the levels. I.e. if you are told to "act spontaneously", you will have to think, consciously or not, about what the concept "spontaneous" means.

My point is that measurement may be possible, after all. I know nothing about it, but presumably the art of EEG measurement is making progress, and what was not previously possible may now be possible.

Reference: Libet, Wright, and Gleason. 'Readiness potentials preceding unrestricted 'spontaneous' vs. preplanned voluntary acts.' _Electroencephalography and Clinical Neurophysiology_, 54 (1982), pp. 322-335.

Best, Lars

Date: Sun Apr 09, 1995 12:03 pm PST
Subject: More Anticipation

[From Bruce Abbott (950409.1500 EST)]

Wow, the infobahn is really hummin' today!

>Rick Marken (950409.1115) --

>>Bruce Abbott (950408.1805 EST) --

>>For example, you might program a beep to occur in the SDTEST3

>>situation, not when a point is earned as currently, but 0.5 sec prior >>to the change in cursor color and thus the change in designated target. >>I predict this will eliminate the annoying delay between color change >>and initiation of the transition to the new target and thus result in >>improved time-on-target.

>Apparently you wrote this after Bill Powers' post of (950408.0715 MST).

Nope. What gave you that impression? (It did arrive shortly after.)

>Perhaps you hadn't read that post before you wrote the above so let me
>quote the sections of Bill's post that are relevant to your comments.

>[Exerpts from Bill's post deleted]

>Can you see that your suggested demonstration of improved control
>through anticipation is a good way for you to maintain a superstition?
>Suppose that the change in cursor color that has been occuring exactly
>.5 seconds after the warning beep occasionally occurs .1 or 0 seconds
>after the beep?

In other words, disrupt the reliability of the cue as a predictor of things-to-come, and control based on that cue deteriorates. Somehow I find that unsurprising. Let's consider a similar change to an ordinary control system. Change SDTEST3 so that the cursor color occasionally changes but, at least for a short while, the active target doesn't. Result: control over points-earning deteriorates. Following strict Marken logic, I conclude that the ordinary control situation is just a good way to maintain a superstition.

>Suppose a distrubance at the instant of the beep requires
>a movement in the opposite to the usual direction?

As with any conflict beween control systems, this will be disruptive. So what? I never asserted that predictive cues would be helpful every time they occur.

>"Anticipation" is a concept that works only in the imaginary, scripted,
>standardized pseudo-world of conventional psychology.

Nonsense. (See paragraph below beginning "Ridiculous" for explanation.)

>It is an appearance (like reinforcement); a side-effect of controlling >the present time representation of a higher level variable.

Rick, all phenomena are "appearances." Explaining them does not make them pseudo-phenomena or non-phenomena. Control theory may show that many of these phenomena emerge as side-effects of controlling, but this is only to say that control theory explains them. It does not render them trivial.

>See why PCT is no fun for conventional psychologists?

No. I don't find the phenomenon of anticipation any less interesting
because it can be explained and described by PCT.

>Predictive cues certainly _might_ change perceptual inputs or reference
>levels; but the modelling will show (I think) that these inputs just
>mess up control -- except under very special (supertitious) conditions.

I've already indicated my agreement with this viewpoint, and stated that my discussion of "anticipation" centers on the other cases. Did you forget?

>Common observation does support the notion of "anticipatory
>control". But common observation also supports the notion that
>stimuli cause responses, reinforcements select behavior and outputs
>are generated "open loop". You have to look at behavior through the
>uncommon lenses of PCT (particularly, through The Test for the
>controlled variable) to see what is really going on when organisms
>behave. "Common observation" is the reason why the life sciences are
>in the horrendous state they're in.

Common observation is also the source of most testable hypotheses. I'm certainly not arguing against submitting the conclusions based on common observation to empirical test; in fact I'm arguing that we do, and that, my friend, is the whole point of my bring up the topic of anticipation.

>>be sure that the situation (for testing "predictive cues" meets my
>>requirements as stated above, or I'll argue that the simulation is
>>inadequate for addressing the question.

>If the requirements you are talking about are the one's you mentioned
>above -- where you give a signal .5 seconds before the change on every
>occasion -- then you are asking me to design a superstitious simulation.
>I think I'll use my own approach, thanks;-)

Well, this will be interesting. I suppose you will demonstrate that control based on predictive cues doesn't work when the cues don't predict. I'll take that conclusion as granted.

>"Predictive cues" may be a good idea if you live in a predictable
>(standardized) environment. But we don't so they're not.

Ridiculous. Do you claim that your actions have no predictable effects on your controlled perceptions? Do you claim that in the real world certain preceptual changes do not reliably follow certain other perceptual changes? Or that disturbances to a lower-level control system produced by the actions of a higher-level system cannot be predicted and countered?

What looks like "anticipation based on
>predictive cues" is probably always an observed side effect of
>controlling a higher order variable.

My thoughts exactly, as I have expressed before. Does this make the phenomenon any less worthy of study (as your comments seem to suggest)?

Regards, Bruce

Date: Sun Apr 09, 1995 12:51 pm PST
Subject: Re: Anticipation

[From Bruce Buchanan (950409.16:50 EDT)]
[Bill Leach 950408.23:47 U.S. Eastern Time Zone]

(I wrote:)
>>. . . isn't it absurd to put more weight on arbitrarily
>>specific simulations than on a varied range of observations of nature ?

(Bill Leach comments -)
>No Bruce it is not absurd at all. The MAJOR message from PCT is that
>much of what we believe as "obvious truth" is NOT.

O.K., I do understand, not only from PCT but from the whole history of science, that what may seem obviously the truth i.e. worthy of belief in one time and place is later seen as not "the truth". Usually what is seen later involves a larger perspective and more information, arranged more adequately i.e. in terms of more useful and comprehensive theories, and the earlier "truth" is seen as limited and relative to circumstances of time and place. But this was not really my point. Nor is the also familiar distinction between "reality" and "appearances". And I know that all observations involve interpretation, as Rick points out. So we are back to the old question of the grounds for believing something to be true.

What I placed in contrast were (1) a selection of perceptual data abstracted from the whole field of possibilities for special attention and study by means of a simulation study; and (2) the larger range of observations within and outside the laboratory setting which provides the spectrum of possibilities, and from which any selection e.g. for simulation, must be made.

In one sense the question is one of where one draws the boundaries that define the field of investigation in relation to the enquiries to be made. There is probably no a priori right answer to this, the question being what is most useful and productive and the best answer the result of an iterative process. Sometimes the goal is not simply to be as exact as possible, for this requires narrower and more clearly circumscribed boundaries. As the field is expanded, uncertainties with respect to relevant measurements may increase. Let me describe the principle, which for me is very fundamental, through three examples.

(1) Almost all physicians approach diagnostic problems in the context of the whole clinical picture. When a laboratory measurement shows up which does not fit the clinical picture it is viewed with suspicion, and may be repeated or disregarded. Factors which can only be assessed impressionistically may be as important as those which can be measured very precisely, and accuracy is not the same as the measure of importance. (Sometimes the Truth is only discovered at autopsy, sometimes not even then.)

(2) Management scientists want to know the values and objectives of an enterprise in order to interprete measures of outcome and success, without which measurements may be of trivial importance.

(2) The history of scientific ideas shows that theories of nature and science have depended in part upon the larger social dimensions of thought of the times. Whitehead and many others discuss this sociology of epistemology and knowledge at length. Theories like those of Evolution and of Relativity have been understood and accepted only in a cultural milieu in which the fundamental concepts make some kind of larger sense to the whole community, including non-scientists.

I know it is said that PCT is value-free, and I think I understand why this is said. (I also think that the idea needlessly worries people who are already afraid that science and technology are "blind".) But PCT does recognize that criteria derived at the levels of systems and principles are applied to the control of lower level functions.

Now it may be said that the fact of _some_ criteria does not dictate _any particular_ criteria (or values). Nevertheless, in saying this, particular concepts of values related to where the boundaries of relevance are being drawn and what is considered to be true or useful are being implied. And this is particularly so in the case of model-building and simulation exercises.

Well, all this is to explain what I meant, too briefly stated, in speaking of the advisability of putting dominant weight upon "a varied range of observations of nature". This does not mean that the answers which may be provided by the specific questions put in the form of experiments and simulations are not highly relevant and valuable, but it does mean that the context of theory and interpretation - and valuation - are part of that relevance.

Cheers and best wishes.

Bruce B.

Date: Sun Apr 09, 1995 1:17 pm PST
Subject: Anticipation Simulaton

[From Bruce Abbott (950409.1600 EST)]

>[Bill Leach 950408.23:47 U.S. Eastern Time Zone]

>>[Bruce Buchanan (950408.11:00 EDT)]

>Bruce A.:
>It's interesting that even the simple act of dropping a book into
>someone's hand or lifting a weight cannot be truly adequately modeled
>without including the effect of anticipatory cues in the simulation.
>
>Rick:
>We don't know that this is true yet. Wait until we do the simulations;
>then we'll see how important those "anticipatory cues" might be.
>
>Bruce B.:
>Pardon me, Rick, but isn't it absurd to put more weigh on arbitrarily
>specific simulations than on a varied range of observations of nature?
>
>No Bruce it is not absurd at all. The MAJOR message from PCT is that
>much of what we believe as "obvious truth" is NOT.

Bill, I think you may have missed Bruce Buchanan's point here. What Bruce is saying is that a specific simulation based on one simple conception of how an "anticipatory cue" affects a control system is not sufficient to discredit "a varied range of observations of nature" which show that such anticipatory cues are not only used but generally help to reduce the effect of sudden disturbances, and often produce obvious effects when the predicted disturbance fails to materialize. The simulation simply may not capture the phenomenon observed in nature and thus prove nothing with respect to that phenomenon.

Regards, Bruce (Abbott)

Date: Sun Apr 09, 1995 2:19 pm PST
Subject: Re: anticipatory cues

[From Bruce Buchanan (950409.18:15 EDT)]

Mary Powers 950409 writes:

> . . . Better to abandon phrases like predictive cue, and let them >wither on the vine, along with the concepts they embody.

and then to
>Bruce B
>
>I'm not sure what your post is about. Is clue a typo for cue?
>They are quite different in meaning.

In a bumbling way I guess that I substituted the word "clue" for "cue" because I was reluctant to accept and use a term ("cue") that seemed to ascribe some anticipatory function to an external object or event in itself. I agree with the idea that everything is present-time perception.

It seemed to me that "cue" implies some kind of future-indicative property of the stimulus, which I did not want to imply, and that the word "clue" would better reflect the active interpretive and logical role of the hierarchical perceptual functions.

This was more of an unconscious slip than an intended substitution! (However, I would not have thought the shift in emphasis would not meet with objections on the basis of PCT.)

Cheers. Bruce B.

Date: Sun Apr 09, 1995 3:13 pm PST
Subject: Extremism, Anticipation

[From Rick Marken (950409.1615)]

Mary Powers 950409 --

>It's all too easy to say, well, that's just Rick, being extreme again.

Bruce Abbott (950409.1340 EST) -

>Well, if the shoe fits....

You missed Mary's point -- rather tellingly, I'm afraid. I said nothing extreme at all, as Mary confirmed by repeating what I had said and explaining why it was a good idea. For example, Mary said:

>I don't think that PCT can afford to use traditional language. It
>invites nothing but-ism. Better to abandon phrases like
>predictive cue, and let them wither on the vine, along with the
>concepts they embody.

People who understand PCT never say that my comments (about PCT anyway) are "extreme"; :"blunt", perhaps, or "tactless" but not "extreme". This is because what I say about PCT is typically correct. But because I have a tendency to be blunt and tactless (poor upbringing;-) I have been called "extreme" by those who wish to preserve beliefs that conflict with PCT. The game goes like this: a person calls my position "extreme" (even though it is exactly the same as Bill Powers's, for example) so that he or she can go away thinking that their own position is consistent with Powers' (because Bill is usually busy trying to build on points of agreement -- even when those points are microscopically small). Only when the points of agreement are negligible does Bill do anything like argue. In this "anticipation" discussion, for example, I think you should notice that when Bill does argue, he is arguing with YOU, not with me. I think you will find that Bill is just as exterme as I am (it's impossible to be otherwise regarding conventional psychology once you understand PCT); he is just a lot less blunt (usually) and considerably more tactful.

Bruce Abbott (950409.1500 EST) --

>I never asserted that predictive cues would be helpful every time
>they occur.

And you don't find this amusing? Predictive cues that don't predict?

Me:

>"Anticipation" is a concept that works only in the imaginary, scripted,
>standardized pseudo-world of conventional psychology.

Bruce:

>Nonsense.

No. This is "nonsense:

'Twas brillig and the slithy toves
Did gyre and gimble in the wabe:

Me:

>See why PCT is no fun for conventional psychologists?

Bruce:

>No. I don't find the phenomenon of anticipation any less interesting
>because it can be explained and described by PCT.

Oh no. I thought you had become a PCTer. Now you say you are still a
conventional psychologist. I am so disappointed. I hope Bill doesn't
get wind of this; he will be devastated;-)

>I suppose you will demonstrate that control based on predictive cues
>doesn't work when the cues don't predict. I'll take that conclusion as
>granted.

If control only works when the "cues" are predictive then we are really not dealing with control at all, are we? For example, let x be a variable that predicts z; so z(t+dt) = k*x(t). Now have x drive an output variable, y, via a system that inserts a delay, dt, between x and y, so y (x+dt) = kx. This system tracks z with y using the predictor x. If we make dt sufficiently large we can even say that the system is "anticipating" z with y based on the predictive cue, x.

Is this model consistent with your notion of control based on a predictive cue (in this case, the variable x)? The model appears to be controlling z-y (keeping it at zero). Is this what you had in mind as predictive control? If not, please diagram your predictive control model. Thanks.

Me:

>"Predictive cues" may be a good idea if you live in a predictable
>standardized) environment. But we don't so they're not.

Bruce:

Ridiculous.

Well, at least it wasn't nonsense;-)

>Do you claim that your actions have no predictable effects on
>your controlled perceptions?

Well, yes. The effects are not completely unpredictable perhaps; but they are NOT very precisely predictable. And, as we see in the E. coli demo, even when the effects of actions on perceptions are _completely_ unpredictable we still get control.

>Do you claim that in the real world certain preceptual changes do not
>reliably follow certain other perceptual changes?

How reliable? I'd say that the reliability is pretty low, if not 0.0. So, yeah, I guess I claim it.

>Or that disturbances to a lower-level control system produced by the
>actions of a higher-level system cannot be predicted and countered?

Of course not. What do you think?

Best   Rick (in extremis)

Date: Sun Apr 09, 1995 3:13 pm PST
Subject: Anticipation

[from Wayne Hershberger]

Tom Bourbon, what is your current e-mail address?

To Bruce A, Rick MARK N+I, and Bill P.

It appears to me that you all implicitly agree that PCT can account for "anticipation" in one manner or another, most likely in terms of a shift of the reference value of the control loop whose input is about to be altered by the impending disturbance. Piece of cake! What is puzzeling is the ad hoc source of the shift. Is it endogenous or exogenous? The shift (new reference value) is the output of a higher order system, but that system's output is a joinf function of its input and reference value. What, I am asking myself is this: what caused the change of output at this higher level (L2), a change of R (L2) or P(L2)? And if it is R(L2), then what changed that R(L2), which is the output of a higher order loop (L3), etc. etc. Since I can Not see this getting up to the level of principles, there must surely be an exogenouse source in the form of an environmental disturbance at some lower level. Or, there is some mechanism akin to reorganization involved that develops alternate parameters for extant control loops which may toggle back and forth at the drop of a hat--or book.

Cheers, Wayne

Date: Sun Apr 09, 1995 7:45 pm PST
Subject: Re: Anticipation

[From Bruce Buchanan *950409.23:00 EDT)]

Wayne Hershberger - 950409 - writes:

>It appears to me that . . . that PCT can account for
>"anticipation" in one manner or another, most likely in terms of a shift
>of the reference value of the control loop whose input is about to be
>altered by the impending disturbance. Piece of cake! What is puzzeling
>is the ad hoc source of the shift. Is it endogenous or exogenous?

Questions: What does it mean to speak of a control loop whose input *is about to be* altered? And why should the source of a shift in reference value be either endogenous or exogenous? Is there another possibility?

I think we need more adequate ways to model and describe the issues involved in these questions. I would like to suggest that the perception of time is consequent upon comparisons made between and among the effects of perceptual control loops, both internal neural and physiological cycles, and external perceptions and variations thereof.

Let us assume that Everything is Perception, and that not all perception is at the level of direct sensation alone. As I understand HPCT, one of the features of ascending levels of the hierarchy has to do with more extended and complex patterns of perception in time and space - e.g. Level 3 configurations in space, Level 5 sequences in time, etc. Higher levels in effect encompass and relate a broader range of events and processes in space and time. In effect, what appears as "the Present", as incorporated in short-term memory accessible at any level, successively expands, within certain limits, in comparison to lower levels.

>The shift (new reference value) is the output of a higher order system, >but that system's output is a joint function of its input and reference >value.

The system/level's output is also a function of its own special complexity and organizational characteristics at that level, which may include a more comprehensive range of "before-and-after" as well as new dimensions, and emotionally/motivationally tinged perception tied to relevant information, etc. (e.g.. depth and colour perception; the perception of risk of aggressive attack; or of the need to bide time before challenging the leader of the monkey troop.)

>What, I am asking myself is this: what caused the change of output at >this higher level (L2), a change of R (L2) or P(L2)? And if it is >R(L2), then what changed that R(L2), which is the output of a higher >order loop (L3), etc. etc. Since I can Not see this getting up to the >level of principles,...

Perhaps not principles conceived in isolation simply as abstract ideas, but there may be _referents_ for principles, in the perceived regularities in nature "out there" (which we cannot know), but which are in fact the origin and justification - objective concomitants - for the principles which we do perceive. In other words, we may consider *F = ma* not simply as an abstract principle of knowledge, but as reflective of a truth about the world which we cannot know directly.

>there must surely be an exogenouse source in the form of an >environmental disturbance at some lower level.

Perhaps not only at a lower level. At higher levels the system and intrinsic factors involve categories which must be open and operative for specifics to register and relate appropriately. A particular mate is not specified, but the process of mating is. At the highest level, related to the perpetuation of the species, biological and ecological cycles all become factors, and the time scale involves life cycles. While the disturbance operate through lower levels the more complex patterns involved are of determining importance.

>Or, there is some mechanism akin to reorganization involved that >develops alternate parameters for extant control loops which may toggle >back and forth at the drop of a hat--or book.

I would suggest that, to discuss processes of anticipation, it is necessary to consider the role of time and of our *perception of time*, and the possibility that anticipation involves higher levels with broader scope; that a conceptual frame of reference adequate to this task is the first requirement; and that such a frame of reference in available for development within HPCT (although I am sure that I have not made the case very well here!).

Cheers! Bruce B.

Date: Sun Apr 09, 1995 9:41 pm PST
Subject: Re: Anticipation

<[Bill Leach 950410.00:24 U.S. Eastern Time Zone]
>[Bruce Buchanan (950409.16:50 EDT)]

(1) Is saying that they guess a lot and that no one really knows how well they perform this task. Medical diagnosis is subjective even WITH laboratory results -

- so what's new? That doctors recognize that fact and temper their decisions with sujective "impressions" and experience is a credit to the medical profession but has no bearing upon scientific method.

(First 2) I won't even touch this one the way it is worded.

(Second 2) I am not sure that I understand what you are saying there. PCT may well be a lot like Einstein's relativity. Very few scientists understood relativity when it was introduced and even Einstein himself was almost shocked at some of the implications that "surfaced" as the theory was compared to an ever wider scope of observation.

Maybe someday what is now considered to be "scientific method" will be looked upon as crude and almost silly. That however is irrelevent right now. Right now, scientists use scientific method and those NOT using scientific method are not doing science. They might be doing something interesting and maybe even useful but whatever it is, it is not science.

>I know it is said that PCT is value-free, and I think I understand why
>this is said. (I also think that the idea needlessly worries people who
>are already afraid that science and technology are "blind".)

I really feel sorry about that but people that are too stupid to recognize that technology is in itself neither "evil" nor "good" are just the sort of folks that are ready to be lead by a Jim Jones (unfortunately, they seem to have the ability to drag some of us with them -- unwillingly of course).

>Now it may be said that the fact of _some_ criteria does not dictate
>_any particular_ criteria (or values). Nevertheless, in saying this,
>particular concepts of values related to where the boundaries of
>relevance are being drawn and what is considered to be true or useful
>are being implied. And this is particularly so in the case of
>model-building and simulation exercises.

>Well, all this is to explain what I meant, too briefly stated, in
>speaking of the advisability of putting dominant weight upon "a varied
>range of observations of nature". This does not mean that the answers
>which may be provided by the specific questions put in the form of
>experiments and simulations are not highly relevant and valuable, but >it does mean that the context of theory and interpretation - and >valuation - are part of that relevance.

OK. I am still left with the choice of either musing about essentially random or at least uncontrolled observations or modeling. In modeling I am faced with the difficulty of trying to ignore results that are repeatable or accept that they exist and then further my attempt at understanding the processes involved (both the model's processes and the living system's processes).

You are, of course, correct when you assert that the PCT models do not model the entire scope of the questions. The point is to do what both Bruce and Rick are proposing and that is to model a part of the phenomenon and build from there.

When one is unwilling to model then one can continue to delude oneself. Observation is at some point but the beginning and the end. Initial observation leads to questions then hypothesis. IF the scientific method is being employed then the hypothesis is tested with models. IF the models work (predict behaviour that is tested by observation) then the model (and theory) are stressed and refined.

At some point we do accept that the hypothesis is a good theory but the moment that we fail to be willing to further stress the theory - science has ended with respect to that theory.

Basically, I need to ask you what you were trying to say. Were you saying that modeling is not the right approach to reaching an understanding? Or were you just claiming that modeling will always fall short of being complete (a point with which I doubt that you will find any disagreement here)?

-bill

Date: Sun Apr 09, 1995 9:41 pm PST
Subject: Re: Anticipation Simulaton

<[Bill Leach 950410.01:14 U.S. Eastern Time Zone]
>[Bruce Abbott (950409.1600 EST)]

>Bill, I think you may have missed Bruce Buchanan's point here. What
>Bruce is saying is that a specific simulation based on one simple
>conception of how an "anticipatory cue" affects a control system is not
>sufficient to discredit "a varied range of observations of nature" which
>show that such anticipatory cues are not only used but generally help >to reduce the effect of sudden disturbances, and often produce obvious >effects when the predicted disturbance fails to materialize. The >simulation simply may not capture the phenomenon observed in nature and >thus prove nothing with respect to that phenomenon.

Yes, I think that I missed that. The point I am trying to make is one that I already know that you agree with and that is that "talking about a phenomenon" does not prove anything. At some point the process must be modeled.

You and Bruce are both right in that just modeling the process is not enough. In the first place even assuming that the model works does not permit one to immediately know why. It is still a fact that designing the model and the environmental conditions forces an honesty upon the researcher that is just not present in discussions. Stressing the working model generally results in deviation from observed behaviour and requires additional analysis.

-bill

Date: Sun Apr 09, 1995 9:41 pm PST
Subject: Re: Controlling people, Accuracy-control

<[Bill Leach 950410.00:15 U.S. Eastern Time Zone]
>[Lars Christian Smith (950409 20:36 CET)]

Much may be learned about neural functioning with the EEG. I suspect
however that there will never be great precision with such a tool. The

concept of reorganization and its' random nature suggest that neural pathway differences between individuals and possibly even the difference in the amount of neural activity for "identical operations" between individuals will always be too great for detailed analysis with EEG.

-bill

Date: Sun Apr 09, 1995 9:41 pm PST
Subject: Re: Anticipation

<[Bill Leach 950409.22:31 U.S. Eastern Time Zone]
>[Rick Marken (950409.1115)]

>>The perception IS the feedback for a controlled perception.

>No. The perception is the representation of the state of the controlled
>variable. "Feedback" is a circular, functional relationship between
>variables. I have trouble with calling the state of a variable, or
>a variable itself "feedback".

In a control system the state of the controlled variable as perceived at the comparitor is the feedback "signal". The feedback "path" is usually taken to be everything in the path from the output of the output function to the comparitor's perceptual input.

It actually seems to me that except in discussions of basic control loop operation even using the term feedback is less than useful. Perception is clearer and avoids potential confusion because of multiple feedback paths used in engineered control systems.

>>The organism can not seperate the components making up the
>>feedback AT ALL as long as control is 'good' (and poorly if control is
>>not good).

>Back to information in perception again, eh?

I don't want to go back through the postings on this yet again but Bill P. did agree that "some information about the disturbance" exists when control is poor. I agreed (and I think that Martin also agreed) that the usefulness such "information" to the control system is pretty much limited to "control is poor" regardless of how closely the variations in the perception may track the disturbance AS long as any control is being attempted.

>If by "feedback" you mean the perception of a controlled variable (as
>you say above) then an individual control system cannot separate the
>component causes of the variations in "feedback" whether control is
>good or bad. Remember, p = o + d and given just p (which is all the
>control system gets) you can't solve for o and d. Another control
>system in the organism can solve for d if it gets both p and o as
>inputs; and it can do this regardless of how well the system controlling
>p controls that variable.

I think that the above agrees with this. A problem in even discussing

this is dealing with just what is meant by "information about the disturbance". If I am opening an unlocked door and the door does not move -- massive control error, did I perceive any information in the disturbance?

>>If I am missing something significant here I would appreciate
>>someone pointing out just what that something might be.

>I think you are missing the significant difference between control based
>on prediction of the future state of a variable and control of a higher
>level aspect of a variable. See my typically non-responsive reply
>(950408.2015) to Bruce Buchanan;-)

No I don't think that I missed that point -- that was specifically what I was referring to when I made my comments about picking up the empty (but perceived to be full) carton.

The finger demo seems to show that we are able to recognize patterns and then control to produce the pattern. Exactly why we do this might still be a mystery but that we do it is not -- nor is it an example of anything more that control of current perception.

Feedforward control in engineered control systems is nothing more than perceiving a specific change in an environmental variable and changing the control of a controlled perception based upon experience with the relationship between the uncontrolled perception and the controlled one.

Such schemes work for two reasons. The first is that usually the gain for the controlled perception is non-linear such that sufficient error will overwhelm the "feedforward" signal (preventing a true "open loop" control situation) and the disturbances present are reasonably predictable by the control systems engineer.

When one attempts to throw a baseball at a target while blindfolded but with, say audible sounds for success/failure, one is approaching an "open loop" control system situation (open loop with respect to the objective of hitting the target). There is no feedback FOR the reference while the behaviour is occurring. One can indeed learn to achieve the goal (at least occassionally) IF the environment is relatively disturbance free. This is 100% 'feedforward control' (again only with respect to the goal of hitting the target). Humans can do it and machines can do it but the machines are far more successful ONLY because in such cases it is a "rigged game" (the environmental disturbances are limited and output functions can be quite precise).

Understanding the act of recognizing and then using a pattern (as in tracking experiements) is probably a worthwhile effort though I am not so sure that anyone is ready for the attempt. This is probably as good an example of "anticipatory control" as one could look for as long as one recognizes that the "anticipatory" part is nothing more than a switch to control of a higher level signal.

As pointed out by Bill P. many, many times... we can ONLY control current perceptions -- there is absolutely no way to control future perceptions. We are able to "synthesize meanings" from current perceptions about what might happen in the future and then alter references appropriately to improve control.

The thought that comes to mind for an example is the idea that an outfielder in a baseball game does not run toward a ball that will ultimately pass well over his present position. His movement away from the ball is probably another example of what some would call "anticipatory control" or even "stimulas cues". Both may be "OK" for describing the behaviour but what is happening is that the fielder has recognized the flight pattern of the ball (a current perception) and recognized that the likely future path will be over his reach (a current perception of previous perceptions - learned experience) at his present location (another current perception).

Is "anticipatory control" then just the case where the control system is not able to act upon the environment in direct opposition to the disturbances?

What I am thinking of here is that in many control situations the controller is able to directly null out the effects of disturbance by overwhelming the force(s) created by the disturbance. In the case of the ball player, the disturbance to the perception (catching the ball) is completely outside of his control until such time as he is able to change environmental conditions that neither affect the ball nor are affected by the ball. This is still control and control of current perceptions at that but it does seem to have a different flavor.

-bill

Date: Tue Apr 11, 1995 5:45 am PST
Subject: Re: Extremism

<[Bill Leach 950411.01:04 U.S. Eastern Time Zone]
>[Bruce Abbott (950410.1100 EST)]

>As the above citations confirm, I offered an argument favoring a
>particular point of view. I don't expect you to agree with that point
>of view, but I would at least expect that you provide some kind or
>reasoned reply to my argument. For example, you might suggest
>alternative terms we could use in this discussion to which you would not
>object. Or you might argue that common terms like "predictive cue" are
>so perjorative that it is well worth the inconvenience of trying to
>communicate the meaning of the term without using the term itself.

This is another one that I probably should stay out of but as usual...

I can almost "hear" Rick's response I think.

I believe that the problem with a term such as "predictive cue" is that it "singles out" an APPEARENT phenomenon that is probably not a unique control phenomenon and worse, does so by implying the stimulus bogey.

I doubt that there is any room in PCT for something like the "predictive cue". However, I do agree with you (I think) in that it is another one of those "phenomenon" that must be defined carefully and then explained by PCT (even if the definition turns out to be multiple phenomenon lumped under one title).

Basically, PCT must be able to deal with all of those "what about xyz?"

type questions and must do so in a manner that DEMONSTRATES the phenomenon "xyz" either IS an illusion or is an expected result from a control system operation under the circumstances.

This is similar to the "problem" that S-R research results must be explainable in PCT except that in this case we are talking about something that is a little more narrowly defined (probably not narrow enough yet though).

-bill

Date: Tue Apr 11, 1995 5:45 am PST
Subject: Re: Anticipation

<[Bill Leach 950411.00:05 U.S. Eastern Time Zone]
>[Bruce Buchanan (950410.11:30 EDT)]

Yes, I was being a bit flippent but there really IS a significant difference between the nature of the knowledge obtained strictly through observation and that which is subjected to both observation and modeling.

The fact is that we do not have the ability to perform much of the more demanding and precise study required by modeling in the general medical field.

As it is, there are serious medical scientists and practictioners that are not at all satisfied with the methodology currently employed in the conduct of most medical research.

There is a vast difference between medical practictioners and medical researchers. Their immediate goals differ markedly.

It reasonable for a practictioner to use methods that experience has shown to work in treatment even when there is no theory support. I would suggest that from an ethical standpoint if one has a patient with a condition that has been shown statistically to have a 50% fatality rate within a year and that a certain medical procedure has consistently reduced that fatality rate to 10% (and is better than any other known procedure) then the procedure's use is justified.

The medical field is only slightly better than the psychological field except that there are some areas of research that are conducted in compliance with the methodology of the hard sciences. Hard science research in the medical profession is very difficult for a number of reasons including both political considerations and the vast complexity of the experimental conditions.

>No, these are not valid as the only alternatives. You are familiar with
>the idea from PCT that everything is perception, and in science,
>comparably, findings depend upon methods, which must be suitable to the
>investigation and problems being addressed. Any field of study presents
>conditions and observations in the light of which, in an iterative
>process, models of selected variables and relationships may then be
>designed. It is simplistic to reduce scientific method to any specific

>formula. I forget who said (wisely, I think, and on the basis of more
>experience than I have had) that scientific method is "the use of
>intelligence no holds barred"!

You really threw me with this one... Do you mean "model" in the strict sense as employed in PCT?

If so, then we probably are in agreement. Modeling does not have to be "computer modeling" to be valid. It does have to have a set of specific and exacting transforms that do not rely upon opinions of the researcher.

The medical profession has "changed its' mind" in very dramatic ways concerning treatment methods and desired patient behaviour in just my lifetime. Most of these changes were a result of better understanding what had NOT been observed because of assumptions.

The use of models (in the PCT sense) and testing against observation by medical researchers is a comparitively new concept.

>In reply to your specific question, I would say that there is no one
>"right approach" to understanding, and that modeling is an invaluable
>approach to improved accuracy of understanding, although it is not the
>only approach and is not always applicable, and must be carefully
>designed if it is to be relevant and not misleading. (I also would not
>expect much disagreement with this.)

Either we do not agree upon what modeling is or we do not agree upon what science is.

I do agree that modeling is not the right approach to learning initially. At the point where we have absolutely NO idea of what is going on in a very complex environment, modeling is a waste of time.

For one thing it is quite important to at least try to refine observation to the point of accurate and unambigious description of what is involved. However, once it is believed that one has identified some basic principles it is time to begin proving those principles and that calls for modeling.

I suppose that it is my opinion that if you can not model a phenomenon then you do not really understand that phenomenon no matter how well you can describe it or otherwise expound upon the subject.

It is the insistance upon actually modeling theory and then subjecting the model to experimental comparison with the phenomenon being studied that has enable our exponential increase in understanding in the physical sciences. Each branch of the physical sciences has had to reach the point where theories were ruthlessly tested against "reality" and long held beliefs were allowed to topple.

Even where it is not currently possible to actually do modeling, the lessons learned from actually modeling where possible help the researcher to recognize assumptions that are not justified by observation.

My perception is that you do not agree that research without modeling is less than ideal.

It is my perception that scientific research without modeling is necessary ONLY because 1) our basic knowledge is too limited (either of the subject or of modeling... or both) or 2) some researchers do not understand science.

>pigeonhole

Yes, I suppose that I did "pigeonhole" your message. I do agree that there are some things that we just can not model. However I think that you and I disagree in that I would add "and thus will just have to do the 'best we can' with substandard methods until our knowledge improves."

-bill

Date: Tue Apr 11, 1995 6:33 am PST
Subject: Re: anticipation

<[Bill Leach 950411.01:24 U.S. Eastern Time Zone]
>[Bill Powers (950410.0900 MST)]

Though it does not change your point in the least, I suspect that the relative motion reference changes as the perception of size changes.

Then is not this what we are calling "anticipation"?

What then about the guy that does not follow the "cue"? What do we call it when the "defender" is not controlling only for relative position but is also "second guessing" the ball carrier? While his 'ultimate' perception is to tackle the ball carrier, he also consciously recognizes that the ball carrier is probably going to try to feint. To me, this still looks like control but the "program" is different.

>Tales of flinging empty boxes across the room are mostly apocryphal,
>pseudo-examples that never actually happened.

I'm not sure that I believe that you said this. I have not had dozens of boxes and milk cartons flying about the kitchen but it has certainly been close a number of times.

I don't see a problem from my view of PCT with this. If I am controlling to pick up something that I believe is heavy, my initial lifting force will be too great. The specific milk carton episode that I remember (though I KNOW similar occurances have been experienced):

I placed a nearly full half-gallon milk carton on the counter with the intent of getting a glass. My wife called me to another room and unknown to me, my eldest son drank more than half the contents (he drank a large glass full, refilled and went outside). I returned to the kitchen, grabbed the carton with my right hand as I went past its' location and ended up catching the carton with my left hand.

I think that most examples are not of a "loss of control" but rather an initial 'overcontrol' action. Again, I don't see any problem with this from a PCT perspective. Certainly you have broken a bolt or screw? With luck you don't hurt yourself in the time that it takes for the new control error to be corrected.

>force/position control

I suspect that in most situations both are used together. I think that if I am picking up something very heavy such as a lead brick that I am using primarily force control, particularly prior to actually lifting the entire weight (ie: Pickup up one side or end to establish a better grip). Also when moving a heavy barrel one again is not concerned so much with position as with force.

-bill

Date: Tue Apr 11, 1995 10:56 am PST
Subject: Re: Anticipation

[From: Bruce Buchanan (950410.11:30 EDT)]

>[Bill Leach 950410.00:24 U.S. Eastern Time Zone]

I wrote:
>>(1) Almost all physicians approach diagnostic problems in the context >>of the whole clinical picture. When a laboratory measurement shows up >>which does not fit the clinical picture it is viewed with suspicion .

>(1) Is saying that they guess a lot and that no one really knows how
>well they perform this task. Medical diagnosis is subjective even WITH
>laboratory results -- so what's new? That doctors recognize that fact
>and temper their decisions with subjective "impressions" and experience >is a credit to the medical profession but has no bearing upon
>scientific method.

Bill this is an absurd interpretation of medical science and of scientific methodology in general, which must always be appropriate to the subject matter. Your use of the terms "guess" and "subjective" indicate to me that you do not adequately discriminate among levels of reliability among observations and perceptions - which is one of the first principles of a scientific approach.

>I am still left with the choice of either musing about essentially
>random or at least uncontrolled observations, or modeling.

No, these are not valid as the only alternatives. You are familiar with the idea from PCT that everything is perception, and in science, comparably, findings depend upon methods, which must be suitable to the investigation and problems being addressed. Any field of study presents conditions and observations in the light of which, in an iterative process, models of selected variables and relationships may then be designed. It is simplistic to reduce scientific method to any specific formula. I forget who said (wisely, I think, and on the basis of more experience than I have had) that scientific method is "the use of intelligence no holds barred"!

>Basically, I need to ask you what you were trying to say. Were you
>saying that modeling is not the right approach to reaching an
>understanding? Or were you just claiming that modeling will always
>fall short of being complete...

In reply to your specific question, I would say that there is no one "right approach" to understanding, and that modeling is an invaluable approach to improved accuracy of understanding, although it is not the only approach and is not always applicable, and must be carefully designed if it is to be relevant and not misleading. (I also would not expect much disagreement with this.)

I might add that I "try to say" exactly what I mean, as well as I can, but communication also involves (1) a fund of shared meanings based upon similar experiences and perceptions, and (2) adequacy at both ends of the channel at coding and decoding more complex formulations and concepts. I perceive a problem when, in your response, you set up an inadequate pair of alternative perceptions into which you want to pigeonhole my message. The extent to which I can deal with this aspect of our communications, which is your field of perceptions, is necessarily very limited indeed! Cheers and best wishes.

Bruce B.

Date: Tue Apr 11, 1995 12:03 pm PST
Subject: Extremism

[From Bruce Abbott (950410.1100 EST)]

>Rick Marken (950409.1615) --

>Mary Powers 950409 --
>
>>It's all too easy to say, well, that's just Rick, being extreme again.
>
>Bruce Abbott (950409.1340 EST) -
>
>>Well, if the shoe fits....
>
>You missed Mary's point -- rather tellingly, I'm afraid. I said nothing
>extreme at all, as Mary confirmed by repeating what I had said and
>explaining why it was a good idea.

The devil made me do it! Mary's line just begged for the response I gave, and I couldn't stop myself. As to missing Mary's point, I not only acknowledged her point, I said that I sympathize with it:

>>I agree that the excess baggage some terms bring with them is >>unfortunate, but I don't see how we can abandon ordinary language terms >>like "predictive cue" without making it extremely difficult to >>communicate. What would you suggest as a substitute?

>>...I sympathize with your frustration, but I think we're better off >>using these terms in their ordinary English senses. The alternatives >>are to speak in

convoluted sentences or to invent an obscure jargon to >>replace them, neither of which I find appealing.

As the above citations confirm, I offered an argument favoring a particular point of view. I don't expect you to agree with that point of view, but I would at least expect that you provide some kind or reasoned reply to my argument. For example, you might suggest alternative terms we could use in this discussion to which you would not object. Or you might argue that common terms like "predictive cue" are so perjorative that it is well worth the inconvenience of trying to communicate the meaning of the term without using the term itself.

If I understand the "argument" you did offer, it consists of the following:

1. A reassertion of your claim.

2. The claim that you are right because Mary Powers agrees with you.

2. A third claim that, when it comes to PCT, you are rarely wrong.

3. The assertion that, when I think Bill Powers is agreeing with me and not you, that Bill is just being diplomatic, because you and Bill are completely redundant with regard to opinion on all matters PCT.

Now what, I ask, does this have to do with the argument at hand? Answer: nothing. It is a total smoke screen which avoids a legitimate reply. It is, in a word, unresponsive.

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

MYTHICAL BEASTIES

I'm really confused at the debate we seem to be having about "anticipation," as I didn't think I was saying anything controversial; in fact when I wrote the original post it was merely to continue a conversation I was having with Bill Powers about modeling the human arm. Bill had noted that certain control actions sometimes occur in anticipation of a disturbance; my reply merely agreed, added additional supporting observations, and noted that the arm model really wouldn't behave the way a real person does unless it included these effects. Bill has since offered additional examples of such action, and we've talked about how such behavior can be accounted for in terms of control-system organization and action. 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.

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. Do I get a response to this argument? No. It's a nice debate trick. If your opponent makes a point you can't answer, pretend he never made it and attack on some other front. But I'm not going to let it go. How about a reply that addresses the argument?

>>Bruce Abbott (950409.1500 EST) --
>
>>I never asserted that predictive cues would be helpful every time

>>they occur.

>And you don't find this amusing? Predictive cues that don't predict?

Who said they don't predict? 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.

>Me: [Rick]
>
>"Anticipation" is a concept that works only in the imaginary, scripted,
>standardized pseudo-world of conventional psychology.
>
>>Bruce:
>
>>Nonsense.
>
>No. This is "nonsense:
>
>'Twas brillig and the slithy toves
>Did gyre and gimble in the wabe:

I know that one too:

'All mimsy were the borogroves,
and the mome rath outgrabe.'

Cute, but unresponsive. A nonreply designed to mask the fact that the argument has not been replied to.

>Me:
>
>See why PCT is no fun for conventional psychologists?
>
>>Bruce:
>
>>No. I don't find the phenomenon of anticipation any less interesting
>>because it can be explained and described by PCT.
>
>Oh no. I thought you had become a PCTer. Now you say you are still a
>conventional psychologist. I am so disappointed. I hope Bill doesn't
>get wind of this; he will be devastated;-)

No, I'm just saying that I'm not Rick Marken. But I'll bet you already knew that. And, at the risk of being repetitous, I'll note again that the reply is unresponsive.

>If control only works when the "cues" are predictive then we are really
>not dealing with control at all, are we?

What? I don't think I said anything remotely resembling the idea that "control only works when the 'cues' are predictive."

>Is this model consistent with your notion of control based on a
>predictive cue (in this case, the variable x)? The model appears to be

>controlling z-y (keeping it at zero). Is this what you had in mind as
>predictive control? If not, please diagram your predictive control >model.

Actually I have several possible models in mind. I'd guess the one that comes closest to your description would be the one presented a short while ago by Bill Powers, proposed as a model for classical conditioning. You can look it up.

Regards, Bruce

Date: Tue Apr 11, 1995 1:32 pm PST
Subject: Anticipation and Control of Perception

[From Rick Marken (950410.0920)]

Wayne Hershberger (950409?) --

Hi Wayne!

>It appears to me that you all implicitly agree that PCT can account for
>"anticipation" in one manner or another, most likely in terms of a shift
>of the reference value of the control loop whose input is about to be
>altered by the impending disturbance. Piece of cake! What is puzzeling
>is the ad hoc source of the shift.

Remember that control systems are organized around control of perception, not output. If a reference (the output of a higher level system) seems to "shift" in anticipation of a disturbing effect to the higher order controlled variable, then this shift must be part of the continuing response to disturbance to the higher order variable. It "looks like" the output shift is a predictive anticipation of the disturbance effect, but it is not.

For example, when the muscles "stiffen" in anticipation of the arrival of a dropped book, this "anticipatory" output (shift in muscle tension) must be part of a loop controlling a higher order perception. This higher order perception might be the event "catching a book"; this event occurs over time and it is made up of visual perceptions (hand holding book, releasing it, book dropping) and proprioceptive perceptions (muscle tension changes).

The goal of this "event control" system is to produce a perception of "catching a book"; lower level references (such as those for muscle tensions) are varied _as necessary_ to produce this perception. Temporal variations in the muscle tension references are driven by error in the "catching the book" event control system. This error typically leads to temporal changes in muscle tension references that produce muscle tension increases that come before (anticipate) the book landing on the hand. This "anticipation" is a property of the "catching the book" event control loop; it is NOT based on a calculated prediction of a future event. This can be demonstrated by adding disturbances to the "catching the book" event that eliminate or substantially change the apparent "anticipation". For example, you could disturb the event by eliminating some of its visual components by dropping the book from behind a screen. The event can still be controlled (you can still do "catching the book") but the "anticipatory" muscle tension changes don't occur because there is no error to drive them.

In a control loop. "anticipation" must be a side effect of the dynamics of output variations that keep a perceptual signal matching its reference. Control systems can't operate properly (in the real world) by computing "predicted" future outputs; control systems must be able to do _whatever is necessary_ in order to control perceptions (and what is necessary is unpredictable in the real world; if it were predictable, we wouldn't need to have been designed as control systems).

Bill Leach (950409.22:31) --

>I don't want to go back through the postings on this yet again but
>Bill P. did agree that "some information about the disturbance" exists
>when control is poor.

If "information about the disturbance" means the ability to reconstruct d based on p (which is what I was talking about) then a control system (which only gets p) gets NO information about the disturbance, whether control is good or bad (and whether Bill P. agreed that "some information about the disturbance" exists or not; if Bill did agree to such a statement I'm sure it was in the context of a different notion of "information about the disturbance").

>If I am opening an unlocked door and the door does not move -- massive
>control error, did I perceive any information in the disturbance?

You (as the door angle control system) get no information about the disturbance; all you as door angle controller "knows" is that you are not getting the perception you want. You (as the many other control systems in you) can figure out that the door is jammed. But this perception is derived from other perceptions besides the one that corresponds to the degree to which the door is open.

Best Rick

Date: Tue Apr 11, 1995 2:26 pm PST
Subject: Re: defective predictive cue

Bruce Abbott (950410.2050 EST)

>Bill Powers (950410.1720 MST) --

>>Bruce Abbott (950410.1100 EST)--

>>>[Rick]
>>>Predictive cues that don't predict?

>>[Bruce]
>>Who said they don't predict? 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.

>Perhaps this will make the point:

>I am looking at an empty coffee cup that has been sitting beside my
>monitor all day. I have a feeling that this is a predictive cue

>concerning something I am going to do. However, even though I have been
>watching this coffee cup for over 12 hours, on and off, so far it hasn't
>predicted anything. Is there something wrong with it?

No, it's only a perceptual signal, and it is only in relationship to some control system of yours that the state of that signal has any relevance to what you may do. So when, for whatever reason, the reference of the perceptual system that "wants" to perceive a full cup of hot coffee goes high, the current perceptual state of that cup (i.e., empty) will, when compared to the reference, generate an error signal that will initiate a program whose ultimate result will be (if conditions permit) a full cup of hot coffee (not necessarily that cup, either). If the cup were already full of hot coffee, the likely result is that you would glance at the cup to confirm that it is indeed at the reference state, and the lower-level program would remain inactive, there being no need to go for a refill.

But this is, it seems to me, a different situation from the ones I have in mind when I refer to "predictive cues" and "anticipation." Let's say that promptly at 3 pm each day Mary comes into the room bearing a tray containing a steaming pot of hot coffee and two clean cups. By mutual agreement, this is your "break" time; you put aside the writing or program and the two you enjoy each other's company and the coffee for a few minutes. It is now five minutes to three. Even though you are on the brink of getting that last subroutine to work on ARM3, you click the "save" button and begin to clear the papers off the coffee table to make room for the tray. But Mary's not here yet. Is there something wrong with you? Or more to the point, what does a clock reading of 2:55 indicate about Mary?

It's now 3:10 and no Mary. What will you do? Why?

I'm probably missing your point, but then again perhaps you are missing mine. Why is this so difficult? On consideration, it seems to me that you believe that I think the "predictive cue" in your senario would directly generate a specific behavioral output--that it would predict that you would get up for a refill. That's not my viewpoint at all, as I hope I've made clear. If that's not what you're thinking, then I'm really lost. Help me out.

Predictiveness is a relationship between perceptual signals. It can be measured, quantified, put into a model. We can detect such relationships and make use of them under some conditions to improve our control over the perceptions that matter to us. We are, of course, controlling with respect to the state of current perceptions when we do so--the state of the predictive signal as well as others. Yet, when I jamb on the breaks to avoid slamming into the car ahead of me, it is not just because I've developed a control system that attempts to maintain a perception of a given distance bewteen my car and the one ahead, but also because the rapidly looming image of the car ahead predicts that very bad things are in store for me if I don't get this puppy stopped in time. Should the brakes not be up to the job, I will probably brace for the impact, not because the force of the impact is already beginning to generate an error between my reference body position and its actual position in the seat, but because I am in effect predicting that these forces will be doing so momentarily, based on the current state of the relevant perceptual signals, and am already beginning to raise the reference levels of muscle control systems so as to minimize the error those forces will generate.

Regards, Bruce

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 empahsis 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: Tue Apr 11, 1995 4:43 pm PST
Subject: Re: Anticipation and Control of Perception

Wayne Hershberger

On Mon, 10 Apr 1995, Richard Marken wrote:

>If a reference (the output of a higher level system) seems to "shift"
>in anticipation of a disturbing effect to the higher order controlled
>variable, then this shift must be part of the continuing response to
>disturbance to the higher order variable.

Right, a disturbance to the HIGHER order variable occasions an output which coincidentally anticipates another disturbance to some lower order system.

>It "looks like" the output shift is a predictive anticipation of the
>disturbance effect, but it is not.

That is, only coincidentally does the disturbance to the higher-order loop (D/H) result in an anticipatory compensation for an impending disturbance to the lower-order loop (D/L). This coincidence is what is puzzling. In controlling its input against its disturbances (D/H), the higher order loop could shift its output so as to exacerbate the adverse influence of disturbances to the lower loop (D/L).

Why doesn't it? The lower loop would not care--as long as it could still outmuscle the "exacerbated distubance." SO, why DOES the higher-order loop save the lower-order loop's bacon. Remember, control heirarchies work the other way round. While controlling their input, lower-order loops help higher-loops control theirs.

Regards, Wayne

Date: Tue Apr 11, 1995 8:40 pm PST
Subject: Predictiveness

[From Bruce Abbott (950411.1135 EST)]

>Bill Powers (950410.2130 MST)]

>>Bruce Abbott (950410.1530 EST)--

>The point I'm trying to make is probably eluding you because it's much
>simpler than you think it is.

>>Predictiveness is a relationship between perceptual signals. It
>>can be measured, quantified, put into a model.

>The problem with this statement is in asserting that predictiveness is
>an objective fact. What I'm trying to say is that predictiveness is a
>perception, a construction created by the brain.

Therefore predictiveness is NOT an objective fact? This is a false dichotomy. Your coffee cup is also a perception, a construction of the brain. Does that mean that the existence of your coffee cup is NOT an objective fact? (Note: I do not wish to get into solipism here, so let's agree that there is an objective reality out there on which perception usually depends.)

>The point I'm trying to make is simple, but it is also subtle. In
>speaking of predictive variables, one is really conflating two ideas.
>One is the idea that objectively, one variable depends on others. The
>other is the idea that this dependency can be taken advantage of by a
>suitably constructed system as a way of making predictions about events
>that are about to happen. The first idea, however, does not imply the
>second: the mere fact that a dependency exists does not imply that it
>will be used by the organism to make predictions. So to call any
>stimulus, cue, physical variable, or perception "predictive" is to
>mistake a process for a property.

But both ideas are correct. Objectively, changes in one variable do often regularly signal changes in another. Whether those linkages are perceived and acted upon by an organism is another issue. True, people can and do learn superstitions, and they often fail to make use of regularities, sometimes to their detriment. This does not change the fact that objective reality provides such regularities in abundance and that organsims, human and otherwise, frequently make use of them.

The objective predictive relationships among preceptual variables may or not themselves be perceived, but they are there regardless. When such relationships are present, it is to my mind entirely sensible to ask, from a scientific point of view, whether they become part of the input to a control system and if so, how the system behaves by virtue of including those inputs.

>>Your son had every reason to expect nothing about the force that
>>would be required to raise each box--after all, each box was
>>different--so he relied on position-control and perceptual signals
>>indicating whether the force being exerted was actually lifting the
>>box. But if every box he lifted weighed around 60 pounds, he'd
>>probably firmly grab the next one and give out with a mighty
>>heave--and feel very silly if that box happened to be empty.

>I agree. This can happen. But it is unimportant, because we have
>mechanisms for recovering quickly from such mistakes, and eventually
>we learn not to make them.

Yes, if given the opportunity to do so. But what about that fly I mentioned? It is not likely to learn from its mistakes. So it comes equipped with a control system that treats a rapidly growing visual image as a disturbance; the error thus generated then triggers a rapid retreat. Certainly there is nothing in this system that is predicting death upon failure to act, but this consequence is the very reason for the existence of the control system in the first place. In the perceptual reality of the fly, in the absence of quick action a looming visual stimulus is almost certain to be followed by a far worse disturbance to a multitude of the fly's control systems.

>I think that in one sense this discussion of expectations and cues that
>can be used to make predictions is a side-issue of far less importance
>in PCT than it was under the behaviorist model. A basic problem that the
>behaviorist model has always had is in explaining how the actions of the
>organism can vary so precisely in the way needed to keep producing the
>same result in a variable environment. These variations seem causeless,
>yet they are too appropriate to be ignored. So the problem is to find >a cause,
and that cause is the so-called discriminative stimulus or >predictive cue.

I disagree, first because I see no danger that we are pushing an S-R view in through the back door (you certainly won't see it in any model I propose) and second, because I feel that predictability is absolutely central to the whole phenomenon of control.

Were there no perceptual regularities to take advantage of, the organism could never learn to control in the first place; there would be no systemmatic changes in the perceptual variable as a function of the system's outputs. I hold that learning to control typically involves perceiving the consequences of one's actions, which is a relationship between two perceptions: sensory input as to what you actually did and sensory input as to how the perception you are attempting to control changed thereafter. If the consequences of your own behavior are unpredictable, there is no control.

Or take your example involving the bucket. Why in the world would you want to maintain the logical variable, gloomy AND bucket-under-hole-in-roof "true," unless there is a predictive relationship between gloom and rain, between rain and leaking water, between leaking water and a wet floor, and between leaking water, a bucket beneath the leak, and a dry floor?

Regards, Bruce

Date: Tue Apr 11, 1995 8:56 pm PST
Subject: Re: anticipation; qualitative cues

[From Bill Powers (950411.08445 MDT)]

Bill Leach (950411.0124 EDT)--

>I returned to the kitchen, grabbed the carton with my right hand as
>I went past its' location and ended up catching the carton with my
>left hand.

That was obviously a memorable event, and no apocryphal. That doesn't negate my claim that most such examples are NOT reports of actual experiences or observations, but are made up.

My point might be made in a different way. Suppose you went back over all the events of that day and listed all the variables you controlled. This would probably be a rather long list, assuming you could recall the day in enough detail to remember even one tenth of the details. In this long list, there would certainly be the example of the milk carton and perhaps a few others like it. But there would be hundreds of examples of control in which the source of the disturbance was invisible and you handled it without so much as a bobble.

We could constrain the list even further: just list the variables controlled as you returned to the kitchen, extended your hand, opened your fingers, closed them around the carton, started to lift the carton, saw and felt what was happening, brought your other hand up, opened it, closed it around the carton, and then did whatever you did with it, while maintaining your balance and moving your body wherever it went next. During all these control actions there was one outstanding disturbance caused by the lack of weight where it was expected. In the background, unnoticed, were myriads of normal control actions involving no anticipations or expectations.

My point isn't that we don't need to understand that event. It's that this sort of event is so rare that we could ignore it and still account for nearly all of the behavior that we see. When you first got the milk carton out of the refrigerator, you didn't have to know what weight to expect in order to pick it up. Most of the time you don't need an expectation to prepare you for disturbances.

In fact, it seems that it's precisely because we DO have an expectation that we have problems of this kind. Normally when we pick something up we simply set a reference level for its position, and the position control systems automatically call up as much force as required to make the object move as we intend. We never have to decide how much force to use; it appears as required. But if we see something that looks heavy, or that we remember or imagine to be heavy, the higher-level systems start interfering with the normal operation of the lower ones, deciding how much force to apply instead of just letting it develop as needed, and of course those estimates are never quite right. Sometimes they are seriously wrong, as in your milk-carton example. So there's a tradeoff; if we anticipate, we can anticipate wrong. If we're only a little wrong, the control system can make up the difference. But if we're a lot wrong, we would be better off not to have expectations.

This thought is for Bruce A. and Rick, too. A signal that something is going to happen can be qualitative or quantitative. Most signals that are talked about are qualitative: if any supra-threshold amount of the signal occurs, it doesn't matter how much signal occurs. That speaks of a logical variable in which only presence or absence counts, and says that the anticipation process is probably a logical or program-like process.

We have problems when a qualitative signal is used to indicate a pending quantitative phenomenon. In one S-R explanation of steering a car, for example, it was said that blowing dust or debris and moving tree-limbs provide "subtle cues" about wind disturbances, and that steering movements are caused by these cues; that explains how the driver keeps the car on the road in a gusty crosswind.

The problem here is that the steering wheel actions needed to keep the car on the road must be very precisely balanced against the changing wind-forces. If a "subtle cue" is to cause the required steering efforts, it must be translated into a quantitatively accurate steering effort (I once computed that to keep the car on a straight road for one mile, using open-loop control, the accuracy must be around 1 part in a million). However, blowing dust and waving tree-limbs are not accurate indicators of how much force the wind is going to exert on the car, so there is no way that they could actually account for the steering efforts. What is basically a qualitative cue is claimed to account for a quantitative effort. So in this case, even though we can see that there are "cues" available, we know that they can't possibly account for the behavior.

When we consider dropping a book onto a hand, the "cue" given by the initial downward acceleration of the book is only semi-quantitative. It involves an estimate of the mass of the book and its velocity when it reaches the hand, plus some sort of intuitive calculation of the force of impact. The most preparation we could expect would be a general tensing of the muscles if the book looks very heavy. As Bruce A. suggested, the only preparation that is likely, the general tensing to receive a heavy object, can be set up well in advance of the drop.

My prediction for a real experiment of this kind, then, is that if a balsa-wood model of the book is dropped, the arm will tense but the hand will not rise. It will simply deflect downward somewhat less than expected. Apart from the tensing of the muscles (which can be deliberately established), the same behavior will be seen with the eyes closed or open. If the person actually tried to generate an upward movement at the instant of impact, we would see many examples in which the upward movement occurred _before_ the impact, others where it occurred _after_ the impact, and still others where it occured at about the right moment but was too large or too small. Estimating a time delay of only 1/4 second with high accuracy is, to say the least, difficult. And the gain in performance would be negligible.

However, accuracy or quality of performance is not the issue between the EAB explanation and the PCT explanation. The problem for the EABer is to explain why the muscles act against the disturbance of the falling book AT ALL, not to say accurately. When you have to account for behavior strictly in terms of external causes, every muscle tension has to have an observable cause. So the only way to account for the resistance of the arm to the impact of the book is to say that the sight of the falling book caused a tensing of the muscles in the biceps. The question of exactly when this tensing occurs is secondary; the biggest problem is to account for any muscular resistance at all to the effect of the book.

To compound the EABer's problem, there is a superstition abroad in the behavioral sciences to the effect that feedback is too slow to account for the rapid responses to disturbances that we see. The figure for the minimum delay started out at 200 milliseconds, the time taken for a saccade, and has gradually crept upward. The latest figure I have seen is "about half a second." So who would suspect that feedback from the touch receptors to the spinal motoneurons could start a rapid rise in muscle tension in less than 10 milliseconds and that a mechanical disturbance could be 99% cancelled in less than 100 milliseconds?

Best, Bill P.

Date: Tue Apr 11, 1995 9:00 pm PST
Subject: Re: coincidental opposition to disturbances

[From Bill Powers (950411.1100 MDT)]

Wayne Hershberger (950411) --

>Right, a disturbance to the HIGHER order variable occasions an
>output which coincidentally anticipates another disturbance to some
>lower order system.

I see what you're getting at: how does the higher-level system know that it's doing something beneficial for a lower-level system, and why should it care?

Remember that this whole subject concerns the special case where the _cause_ of a disturbance is perceivable, in addition to the usual effect of a change in a controlled variable. And furthermore, there is a perception of the cause (or something related to it) considerably in advance of the actual disturbing effect.

The solution we've been looking at says that the higher system is looking at a relationship between the perception of the cause or the cause-predictor and the perception of the controlled variable. The reason this is done is that the disturbance is of a kind that can cause a considerable departure of the lower-level controlled variable from its reference level before the action can come into play to correct the error. So without some form of anticipation, the higher level systems will be unprotected against the disturbance for a long enough time to make a difference at the higher level. If that were not true, there would be no need for anticipation.

There are several possibilities for the type of controlled variable at the higher level that could produce the anticipation. One is a logical controlled variable, the implication "it is not the case that the signal has been perceived and the action has not changed." A simpler one would be a sequence: signal,action. If the signal occurs, the sequence is brought to its reference state by changing the reference signal to the action-producing system. An even simpler one would be a perception derived from the controlled variable plus the signal; this, in fact, is a model of classical conditioning (although I have yet to figure out how the right signal-perception is chosen to be added to the perception of the controlled variable). Your analysis of classical conditioning in PCT terms inspired this latter model.

So we're trying to find a model to fit a very special set of circumstances:

1. The effect of action on the controlled variable is considerably delayed, so sudden disturbances can cause large and protracted changes in the controlled variable before the action can correct them.

2. There is some event preceding the effect of a disturbance on the controlled variable by a time longer than the delay in the effect of the action on the controlled variable.

3. The event preceding the disturbance is represented in perception.

4. Enough variation in the controlled variable results from the delay in action to cause errors in higher-level systems.

The answers we are looking at basically address the question of what the higher levels that are disturbed because of delayed action in the lower systems can do to reduce the disturbance that passes through to the higher systems. So no "interlevel altruism" is involved; the higher systems are acting for their own sakes.

Best, Bill P.

Date: Wed Apr 12, 1995 4:28 am PST
Subject: Organisms Control

[From Rick Marken (950411.1220)]

It all boils down to the fact that _organisms control_. Conventional psychologists don't deal with the fact that organisms control. Instead, they are busy studying the side effects of control: the appearance of S-R, control by contingency and planned output. Conventional behavioral scientists ACTIVELY AVOID seeing organisms as controllers by talking about behavior in a way that denies the fact that organisms control.

Bill Powers (950410.2130 MST) put it this way--

>We must remember that the MAIN reason for invoking predictive stimuli >was not to explain cases in which we know what the predictive stimulus >is, but to explain all the cases where behavior seems to vary with a >mysterious

appropriateness, but for no apparent reason. In denying >purposiveness in behavior, it has always been necessary to speak of >"subtle cues" or "subliminal stimuli" or in general _unobserved_ >predictive stimuli which were the only known way of explaining what was >observed.

I (950410.2145) put it this way:

>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 can get conventional psychologists to understand PCT by simply getting them to use a new vocabulary: I think I agree with Bruce Abbott on this. The only way to get conventional psychologists to understand PCT is to get them to understand why the old vocabulary is obscurantist. We have to get conventional psychologists to understand and accept the FACT that organisms control. When conventional psychologists finally see what controlling is and that organisms are doing it ALL THE TIME then they will just stop doin what they have been doing for the last 100+ years and start doing PCT.

So how do we convince conventional behavioral (and other life) scientists that organisms control?

Best   Rick

Date: Wed Apr 12, 1995 5:20 am PST
Subject: Medical science and modeling

[From Bruce Buchanan (950411.23:55 EDT)]

[Bill Leach 950411.00:05 EDT] writes:

>As it is, there are serious medical scientists and practictioners that
>are not at all satisfied with the methodology currently employed in the
>conduct of most medical research.

Of course there are. Who would say that there is no room for improvement? But it is my impressions that actual problems relate more to the intractibility of problems and materials, etc., than to the absence of methodological concepts, which are highly sophisticated. Promising advances have almost always involved the exploitation of new apparatus and data (e.g. CAT and PET scans, etc.) as is also the case in other sciences (radiocarbon dating, etc.). Key advances also stem from the new data and theories which are really the sources for testable models.

>There is a vast difference between medical practictioners and medical
>researchers. Their immediate goals differ markedly.

In my view they are not very different in terms of fundamental scientific method. For the researcher the goal may be to discover general principles at work in biological systems per se, and in diseases such as cancers or infections, etc., with a view to methods of possible intervention. For the practitioner the goal is to ascertain what is going on in a particular patient, utilizing observations informed by theory to narrow down the possibilities diagnostically. The processes

of problem-solving are not radically different. In each case they are: collect relevant data, look for patterns (i.e features of pathogenesis or a diagnosis) on the basis of which predications may be made, and new findings used to help support or disprove hypotheses. (I am speaking of course of standards of practice as taught in medical schools, not the sloppy and unscientific marginal kinds of practice which also exist.)

> Modeling does not have to be "computer modeling" to be valid. It does >have to have a set of specific and exacting transforms that do not rely >upon opinions of the researcher.

Right. I understand by *model* not a formulation or concept unique to PCT (though it is certainly well exemplified by PCT), but "an encapsulation of some slice of the real world within the confines of the relationships constituting a formal mathematical system" (so defined by John L. Casti on page 1 in his book _Alternative Realities: Mathematical Models of Nature and Man_).

>The medical profession has "changed its' mind" in very dramatic ways
>concerning treatment methods and desired patient behaviour in just my
>lifetime. Most of these changes were a result of better understanding
>what had NOT been observed because of assumptions.

No, the change were not due mainly to changed assumptions (although there were some of these) but to closer observation made possible by discoveries of new instruments and methods, new facts, new knowledge of biochemistry, new drugs, etc. Some of your comments are perhaps more applicable to psychological theory and practice than to biological medicine.

>The use of models (in the PCT sense) and testing against observation by
>medical researchers is a comparitively new concept.

I do not think that this is so. Concepts of homeostasis and autonomic systems and hormonal regulation, etc., involve complex interactive and feedback models which go back to the last century and foreshadow some of the system ideas in B:CP and PCT.

I wrote:
>>In reply to your specific question, I would say that there is no one
>>"right approach" to understanding, and that modeling is an invaluable
>>approach to improved accuracy of understanding, although it is not the
>>only approach and is not always applicable, and must be carefully
>>designed if it is to be relevant and not misleading. . . .

>Either we do not agree upon what modeling is or we do not agree upon >what science is.

Perhaps there are points where we disagree about both! For science, everything in the world is potentially related to everything else; but, since human beings cannot handle all of "reality" at once, we select items, from among all that we perceive, define them as figures out of the background of everything else, choose variable features which appear to represent significant properties of those objects we define, and examine how these variables relate to others we might select. Thus our predecessors have discovered that F = ma, E = mc2, and laws of planetary motion, etc.

But observations, perceptions of pattern, labels and taxonomies, etc. are required *before* and *as a basis for* more precise modeling. For no matter how well modeling is done it cannot, in principle, be the whole story. In fact, higher level criteria (controlled variables) may also be required in the iterative processes by which a useful model is developed, to join observation with logic, so that units and variables are most usefully characterized and quantified, and the model is both accurate and as simple as possible. If this is what you imply by "models (in the PCT sense)", O.K., but the characteristics of good models are not unique to PCT even if PCT provides good examples.

>I suppose that it is my opinion that if you can not model a phenomenon
>then you do not really understand that phenomenon no matter how well you
>can describe it or otherwise expound upon the subject.

However, since *the model _is not_ the phenomenon*, and a model can be no more than the best stand-in or proxy designed by man to date, even an excellent model is no guarantee that you really do "understand the phenomenon" itself. For the model is still a selected and simplified version, cut to the cloth of our limited human capacities.

(As currently in the news: The Pentagon had supreme confidence in the techniques of modeling, but when MacNamara & Co., in pursuing the Viet Nam war, confused their models with reality the resulting action was tragically misguided. The lesson is not merely that their models were incomplete and mistaken but that any such models are inadequate as the sole grounds for action.)

>My perception is that you do not agree that research without modeling >is less than ideal.

I think the usefulness of modeling depends upon the problem and is an empirical question, i.e. to be decided by trial, not with reference to any "ideal". Serious inquiries into nature (e.g. research) take evidence from wherever it appears, to study or examine it logically and in relation to other evidence. Sometimes strict formal relationships, as in the mathematical laws of gravitation, can be found. Sometimes the only useful models are jerry-built through iterative trials in relation to multifactorial changing environments (e.g. economics, ecology).

But also let me say that, for a person who claims that PCT and perhaps science must be "value-free" it is paradoxical that you have some *ideal* in terms of which your are judging methods of reaching truth. The question may be asked: On what grounds do you judge that a model is the best or ideal standard of truth? I am not criticizing you for making such judgements. I am just pointing out that the question of higher level criteria is a legitimate one, and at the least it is not being consistent to deny that you are applying higher level values or controlled variables when in fact your must do so.

>. . .However I think that you and I disagree in that I would add "and >thus will just have to do the 'best we can' with substandard methods >until our knowledge improves."

Here we are again with an implied value ("substandard") imported from an ideal. Insofar as any "ideal" holds that Perfection involves simply reduction to a totally predictive model I would think it mistaken and wrong even as an "ideal".

Standards of perfect conceptual clarity, which are useful as intellectual criteria of our thought processes, are quite misleading when applied as criteria to the real world, which may not only be "queerer than we suppose, but queerer than we _can_ suppose" (as Hoyle said somewhere), and in any case are irrelevant to the universe which we cannot know. IMHO there are grounds for a lot of humility in relation to man's ultimate capacities for knowledge.

Sorry to be so long winded.

Cheers and best wishes. Bruce B.

Date: Wed Apr 12, 1995 7:28 am PST
Subject: Re: anticipation

[From Bill Powers (950410.0900 MST)]

RE: anticipation.

This is becoming an important topic, because it's bringing out a lot of subjects we've neglected. One of the most important is the way theories can become part of descriptions if we're not careful. As I pointed out last night, it is possible to come up with a pure "narrative description" in which theory is minimized at least at one level. In talking about this this morning, Mary and I realized that even in basic terms like stimulus and response there is theory being smuggled in through the words. If I turn on a light, that is one physical event. If a rat jumps in a specific direction, that is another physical event. So a pure narrative description might report that the light turned on and then the rat jumped. On the other hand saying that a light-stimulus was followed by a jumping response is smuggling a theory into the discussion -- the theory that the jumping event was a response to a stimulus produced by the light. That is a specific model of behavioral organization, not just a report on observations.

Even to say that the rat jumped toward one door rather than another is to sneak a theory into the description. "Toward" is a specific relationship between the direction of jumping and the direction of the door. Exactly the same jump could be described as jumping to the left of the other door, away from the pedestal, at right angles to the direction of a laboratory window, and so on. Each of these descriptions carries the assertion that the jumping occurred in a specific relationship to something else, so the relationship and the something else played some part in the observed behavior.

Bill Leach (950409.2231 EDT) --

To your example of the ballplayer not chasing a ball that will go well over his position, I can add another twist (that I've mentioned before). An outfielder catching a fly ball headed straight toward him, it is said, moves so as to keep the apparent rate of rise of the ball constant and slow. If this perception is controlled, the ball will arrive within catching range. If you look at the behavior of the ball player, it will seem that the sight of the ball causes the player to run in anticipation of the catch, but in fact something is under control all the way (or at least intermittently as the player casts glances over his shoulder while running toward the fence).

This leads into another subject: feints. A wide end carrying the football is approached by a defender, and feints one way and the other until the defender "buys" the direction the runner is apparently going; the runner switches to the other direction and fakes the defender out of his socks. The defender's movements look like anticipatory responses to the runner's changes in direction, but in fact the defender is simply trying to maintain a relationship to the runner, between the runner and the goal line. The runner's next-to-last move involves a perceptible increase in speed to one side, which the defender counters with an acceleration in that direction -- but the runner has initiated a sequence that involves an immediate switch to the other direction, and the defender can no longer reverse his movement in time. The success of the feint depends on the fact that the defender has a time-lag in his control system and can't physically change his velocity instantly, whereas the runner can produce two changes in direction of running without letting the first change go to completion.

So some apparent anticipations might arise from continuous control of a relationship.

RE: Picking up the empty box that you think is full.

Tales of flinging empty boxes across the room are mostly apocryphal, pseudo-examples that never actually happened. Unfortunately the literature of psychology, like that of politics, is full of such invented examples, constructed to fit one's theory but never actually observed, like Reagan's famous "Welfare Queen" who drove up in her Cadillac to collect welfare checks for her nonexistent 27 children -- while Reagan portrayed "outrage." The Welfare Queen never actually existed. All you have to do is ask yourself a few questions and this will be obvious -- questions like "How come nobody could find her afterward? Who saw her doing this and knew how many checks she was collecting, yet didn't say anything at the time? Are Cadillacs that difficult to track down?" and so on (Neat example thanks to Mary).

But back to boxes. If in fact people expecting an average-weight box would fling a box containing a feather pillow over their heads, how come UPS hubs, where human beings spend the whole day transferring mysterious boxes from one conveyor belt to another or one cart to another or one truck to another aren't filled with flying boxes of lingerie and littered with dropped boxes of books? My son, who once worked at such a hub, assured me a long time ago, when I asked, that he simply picked up the boxes and put them where they belonged, although he confessed to throwing a few boxes to save some steps.

Whether you fling a light box or just pick it up depends on whether you're using force control or position control. If you're using force control, you set the reference force to a level that you think will cause the box to rise -- but the force you sense depends on the mass and acceleration of the box, so to achieve a particular upward force you have to accelerate a light box at a high rate. If you're using position control, as most of us have learned to do, you set a reference level for the position of the box, and the lower-level systems produce as much force as necessary to accelerate the box and decelerate it again as it approaches the zero-error position. No flinging, no dropping. And no need to know what's in the box.

In listening to psychological arguments you have to ask constantly whether you're hearing an apocryphal tale constructed to fit a theoretical expectation or a

report of an actual study. You have to watch out for that in PCT, too -- and everywhere else that people are trying to win arguments.

Reagan, when challenged to produce the evidence that this Welfare Queen actually existed, made up three FBI agents who witnessed the event, but the FBI agents could not be found either. (I just made that part of the story up to prove my point, and for that matter I don't know where Mary got the basic story, either. A lot of arguments are fueled by this sort of bullshit, in and out of science. What is Truth?).

--

Bruce Buchanan (950409.2300 EDT) --

>I would suggest that, to discuss processes of anticipation, it is
>necessary to consider the role of time and of our *perception of
>time*, and the possibility that anticipation involves higher levels
>with broader scope; that a conceptual frame of reference adequate
>to this task is the first requirement; and that such a frame of
>reference is available for development within HPCT

Best suggestion I've heard all day. 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 (like a few I've mentioned above) can be handled with a model that doesn't actually involve any anticipation at all. And in the few circumstances where real anticipation is found, I think the answers will be rather simple. The explanations get complex only when you think the environment has to be responsible for everything.

Best to all, Bill P.

Date: Wed Apr 12, 1995 7:33 am PST
Subject: Re: defective predictive cue

[From Bill Powers (950410.1720 MST)]

Bruce Abbott (950410.1100 EST)--

[Rick]
>Predictive cues that don't predict?

[Bruce]
>Who said they don't predict? 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.

Perhaps this will make the point:

I am looking at an empty coffee cup that has been sitting beside my monitor all day. I have a feeling that this is a predictive cue concerning something I am going to do. However, even though I have been watching this coffee cup for over 12 hours, on and off, so far it hasn't predicted anything. Is there something wrong with it?

Best, Bill P.

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:

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

Well, that's a start. I don't follow the one for reinforcement--even after correcting the typo (I assume you mean "variable"). 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 lession 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 occurence 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 is 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 Apr 12, 1995 3:57 pm PST
Subject: Re: anticipation etc.

[From Bill Powers (950410.2130 MST)]

Bruce Abbott (950410.1530 EST)--

The point I'm trying to make is probably eluding you because it's much simpler than you think it is.

> Predictiveness is a relationship between perceptual signals. It can be measured, quantified, put into a model.

The problem with this statement is in asserting that predictiveness is an objective fact. What I'm trying to say is that predictiveness is a perception, a construction created by the brain. Unless the brain contains a specific function that treats one variable as a predictor of another, no prediction will be made. In order for such a function to be constructed, it is not necessary that the first variable regularly precede the second; I can use any variable as a predictor of any other variable. Of course random selection of predictors is not going to prove very useful; it leads, as you know, to superstition, or simply fails to produce any valid predictions. But that doesn't matter; the fact that we can construct such predictive functions confirms the fact that prediction is something that has to be done by a brain function. Neither a physical variable nor a signal that represents it can predict anything. Prediction is a process, not a trait or a property of a physical variable or a perception of it.

So to attach an adjective like "predictive" to a noun like cue, signal, variable, or perception is simply a mistake.

Yes, a variable can be used in a calculation to make a prediction of another variable -- but it is the calculation that is doing the predicting, not the variable. The quality of the prediction can be quantified -- but only by some process that does such quantification. When we put a so-called "predictive input" into a model, we are not finished; we must also put in the specific function that does the predicting. In specifying the "predictive input" we don't distinguish it from any other input: it's just another variable. What makes it seem predictive is what the organism does with it, not any special character of the variable.

As we understand the physical world, essentially every physical variable is a function of concurrent and previous values of other physical variables. For any event, therefore, there is an endless number of other variables that could be used to make predictions. But there is nothing in that physical world that forces a prediction to be made. Most variables that could be used for prediction are simply ignored. Those variables lead to the values of other variables all of the time, yet they play no role in predictions.

The point I'm trying to make is simple, but it is also subtle. In speaking of predictive variables, one is really conflating two ideas. One is the idea that objectively, one variable depends on others. The other is the idea that this dependency can be taken advantage of by a suitably constructed system as a way of making predictions about events that are about to happen. The first idea, however, does not imply the second: the mere fact that a dependency exists does not imply that it will be used by the organism to make predictions. So to call any stimulus, cue, physical variable, or perception "predictive" is to mistake a process for a property.

> Your son had every reason to expect nothing about the force that would be required to raise each box--after all, each box was different--so he relied on position-control and perceptual signals indicating whether the force being exerted was actually lifting the box. But if every box he lifted weighed around 60 pounds, he'd probably firmly grab the next one and give out with a mighty heave--and feel very silly if that box happened to be empty.

I agree. This can happen. But it is unimportant, because we have mechanisms for recovering quickly from such mistakes, and eventually we learn not to make them. When we pull on a door, we adults no longer plant both feet together and lean back, because once in a while the door gave way too easily or was pushed open from the other side and we ended up flat on our backs. We plant one foot, and keep the other back a way just in case.

I think that in one sense this discussion of expectations and cues that can be used to make predictions is a side-issue of far less importance in PCT than it was under the behaviorist model. A basic problem that the behaviorist model has always had is in explaining how the actions of the organism can vary so precisely in the way needed to keep producing the same result in a variable environment. These variations seem causeless, yet they are too appropriate to be ignored. So the problem is to find a cause, and that cause is the so-called discriminative stimulus or predictive cue. How else could an organism know that it had to vary its action when a disturbance came along? The whole idea was that behavior is caused by external events, so if behavior changes in such a way as to counteract a disturbance, there must have been some environmental event that told it to do so, some precursor of the disturbance.

We know now that no such precursor is required. A control system resists disturbances automatically, simply because of the way it is organized. No external impetus is needed to make it do so. There is no need for the organism to anticipate disturbances of most kinds in order to minimize their effects. So in most real circumstances, the explanation that relies on a precursor or warning or predictive stimulus is unneeded.

Of course we have to recognize that there are circumstances under which anticipation is helpful or even necessary for control. But in the context of all the control actions we are performing, all continually resisting disturbances of many kinds, the kind of control that requires anticipation is relatively rare. We must remember that the MAIN reason for invoking predictive stimuli was not to explain cases in which we know what the predictive stimulus is, but to explain all the cases where behavior seems to vary with a mysterious appropriateness, but for no apparent reason. In denying purposiveness in behavior, it has always been necessary to speak of "subtle cues" or "subliminal stimuli" or in general _unobserved_ predictive stimuli which were the only known way of explaining what was observed.

Since we can now explain the same phenomenon in a much more direct way, there is no longer any need to find or imagine cues and warnings that cause behavior to change in just the way required to maintain a constant result. We can now see the phenomenon of prediction as a special case, one that does require to be dealt with, but which by no means extends to behavior in general. Most behavior takes place without any need for prediction.

Best, Bill P.

Date: Wed Apr 12, 1995 4:49 pm PST
Subject: S-R language, Anticipation, etc

[From Rick Marken (950411.2200)]

Bruce Abbott (950411.1135 EST) --

>Your coffee cup is also a perception, a construction of the brain. Does
>that mean that the existence of your coffee cup is NOT an objective
>fact?

The perception of the coffee cup is an objective fact; the "coffee cup" is not. Or are you one of those people who knows what's REALLY out there because they can see "beyond" their perception.

>I see no danger that we are pushing an S-R view in through the back
>door (you certainly won't see it in any model I propose)

The "danger" is that the use of S-R language makes it difficult to see how control works, even in one's own models. S-R language, for example, makes it possible to imagine that one is dealing with a control model when, in fact, one is not. This seems to be what happened in the E. coli demos. You developed an E. coli model that _seemed_ (to you) to have learned, via selection BY consequences, to control. I don't know whether or not you still believe that this was actually going on (that consequences actually "selected" a control organization) but the language apparently made it difficult for you to see that the control organization was not "selected" (controlled) at all; it was an unintended result

of one particular environmental set up, like the tracking produced by my predictive control model (it tracks only when the environmetal variable causes outputs that look like tracking).

>feel that predictability is absolutely central to the whole phenomenon
>of control.

Well, yes. But unpredictability is central too; control is the ability to produce predictable results in an unpredictable environment.

>Were there no perceptual regularities to take advantage of, the
>organism could never learn to control in the first place;

To some degree this may be true; but the "regularities" can be pretty weak. Consider learning to adjust the temperature of tap water. How much quantitative regularity is there in the relationship between your muscle actions and their effect on the result to be controlled (water temperature)? Remember, you can do this with all kinds of different handles (hot separate from cold, single throw) with different kinds of non-linearities in the relationship between throw and flow. And yet, I bet that even EABers have learned to adjust the temperature of tap water and can now do it wherever they are;-)

Bruce Abbott (950411.1620 EST) --

>I don't follow the one for reinforcement--even after correcting the
>typo (I assume you mean "variable"). I suggest a closer approximation
>would be "reinforcER."

Why? "ReinforcER" still suggests that an entity (like a food pellet) is able to do something to behavior (strengthen it) that we know it can't do. A food pellet is just a perceptual variable (or an aspect of a perceptual variable, like rate of food delivery). In operant experiments, this perceptual variable is under control.

>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.

How do they DO this? They can be used as the input to a prediction algorithm. But the things that are referred to as "predictive cues" are just disturbances or perceptual variables. As Bill noted, ALL variables are predictive cues in the sense you mean; all variables predict other variables.

>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.

Ain't that the truth! Well, for you and me maybe;-) I've found that, if you're going to cheat off of anyone in god's "Nature of behavior" class, you'll always get an A if you cheat of off Bill P.

Best Rick

Date: Wed Apr 12, 1995 5:32 pm PST
Subject: Re: predictiveness

[From Bill Powers (950411.2350 MDT)]

Bruce Abbott (950411.1135 EST) --

>>The problem with this statement is in asserting that predictiveness is >>an objective fact. What I'm trying to say is that predictiveness is a >>perception, a construction created by the brain.

> **Therefore predictiveness is NOT an objective fact? This is a false dichotomy. Your coffee cup is also a perception, a construction of the brain. Does that mean that the existence of your coffee cup is NOT an objective fact? (Note: I do not wish to get into solipism here, so let's agree that there is an objective reality out there on which perception usually depends.)**

Yes, let's agree that there is an objective reality; I hope you will also agree that it is best described in the terms of physics. Physics is a collection of models in brains, of course, but as models go it is the most completely self-consistent one we have, not easily to be displaced in any basic ways.

I'm floundering somewhat for words; the point I'm trying to make seems perfectly clear to me, yet it obviously is far from self-evident. To me, prediction is a psychological process, a mental computation, something done by a brain. It takes a presently-occurring perception and from it (and some rule, law, or generalization) generates an expectation (still in present time, always in present time) of a perception yet to come. In the physical world which we presume to underlie the given perception, however, all that exists in present time is the physical entity, variable, event, whatever, in its present state. The variable simply has a value and derivatives, and functional links with other variables. At the time we observe it, we observe nothing of any future effects of the variable.

From the present state of the physical world, the next state arises in a continuous flow. We have been able to develop a respectable number of principles or laws from which we can calculate expected states of the world on the basis of presently-occurring states. This is what we call "prediction."

To me, saying that the present state of any physical variable "predicts" the future state of some other physical variable is to ignore the mental process that is involved. I can see using this expression in an informal way, as shorthand for saying "from the state of this variable _I can predict_ or _someone can predict_ the future state of another variable." But to use the phrase that way implies that one is always conscious of the fact that the prediction is being made by some being on the basis of certain assumptions and with the help of symbol-manipulating processes. In short, a prediction is always a _subjective judgment_. If we realize this, we might fall into the use of terms like "predictive variable," but we will never be under the impression that the variable itself, with no help from a human being, can make predictions.

To me, speaking of the predictiveness of a physical variable is just like speaking of the sweetness of a chemical substance or the loudness of a sound. These are all human perceptions, and mean nothing when we consider physical phenomena without regard to the way they appear in human perceptions. A prediction is a process that leads to an expectation, not to a state of the physical world.

I know I'm swimming upstream here, but it has always seemed both useful and methodologically important to maintain a consciousness of the degree to which the world we experience is shaped by our internal perceptual organization. How else can we begin to separate out what is optional or idiosyncratic in experience from what is mandatory? In physics this has been done (as far as possible to date) by reducing observations to very simple terms, low-level perceptions on which agreement is easy to obtain and of which replication is easy to achieve. But in psychology, there has been very little attempt to do this, outside some parts of psychophysics. Most properties of human beings that psychologists talk about are blatantly subjective judgments, like "competence" or "confidence" or "socialization." They are evidence of the organization of the observer more than the observed.

You say

> **Objectively, changes in one variable do often regularly signal changes in another.**

But "objectively", this is precisely what they do NOT do. If you remove the human observer from the scene, all you have is a variable changing. it does not send out any signals telling us what will happen next. The future has not yet arrived; there is no label hanging from the variable saying what values of other variables at some future time it predicts. There are no "signals." There are only variables in the states that they are in.

> **Whether those linkages are perceived and acted upon by an organism is another issue.**

It is the ENTIRE issue. There is no way to "perceive" those linkages in the sense of making a physical observation; the linkages are mental/internal, not physical/external. When there is a flash of lightning, a compression wave begins to spread in the air. It spreads and spreads and eventually merges back into the thermal agitation of air molecules. And that is all that happens in the physical "objective" world. At no point is there anything but the present state of the world, continually transforming into succeeding states. By the time the compression wave enters a human ear and gives rise to auditory signals in the nervous system, the wave of electromagnetic energy from the flash has long passed. At no point is there anything to observe of the physical world that represents a linkage between the flash of lightning and the compression wave in the air.

The linkage is not out there in the physical world; it is here in our brains, in the mental world. Even to deduce that there is a connection, we have to use memory, to be able to observe a past event at the same time as a present one. Memory creates the possibility of a cause-effect

perception, of concepts like delay, of concepts like time. It is memory, and the operations we perform on signals both from present perceptions and from recorded perceptions, that leads to the notion of physical laws, of one thing affecting others through time.

When we model behavior in an environment, we do not put things like sweetness or loudness or predictivity into the environmental part of the model. We put chemical concentrations there, and vibrational energy, and the present states of physical variables. Especially in a simulation, we focus strictly on present time and the evolution of the present state of the physical world into the next adjacent state. Even we who construct the simulations have to wait to see what future states develop out of all the variables that are interacting in one present moment after another. Even a system of differential equations represents only present time relationships among values, derivatives, and integrals of variables.

If we want prediction to play a part in the model, we can't put it into the environment. The environment part of the model will remain exactly the same whether prediction occurs or not. The only way to get prediction is to put it into the model of the behaving system; to equip it with computational methods that specifically extrapolate from the present into a hypothetical future.

I should point out that a predictive stimulus has a peculiar property if we take the term literally. What the stimulus predicts is that a controlled variable is going to be changed from its present state. But if the behaving system is properly organized, it will institute a disturbance-resisting action just as the disturbing variable in the environment begins to influence the controlled variable -- and as a result, the predicted effect of the disturbance doesn't happen. So as a predictor of what is going to happen, the predictive stimulus is a total failure. It predicts an effect of a disturbance that would happen if there were no organism present. But if no organism is present, the predictive stimulus has no behavior to affect, even though, as we would see if we could observe and analyze a memory record of the physical processes, we would find that a prediction based on the value of the "stimulus" (which is no longer a stimulus since there is nothing to be stimulated) would now succeed!

What I am arguing for is a terminology that puts the physical world outside the organism and everything else inside it. A term like "predictive stimulus" fails to do this. Many terms in EAB and in the rest of psychology mix the domains, attributing to physical events aspects which are really generated by the behaving system and do not exist in the physical world. This mixing of domains becomes perfectly obvious when we try to construct working models of behavior, even very simple behaviors. We find that we are unable to distinguish a model of a variable that is predictive from a variable that simply has a value and derivatives. Predictiveness simply does not belong in the physical domain.

Best, Bill P.

Date: Wed Apr 12, 1995 6:41 pm PST

Subject: EEG, b3dpbmcgaW

[From Rick Marken (950412.1100)]

Lars Christian Smith (950409 20:36 CET) --

>How about Benjamin Libet's work on what he calls "readiness potential"? >It is a measurable process in the brain, preceding the conscious >decision to act, and therefore shows that what we perceive as a >conscious decision to act begins unconsciously. A PCTer would interpret >this as a delay caused by having to go up the levels. I.e. if you are >told to "act spontaneously", you will have to think, consciously or not, >about what the concept "spontaneous" means.

I like your interpretation of what is required when one is asked to "act spontaneously"; one does have to "go up a level" (in imagination) to see if what one is currently doing is "spontaneous". But I'm not sure that the EEG data really confirms (or denies) this notion. It seems to me that there is a very large gap (at the moment, anyway) between the neurological assumptions of the PCT model, which takes individual neurons to be the relevant units of control (individual neurons carry the perceptual, reference and error signals that implement a control system) and measures of average neural activity, which is what is picked up in the EEG. I'm sure there are fairly reliable and suggestive patterns that can be found in the EEG. I just don't think these patterns say one thing or the other about the PCT model. I don't think EEG data will be relevant to PCT until someone actually shows what the neural level PCT model predicts about EEG patterns under various circumstances.

Wayne A Hershberger (950412) says:

>dmVseSBvbiB0aGUgbmV0KSwgdGhhdCBJIGFtIGxlZnQgYSBiaXQgZGl6enku
>ICBIb3dldmVyLCBJIGFtDQpmb2xsb3dpbmcgaW4gdGhlIHNlbnNlIHRoYXQg
>SSBjYW4gc3RpbGwgc2VlIHlvdXIgZHVzdC0tYW5kIHdoZW4geW91IGFsbA0K

To which I can only reply:

lZHU+DQpUbzogQnJ1Y2UgQ **;-)**

Best Rick

Date: Wed Apr 12, 1995 7:49 pm PST
Subject: Re: coincidental opposition to disturbances

Wayne Hershberger

On Tue, 11 Apr 1995, William T. Powers wrote:

A very lucid and constructive post in which he observed that in addressing the question of anticipatory output'

>we're trying to find a model to fit a very special set of
>circumstances:
>
>1. The effect of action on the controlled variable is considerably
>delayed, so sudden disturbances can cause large and protracted changes

>in the controlled variable before the action can correct them.
>
>2. There is some event preceding the effect of a disturbance on the
>controlled variable by a time longer than the delay in the effect of >the action on the controlled variable.
>
>3. The event preceding the disturbance is represented in perception.
>
>4. Enough variation in the controlled variable results from the delay >in action to cause errors in higher-level systems.
>
>The answers we are looking at basically address the question of what the
>higher levels that are disturbed because of delayed action in the lower
>systems can do to reduce the disturbance that passes through to the
>higher systems. So no "interlevel altruism" is involved; the higher
>systems are acting for their own sakes.

Bill, one of the higher order systems that might be involved in this process is one that tries to control generalized or pooled error and whose output tweaks gain and delays in "subordinate" loops--or, perhaps calibrates the "scale" of their input or output transfer functions. The disturbance of any sluggish lower-order loop would also constitute a disturbance to any higher-order loop controlling some overall level of pooled error; the anticipatory signal would not need to do that job. The pooled error could have a long integration interval and a long loop time.

Best Regards, Wayne

Date: Wed Apr 12, 1995 9:15 pm PST
Subject: Re: anticipatory output

[From Bill Powers (950412.1105 MDT)]

Wayne Hershberger (950412) --

> Bill, one of the higher order systems that might be involved in this process is one that tries to control generalized or pooled error and whose output tweaks gain and delays in "subordinate" loops--or, perhaps calibrates the "scale" of their input or output transfer functions. The disturbance of any sluggish lower-order loop would also constitute a disturbance to any higher-order loop controlling some overall level of pooled error; the anticipatory signal would not need to do that job. The pooled error could have a long integration interval and a long loop time.

Yes, a real possibility. We've done essentially nothing toward modeling control through adjustment of parameters. The sort of thing you're talking about would apply to the general manner of controlling many variables -- whether it's done in a lazy and sloppy way, or with reasonable skill, or at a high level of tension and jumpiness. The nearest approach to modeling this dimension of control is in the arm model, where by using opposed nonlinear muscles and allowing for control of muscle tone, we could create all states of control from total flaccidity to the jittery brink of spontaneous oscillation.

But that's not the same as the situation that Bruce Abbott proposed, where one environmental variable (over which we have no control) simply provides a signal saying that another environmental variable (also uncontrollable) is going to change pretty soon, and where that other environmental variable will create a significant error in a number of control systems at several levels. With the advance warning given by the signal, we can learn to prepare for the onset of the disturbance by starting an opposing behavior before the disturbance actually happens (including arranging to be elsewhere when the disturbing event takes place).

As to _why_ we learn to do this, your explanation may have some relevance. Presumably we begin by suffering the consequences of unpredicted disturbances which we can't correct with normal efficiency. So this would lead to a lot of "generalized or pooled error" in systems at different levels. It could even lead to intrinsic error and start reorganization going. The solution to this generalized error is to search for some reliable indicator that the disturbance is going to happen, so it can no longer take us by surprise. If we keep getting bitten by snakes, eventually we notice that this happens mostly when we get too close to piles of rocks on warm days, and we learn to avoid proximity to piles of rocks (either on warm days or altogether). The piles of rocks are treated as indicators of future snakebites. We learn to control for a rule: keep your distance from piles of rocks. Of course this doesn't protect against snakebites, because while snakes like warm piles of rocks they are not confined to them. However, in the long run staying away from the piles of rocks leads to a reduction in "generalized error."

In my preliminary attempts to model classical conditioning along the lines you suggested, I ran into a problem which is still under consideration. What Bruce Abbott was describing at one point was literally a signal -- a flash of light, say, arranged by an experimenter to occur some time before a shock delivered through a floor grid. What the rat would learn would be to jump just before the shock.

The interesting aspect of this is that a flash of light by itself does not, through physical laws, predict a shock. Both the flash of light and the shock are created by a third party, the experimenter, who is not bound by natural law to stick to natural contingencies. The experimenter can use literally any physical event that the rat can perceive to signal a change in any other physical variable.

Rats can learn under these conditions. So if we're to model this process including the learning phase, the model must be able to treat _any_ randomly-selected perception as if it represents a incipient change in _any_ controlled perception.

This implies that the model must have available to it an assortment of inputs representing the states of environmental variables, and some way of perceiving a regular temporal relationship between any of these variables and the controlled variable in question (the basis, as you said, of an "unconditional response"). Then there must be a mechanism, preferably simple, for gradually incorporating the variable that most

reliably precedes the occurrance of the disturbance into the perceptual function that detects the state of the controlled variable. This mechanism must work with ALL of the possible indicators of the disturbance, because in principle any one of them might turn out to be the signal that is needed, at the experimenter's whim.

Perhaps the critical variable here is the error signal in the control system, because the basic problem is that the normal response to a disturbance of the controlled variable is not keeping the error sufficiently small. What is needed is a mechanism that will be activated by large error signals, and as a result will start bringing various inputs into the definition of the perceptual signal and keep doing this until an input is found that will result in reducing the error. I am currently favoring this approach, because it can also account for the fact that a particular variable is being controlled -- in other words, the "anticipation" aspect of the system is just an extension of the same process that resulted in a variable being controlled in the first place. If that could be achieved, we would have a very economical model of classical conditioning.

As you can see, I'm more concerned with the details of how a model like this could be made to operate, and not so much with WHY such a mechanism should exist, which is a higher-order consideration.

What is the MIME stuff?

Best, Bill P.

Date: Wed Apr 12, 1995 9:18 pm PST
Subject: Re: Predictiveness

[From Bruce Abbott (950412.1055 EST)]

>Bill Powers (950411.2350 MDT) --

>Yes, let's agree that there is an objective reality; I hope you will
>also agree that it is best described in the terms of physics.

Agreed.

>To me,
>prediction is a psychological process, a mental computation, something
>done by a brain. It takes a presently-occurring perception and from it
>(and some rule, law, or generalization) generates an expectation (still
>in present time, always in present time) of a perception yet to come.

Yes, I would agree with you here as well.

>In the physical world which we presume to underlie the given perception, >however, all that exists in present time is the physical entity, >variable, event, whatever, in its present state. The variable simply has >a value and derivatives, and functional links with other variables. At >the time we observe it, we observe nothing of any future effects of the >variable.

Yes, but are derivatives real or only useful mathematical abstractions--can there be a rate of change without a passage of time? I'm asking this because it seems to me that what we perceive as change results from a psychological process of comparison between past and present perceptions, and thus has the same logical status as predictiveness. A predictive relationship between two variables is as objectively quantifiable as the rates of change of those variables and does not depend on the psychological processes of the observer any more than the rates do. It is a relationship between two patterns of change.

Thus I view predictiveness as a property of the physical world in the same way as rate of change is. Both terms refer to relationships. A predictIVE signal is one having a certain more-or-less regular temporal relationship with another signal. PredictION, on the other hand, it a psychological process involving the perception of a predictive relationship between variables and the use of inference.

>To me, saying that the present state of any physical variable "predicts"
>the future state of some other physical variable is to ignore the mental
>process that is involved. I can see using this expression in an informal
>way, as shorthand for saying "from the state of this variable _I can
>predict_ or _someone can predict_ the future state of another variable."

Yes, WE do the predicting. But the special kind of relationships that permit us to do this with some success are in fact "out there" in the real world (or at least in what we take to be the real world, or perceptions). Shouldn't such relationships be called something, to make discourse clear and simple?

>But to use the phrase that way implies that one is always conscious of >the fact that the prediction is being made by some being on the basis >of certain assumptions and with the help of symbol-manipulating >processes. In short, a prediction is always a _subjective judgment_. If >we realize this, we might fall into the use of terms like "predictive >variable," but we will never be under the impression that the variable >itself, with no help from a human being, can make predictions.

To say that a signal is predictive is only to say that its relationship with another signal is such as to permit predictions to be made. This, it seems to me, is the ordinary sense in which ordinary people understand the term.

What gets confusing is when you insist on adopting an idiosyncratic, literal, animistic meaning and don't bother to cue your poor correspondent that this is the way you are interpreting his words. I use the term "predictive" because I know of no clearer way to communicate the relationship I have in mind, knowing full well that is is only a shorthand. If you have a better term, let's hear it.

Given what I have said above, you may be surprised to hear that I appreciate the merits of your argument for clear language. In other contexts, I have offered similar arguments; for example, I have attempted (without much success, I might add) to impress on students and colleagues that the term "reinforcement" refers to an effect on a response and not on a person or animal, that for this reason it is not appropriate to speak of, for example, "reinforcing the rat." Technical terms are coined so they can be given a precise meaning and thus facilitate communication; to use them loosely and improperly destroys their value. But until you give me a better vocabulary with which to describe these relationships, I

will continue to find using terms like "predictive signal" a convenient shorthand. It shouldn't pose a problem if we can agree on what is meant.

Regards, Bruce

Date: Thu Apr 13, 1995 1:02 am PST
Subject: Re: anticipation; qualitative cues

<[Bill Leach 950412.18:07 U.S. Eastern Time Zone]
>[Bill Powers (950411.08445 MDT)]

I suppose that I might not have read what you said correctly but I took you to mean that such things do not happen at all. They are probably more common than you seem to be giving them credit for but even if true, it does not in my mind change anything about the phenomenon of control.

Another, more common, example of a situation where force is what is controlled and not position is in the use of a hand truck (again with a heavy load). I would also suggest that in the use of many hand tools, the 'thing' that is controlled (primarily) is again force and not position.

I don't see any problems with these. One might refer to 'anticipation' but in my simple mind it is still a matter of a high level control system sending references to lower-level systems. An error in any of these systems will result in poor control IF the error is not corrected quickly enough.

I think that all of the problem with 'anticipation' is really an example of thinking in terms of "advanced calculations" to provided controlled output as opposed to recognizing that for the vast majority of situations a reference setting error will result in a higher level system error which resets the original reference that was the source of the error. If due to the physics of the situation this correction does not occur quickly enough, then the subject notices the error and may even loose control of the goal entirely.

We 'anticipate' things because of experience. While we might not have much of a 'handle' on how this works, we really don't know exactly how we learn to walk either and I sorta figure that one envolved a bit of 'anticipation' too.

-bill

Date: Thu Apr 13, 1995 1:16 am PST
TO: * Purposeful Leadership / MCI ID: 474-2580

<[Bill Leach 950412.19:51 U.S. Eastern Time Zone]
>Message: 67287 on Wed, 12 Apr 1995 07:39:04 -0500
>Author : Wayne A Hershberger <tj0wah1@CORN.CSO.NIU.EDU>

Wayne, I don't know what happened by your "MIME" encoded message arrived inside a "MIME" encoded message and thus was unreadable on my system.

If that were not bad enough, there were THREE MIME headers present.

-bill

Date: Thu Apr 13, 1995 1:17 am PST
Subject: Re: Medical science and modeling

<[Bill Leach 950412.18:46 U.S. Eastern Time Zone]
>[Bruce Buchanan (950411.23:55 EDT)]

>practictioner/researcher

OK, to you that is not much difference... to me it is significant but I will agree at least that your rough descriptions were good.

>For science, everything in the world is potentially related to ...

This is more a philosophy arena than science.

>... we select items, ..., choose variable features which appear ...

This becomes science when we model the phenomenon and stress the model.
Also, "F = Ma" and "E = MC^2" ARE models.

>But observations, ... etc. are required *before* and *as a basis ...

Agreed.

>For no matter how well modeling ... not ... whole story.

Agreed.

>... *the model _is not_ the phenomenon*, and a model can be no more ...
>is no guarantee that you really do "understand the phenomenon" itself.

Agreed, but if you can not model the phenomenon then you are in grave danger of equating correllation to causality -- a VERY common problem in today's "science". Admittedly, just "making" a model is no improvement. The model absolutely must be stressed and tested rigorously.

>... Pentagon ...

You have heard the oxymoron "Military Intelligence", yes?

>Serious inquiries into nature (e.g. research) take evidence from
>wherever it appears, to study or examine it logically and in relation
>to other evidence. Sometimes strict formal relationships, as in the
>mathematical laws of gravitation, can be found. Sometimes the only
>useful models are jerry-built through iterative trials in relation to
>multifactorial changing environments (e.g. economics, ecology).

Alchemy was science of the time. Serious researchers without models will create "thought models", "test" and them and use them to direct further research. At some point the presumed knowledge reaches a level where some aspects of the research can be modeled in the strict sense. When this is possible real scientists will begin doing just that.

It is when strict modeling becomes common that knowledge and understanding in a field "explodes". The soft-sciences have had a veritable explosion in INFORMATION but no explosion in the amount of understanding. That explosion will have to wait for modeling and indeed there has been a sharp increase in modeling attempts for some areas.

>But also let me say that, for a person who claims that PCT and perhaps
>science must be "value-free" it is paradoxical that you have some
>*ideal* in terms of which your are judging methods of reaching truth.
>The question may be asked: On what grounds do you judge that a model is
>the best or ideal standard of truth? I am not criticizing you for making
>such judgements. I am just pointing out that the question of higher
>level criteria is a legitimate one, and at the least it is not being
>consistent to deny that you are applying higher level values or
>controlled variables when in fact your must do so.

PCT is morally and ethically neutral. In the usual meaning of the term "value" it is value free. I have never claimed that *I* am value free! I certainly have never claimed NOT to have opinions -- If I had done so, the laughter would have crippled the internet for years!

That our understanding of the physical reality as we perceive it has improved due to scientific method at a rate so great as to not be comparable to the observe/conjecture/believe method is sufficient reason.

>... grounds ... humility ... man's ultimate capacities ... for >knowledge.

I don't agree but then that is not a science question (at least not one that science can answer at this stage).

No apology needed as I did not view your posting as "long winded".

-bill

Date: Thu Apr 13, 1995 1:24 am PST
Subject: Re: S-R language, Anticipation, etc

<[Bill Leach 950412.19:24 U.S. Eastern Time Zone]
>[Rick Marken (950411.2200)]

It occurred to me that a "generated output" mindset is potentially interfering in this discussion.

There is absolutely nothing wrong with 'anticipation' as long as one recognizes that a current perception that is perceived to relate to a reference results in the generation of references for lower level systems (including sequence and/or 'program') which in turn create reference values for yet lower level systems.

I mentioned walking in a post in response to one of Bill P.'s messages. The 'simple' act of walking is an excellent example (I think) of the sort of thing that we mean when we talk about 'anticipation' except that we all do it so automagically that we seldom think about the matter.

The human body walking was once described as controlled falling. This is certainly not far off the mark if off at all. In a sense, we anticipate the fact that we will fall on our face if we don't take some action.

The control program that "handles" walking, sets various references for sequencers. Many perceptions are involved and certainly the "balance" input perception from the inner ear is a very important one to this complex control process. But once again, it is not the outputs that are a result of 'anticipation'. The outputs are still a result of difference between perception and reference.

Anticipation in this instance resulted from some very complex learning process related to moving about as an infant with no doubt additional refinements going on for some years.

If you have ever "run up" a flight of stairs where one step was out of line by more than about an 1/8 of an inch, you have experience an 'anticipation' error in a process that quite normally proceeds without thought. The same sort of experience occurs when you are off on the number of steps when it is too dark to discern that you are at the landing.

We may be a bit premature in even trying to consider modeling this phenomenon but I suspect that it envolves multiple parallel control systems with very non-linear error or output gain.

In the simple examples of lifting, there are certainly references for both force and position. Even if the 'intent' is only a position change of some object, the application of force to achieve the goal is not simple.

I know that mention has already been made concerning such ideas as gradual change in position reference but is it really likely that only one of several related perceptions would be used to effect control?

-bill

Date: Thu Apr 13, 1995 2:53 am PST
Subject: Re: Anticipatory Output

[From Bruce Abbott (950412.2025 EST)]

>Bill Powers (950412.1105 MDT) --

>This implies that the model must have available to it an assortment of
>inputs representing the states of environmental variables, and some way
>of perceiving a regular temporal relationship between any of these
>variables and the controlled variable in question (the basis, as you
>said, of an "unconditional response"). Then there must be a mechanism,
>preferably simple, for gradually incorporating the variable that most
>reliably precedes the occurrance of the disturbance into the perceptual
>function that detects the state of the controlled variable. This
>mechanism must work with ALL of the possible indicators of the

>disturbance, because in principle any one of them might turn out to be
>the signal that is needed, at the experimenter's whim.

>Perhaps the critical variable here is the error signal in the control
>system, because the basic problem is that the normal response to a
>disturbance of the controlled variable is not keeping the error
>sufficiently small. What is needed is a mechanism that will be activated
>by large error signals, and as a result will start bringing various
>inputs into the definition of the perceptual signal and keep doing this
>until an input is found that will result in reducing the error. I am
>currently favoring this approach, because it can also account for the
>fact that a particular variable is being controlled -- in other words,
>the "anticipation" aspect of the system is just an extension of the same
>process that resulted in a variable being controlled in the first place.
>If that could be achieved, we would have a very economical model of
>classical conditioning.

Absolutely perfect--a great start on the problem. I would suspect that the various inputs being "brought into the definition of the perceptual signal" are not tried at random; I envision a mechanism that "finds" those inputs that have the "correct" (predictive) relationship, a sort of contingency detector. Only those perceptual variables that change in the proper time relationship to the disturbance would be added to the input transfer function. It seems to me that such a contingency detector could be constructed easily from simple neural elements.

Regards, Bruce

Date: Thu Apr 13, 1995 5:13 pm PST
TO: * Purposeful Leadership / MCI ID: 474-2580

This message is in MIME format. The first part should be readable text, while the remaining parts are likely unreadable without MIME-aware tools.

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Content-Transfer-Encoding: BASE64
Content-ID: <Pine.3.89.9504120704.C27964@corn.cso.niu.edu>
Content-Description:

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Date: Fri Apr 14, 1995 6:54 am PST
Subject: CSG-L Digest - 12 Apr 1995 to 13 Apr 1995

There are 9 messages totalling 636 lines in this issue.

Topics of the day:

1. EEG, b3dpbmcgaW
2. your mail
3. An effect on a response?? (2)
4. anticipatory output
5. Predictive control
6. effect of reinf.; timing is all
7. Pedictive control system
8. predictive control: rate plus proportional perception

Date: Thu, 13 Apr 1995 07:58:12 -0500
From: Wayne A Hershberger <tj0wah1@CORN.CSO.NIU.EDU>
Subject: Re: EEG, b3dpbmcgaW

On Wed, 12 Apr 1995, Richard Marken wrote:
\>
\>Wayne A Hershberger (950412) says:
\>
\>\>dmVseSBvbiB0aGUgbmV0KSwgdGhhdCBJIGFtIGxlZnQgYSBiaXQgZGl6enku
\>\>ICBIb3dldmVyLCBJIGFtDQpmb2xsb3dpbmcgaW4gdGhlIHNlbnNlIHRoYXQg
\>\>SSBjYW4gc3RpbGwgc2VlIHlvdXIgZHVzdC0tYW5kIHdoZW4geW91IGFsbA0K
\>
\>To which I can only reply:
\>
\>lZHU+DQpUbzogQnJ1Y2UgQ ;-)

Pithy. Rick, you have such a way with words! :-)

As for the rest of you guys, I apologize for the hieroglyphics. I am using communication software that is new to me. I see that some of it is incompatible witn the CSG-net. Sorry.

Regards, Wayne

Date: Thu, 13 Apr 1995 08:15:31 -0500
From: Wayne A Hershberger <tj0wah1@CORN.CSO.NIU.EDU>
Subject: Re: your mail

On Wed, 12 Apr 1995 bleach@BIX.COM wrote:

>Wayne, I don't know what happened by your "MIME" encoded message arrived
>inside a "MIME" encoded message and thus was unreadable on my system.

Bill, does this mean that you and others can decode a MIME encoded message, if I but do it correctly at this end? Thanks for your help.

Regards, Wayne

Date: Thu, 13 Apr 1995 08:41:28 -0700
From: Richard Marken <marken@AEROSPACE.AERO.ORG>
Subject: An effect on a response??

[From Rick Marken (950413.0845)]

Bruce Abbott (950412.1055 EST)--

>I have attempted (without much success, I might add) to impress on >students and colleagues that the term "reinforcement" refers to an >effect on a response and not on a person or animal, that for this reason >it is not appropriate to speak of, for example, "reinforcing the rat."

But now that you understand PCT I'm sure that you also try to impress on students and colleagues that "reinforcement" (the apparent effect of an environmental variable on a response) is an illusion. Right?

Best Rick

Date: Thu, 13 Apr 1995 10:21:13 -0500
From: Wayne A Hershberger <tj0wah1@CORN.CSO.NIU.EDU>
Subject: Re: anticipatory output

[From Wayne Hershberger]

(Bill Powers (950412.1105 MDT)
>
>Wayne Hershberger (950412) --
>

> Bill, one of the higher order systems that might be involved in
> this process is one that tries to control generalized or pooled
> error and whose output tweaks gain and delays in "subordinate"
> loops--or, perhaps calibrates the "scale" of their input or output
> transfer functions. The disturbance of any sluggish lower-order
> loop would also constitute a disturbance to any higher-order loop
> controlling some overall level of pooled error; the anticipatory
> signal would not need to do that job. The pooled error could have
> a long integration interval and a long loop time.
>
>Yes, a real possibility.
>But that's not the same as the situation that Bruce Abbott proposed,

Right. When speaking about recalibration I was unwittingly thinking (?) about perceptual adaptation--a related but different matter than classical conditioning: discrete anticipatory outputs. I realized this some time after I had sent my post and intended to withdraw that particulr suggestyion from consideration, but I see you're ahead of me.

>As to _why_ we learn to do this, your explanation may have some
>relevance. Presumably we begin by suffering the consequences of
>unpredicted disturbances which we can't correct with normal efficiency.
>So this would lead to a lot of "generalized or pooled error" in systems
>at different levels. It could even lead to intrinsic error and start
>reorganization going.

Yes, I think so, both for classical conditioning and for perceptual adaptation. But, for now, we are considering the former phenomenon, particularly, discrete anticpatory output that is synchronized with a discrete environmental disturbance.

>The solution to this generalized error is to
>search for some reliable indicator that the disturbance is going to
>happen, so it can no longer take us by surprise.

Yes. Sort of. Pavlov's dogs could anticipate getting food powder when they were strapped into the experimental harness, but they could not anticipate EXACTLY WHEN unless a forewarning signal preceded the blast of powder. Timing is everyting.

>In my preliminary attempts to model classical conditioning along the
>lines you suggested, I ran into a problem which is still under
>consideration. What Bruce Abbott was describing at one point was
>literally a signal -- a flash of light, say, arranged by an experimenter
>to occur some time before a shock delivered through a floor grid. What
>the rat would learn would be to jump just before the shock.

I think this is simple avoidance as illustrated in your Crowd program. Avoidance is not a problem for PCT; it's a problem for S-R-reinforcement theory. The S->R->reinforcement people must explain avoidance as escape from some fear eliciting stimulus because the ABSENCE of a shock could not reasonably be expected to reinforce the avoidance resopnse in question (nothing doing something? just think about all the bad things that are not happening when we do nothing--we should all be as vegetative as trees.) So, fear is said to be a response classically conditioned to a signal and the animal learns how to escape the noxious fear.

Fear, like perceptual adaptation, may be related to the sort of discrete anticipatory output that concerned Bruce but I think we should put fear on the back burner for now.

>Perhaps the critical variable here is the error signal in the control
>system, because the basic problem is that the normal response to a
>disturbance of the controlled variable is not keeping the error
>sufficiently small. What is needed is a mechanism that will be activated
>by large error signals, and as a result will start bringing various
>inputs into the definition of the perceptual signal and keep doing this
>until an input is found that will result in reducing the error. I am
>currently favoring this approach, because it can also account for the
>fact that a particular variable is being controlled -- in other words,
>the "anticipation" aspect of the system is just an extension of the same
>process that resulted in a variable being controlled in the first place.
>If that could be achieved, we would have a very economical model of
>classical conditioning.

Yes, particularly the error signal in the lower-order system, particularly abrubt increases in that signal, because timing is everything.

Best regards, Wayne

Date: Thu, 13 Apr 1995 11:43:52 -0700
From: Richard Marken <marken@AEROSPACE.AERO.ORG>
Subject: Predictive control

[From Rick Marken (950413.1145)]

Bill Leach (950412.19:24) --

>There is absolutely nothing wrong with 'anticipation' as long as one >recognizes that a current perception that is perceived to relate to a >reference results in the generation of references for lower level >systems (including sequence and/or 'program') which in turn create >reference values for yet lower level systems.

Yes, indeed.

I have written a model of a simple predictive control system that works just great. It works like this:

```
         |-->|ct+(ct-pt)| --r
         |                  |
         |                  v
         |            ---->|C| ----
         |            p            |
         |            |            |    System
________|f|__________|i|__________|o|_________________
         |            ^            |
         |            |            m    Environment
         t            c<-----------|
                      ^
                      |
```

d

This is a model of pursuit tracking; t is the visible target; c is the cursor, which the subject controls; d is a disturbance to the cursor. The prediction in this model is incorporated into the computation of the reference signal (r) but it is equivalent to putting it into the perceptual function (i). The prediction is a simple, linear prediction. The system is assumed to have a perception of the current target position, ct, and of the target position at some fixed time in the past, pt (in the simulation pt was the target position at one sample interval prior to the present). The reference signal is the predicted position of the target in the next sample period assuming that it continues to move in the same linear path; r = ct + (ct-pt). So the cursor control system is controlling the cursor relative to a reference that represents a continuslly revised, linear extrapolation of cursor movement.

This predictive control system improves tracking performance if the cursor control system is sluggish (a small k value for a linear integrator). The prediction "makes up" for this sluggishness, even though the prediction is not always accurate. The improvment with predictive control (compared to just using ct as the reference input to the cursor control systems) can be substantial; the rms tracking error with predictive control was sometines 1/2 what it was without prediction (r = ct); the level of improvement depends on the sluggishness of the cursor control system.

If the cursor control system is not sluggish (if the k for the integrator is about .8) then the predictive controller typically does worse than the controller without prediction; with a good cursor controller, the occasional mispredictions show up as increased rms error.

By the way, these results apply whether the target movement was sinusoidal of low pass random, which is not surprising for this prediction algorithm.

Note that this is just a good old fashioned control systen; the output (m) is not anticipating anything; it's just doing what's necessary to keep p = r. The r value represents an estimate of a future position for t; if r had come from a higher level system, I suppose it could be called an anticipated output. But if r were the output of a higher level system, it would be continuously changing (as necessary) to keep that higher level perception under control. The error in the higher level system would determine the "degree of anticipation" (the could be varied in terms of the time difference between pt and ct) represented by r. I'll try out this two level model tonight; the higher level system will just control t-c, keeping it at 0. Variations in the "degree of anticipation" represented by the output of this system will be necessary if there are continuous changes in the slugishness of the lower level (cursor control) loop.

If this two level system works (and I'm sure it will), I think I will have proved to myself that there is anticipation in control systems; and that it can be used as a variable aspect of a loop that is controlling perceptions. I think this will also prove that Bruce Abbott was right about "anticipation"; anticipation can contribute to control of perception. So modelling can have it's up sides and its down sides;-)

Best Rick

Date: Thu, 13 Apr 1995 13:10:34 -0600
From: "William T. Powers" <POWERS_W%FLC@VAXF.COLORADO.EDU>
Subject: Re: effect of reinf.; timing is all

[From Bill Powers (950413.1115 MDT)]

Wayne Hershberger (950412)--

RE: effect of reinforcement

Rick caught the phase that I skipped over. Of course it is the response that has an effect on the reinforcement, not the other way around. No action, no reinforcement. Any effect of the reinforcement on the response is strictly imaginary, whereas we can see how the response produces the reinforcement.

I think I prefer the term "action" to the term "response." I don't mind going along with conventional terms -- for example, it makes sense to speak of a response to a disturbance of a controlled variable. But just to speak of a "response" in the abstract is to ignore the external part of the loop, in which the so-called response is one of the causes of the actual input variable. To say "action" doesn't commit one to making a causal claim.

RE: timing is all

> we are considering the former phenomenon, particularly, discrete anticpatory output that is synchronized with a discrete environmental disturbance.

In the light-->shock example I made up, the critical timing is that the rat must jump into the air _before_ the shock occurs, but not so much before that it has come down onto the grid again when it occurs.

To create a true sychronization between truly discrete events is just about impossible. A discrete event has zero duration, so any timing error whatsoever -- even a nanosecond -- prevents a coincidence. Real events have a beginning, a middle, and an end; they take a finite time to occur, and there is a pattern of changes within the event. Many discrete actions are really just continuous actions produced under conditions where an impulse-disturbance is applied. Consider the "reflex response" to contact with a hot object. If the hot object is slowly brought toward the skin, the skin temperature will begin to rise and will exceed the maximum acceptable temperature before contact is made (because of heat radiation and air conduction). Long before that point, the skin will be drawn away from the hot object, so the temperature never becomes uncomfortable. I used to do this as a way of seeing if my soldering iron was hot enough yet. I'd bring it toward my cheek, and feel the radiation. I never burned myself, because as the iron moved toward my cheek, the temperature would rise. When I felt a certain degree of warmth I stopped bringing the iron closer. If at that point the iron was far enough away from my skin, it was ready to melt solder.

An apparently discrete stimulus is simply one that changes so fast that a casual observer doesn't notice the time it takes to change from zero to maximum. And while external stimulation can be removed very rapidly, the signals generated by it normally take time to die out, because the physical effects do not go instantly to zero.

Discrete actions are really impossible. Any action requires a limb to be accelerated to some velocity, then decelerated again, all of which takes time. The only way to create an apparently discrete onset of an action is to let the action cause some physical event that takes much less time to occur, such as a switch closure. Then the switch closure is used in place of an actual measure of the behavior.

The discreteness of real stimuli and responses is, in most cases I have thought about, an artifact of the external apparatus, not a characteristic of the behaving system.

> Pavlov's dogs could anticipate getting food powder when they were strapped into the experimental harness, but they could not anticipate EXACTLY WHEN unless a forewarning signal preceded the blast of powder. Timing is everything.

You're moving the anticipation back another step. The signalled event was the puff of dry food powder into the dog's mouth; the signalling event was the bell, wasn't it? When the bell sounded, the saliva would begin to flow, ahead of the time that the food powder was given. Without the bell (and assuming that just being strapped into the apparatus wasn't signal enough), the saliva would begin to flow after the disturbance occurred; after the food powder arrived. But if it started flowing before the food powder arrived, no harm was done. So synchronization was not required. I believe Bruce Abbott said that the maximum flow would occur if the bell occurred about 5 sec before the food powder. This implies that if the bell occurred longer before the food powder, the saliva flow would begin to shut down again before the powder arrived. This looks like the response of a continuous control system to a brief disturbance. The sound of the bell becomes part of the controlled variable, so either food powder or the bell can be sensed as a disturbance that is counteracted by the saliva flow.

> The S->R->reinforcement people must explain avoidance as escape from some fear eliciting stimulus because the ABSENCE of a shock could not reasonably be expected to reinforce the avoidance resopnse in question (nothing doing something? just think about all the bad things that are not happening when we do nothing--we should all be as vegetative as trees.)

Much easier when we say that the reference level for the perception is set to zero, eh? However, I think EABers would see this as an example of negative reinforcement: the behavior that removes a noxious stimulus is reinforced by the removal. No problem (when all you have to do is find the right words). Anyway, I don't think the EAB people would be allowed to say that f-word.

>>the "anticipation" aspect of the system is just an extension of the
>>same process that resulted in a variable being controlled in the first
>>place. If that could be achieved, we would have a very economical
>>model of classical conditioning.

> Yes, particularly the error signal in the lower-order system, particularly abrupt increases in that signal, because timing is everything.

I'm going about this in a somewhat different way. Suppose that in the environment there is some sort of activity going on that leads up to a disturbance each time it occurs. An example is a teapot on a stove. As the water heats up, the water starts to sizzle, then blurp occasionally, and finally, just before it goes into a rolling boil, it begins to produce a characteristic continual bubbling sound. If you wait too long after that, hot water starts spitting out of the spout and you have to clean up the counter, and maybe get burned.

If you wait for the water to start spitting out, it's too late; no matter how fast you snatch the pot from the fire, the spitting will go on for ten or fifteen seconds, making a real mess. (Mary would say don't fill the teapot so damned full, but what does she know about cooking?). So the problem is which precursor sound to use as a signal to turn down the heat or remove the teapot. Only I want to solve the problem without talking about signals to do things, which is just the S-R solution.

Let's suppose that there is no reasoning involved, just learning from experience. In principle, the state of boiling of the water is indicated by any of a number of sounds. We control the boiling indirectly, by controlling the sounds. So let's just start varying the weights assigned to all the different sounds in constructing a perception to control as a means of controlling water temperature. If the water isn't hot enough, or gets too hot and starts spitting out, vary the weights some more. Eventually, a perception will be constructed based on the weighted and time-averaged sum of all the perceptions that occur around the time of boiling. Bringing this perception to the right reference state (probably zero, in this case) will result in water of the right temperature.

The water-temperature error will be minimized when high weights are given to the sounds occurring at a certain time before the spitting would begin. Which perceptions those would be depends on the basic control action -- whether it's removing the pot from the burner or turning the temperature down to simmer. If you prevent the spitting by turning down the heat, you have to give high weights to perceptual components that occur earlier in the process, because it takes time for the heat input to drop.

So anyway, this approach yields something that looks just like classical conditioning, including the timing -- but nothing is doing any timing. All that's happening is that we're altering the nature of the controlled perception until we get the desired result at a higher level: hot water with no spitting.

I think this model will work using the e-coli type of reorganization to vary input weights for a perceptual function. We will automatically get an error-correcting action that starts before the main variable is actually disturbed, by just the right amount of time. There is no one specific environmental event that serves as "the signal." There are simply perceivable processes that occur on the way to producing the main disturbance. In a real environment, where many such perceivable processes may be going on at the same time, the organism has a wide choice for constructing a suitable perception. One person might use the sounds of boiling, another the sight of wisps of steam starting to come from the spout. Another might just look at a clock. There's no unique precursor event. The person will experiment with precursor events until one is found that permits producing the intended outcome.

In the laboratory, of course, there is only one precursor event, so it's pretty sure that the final perception will include it as a component. The situation is rather like that in a operant-conditioning cage, where only one action will produce the food, so that action is learned.

Best, Bill P.

Date: Thu, 13 Apr 1995 16:53:01 -0500
From: Bruce Abbott <abbott@CVAX.IPFW.INDIANA.EDU>
Subject: Pedictive control system

[From Bruce Abbott (950413.1650 EST)]

>Rick Marken (950413.1145) --

>I have written a model of a simple predictive control system that works >just great. It works like this:

```
>
>       |-->|ct+(ct-pt)| --r
>       |                  |
>       |                  v
>       |            ---->|C| ----
>       |            p            |
>       |            |            |    System
>______|f|__________|i|__________|o|________________
>       |            ^            |
>       |            |            m    Environment
>       t            c<-----------|
>                    ^
>                    |
>                    d
```

This model handles a slightly different situation from the one I've been talking about, but the principle is the same. The situation I was thinking of would use the state of a SECOND perception whose changes precede those of the disturbance to anticipate the changes in disturbance; here the "second" perception is a property of the disturbance itself, the velocity of the target, which is used by the model to compute (anticipate) the future position of the target. (The target's position is being changed by a disturbance not shown in the above diagram.)

>Note that this is just a good old fashioned control system; the output >(m) is not anticipating anything; it's just doing what's necessary to >keep p = r.

Yep.

>If this two level system works (and I'm sure it will), I think I will >have proved to myself that there is anticipation in control systems; and >that it can be used as a variable aspect of a loop that is controlling >perceptions. I think this will also prove that Bruce Abbott was right >about "anticipation"; anticipation can contribute to control of >perception. So modelling can have it's up sides and its down sides;-)

Funny, it looks all "up sides" to me! I especially liked this part:

>Bruce Abbott was right

(;->

Regards,   Bruce

Date: Thu, 13 Apr 1995 17:29:18 -0500
From: Bruce Abbott <abbott@CVAX.IPFW.INDIANA.EDU>
Subject: Re: An effect on a response??

[From Bruce Abbott (950413.1725 EST)]

>[From Rick Marken (950413.0845)] --

>>Bruce Abbott (950412.1055 EST)

>>I have attempted (without much success, I might add) to impress on >>students and colleagues that the term "reinforcement" refers to an >>effect on a response and not on a person or animal, that for this >>reason it is not appropriate to speak of, for example, "reinforcing the >>rat."

>But now that you understand PCT I'm sure that you also try to impress >on students and colleagues that "reinforcement" (the apparent effect of >an environmental variable on a response) is an illusion. Right?

But of course! (Pass the Gray Poupon...)

>Bill Powers (950413.1115 MDT)]
>
>Wayne Hershberger (950412)--
>
>RE: effect of reinforcement
>
>Rick caught the phase that I skipped over. Of course it is the response
>that has an effect on the reinforcement, not the other way around. No
>action, no reinforcement. Any effect of the reinforcement on the
>response is strictly imaginary, whereas we can see how the response
>produces the reinforcement.

I couldn't find where Wayne had said anything like this, so I assume that
Bill really meant to refer to the same post Rick was responding to above. (?)

Not to worry; I haven't reverted to oldthink. I'm talking about how that term is defined by traditional reinforcement theorists. Anyway, that was in a former life. (;->

Regards, Bruce

P.S. Rick: when will we be seeing your post of the Turbo Pascal source code of your "anticipation" model? I can't wait to run it.

Date: Thu, 13 Apr 1995 20:33:11 -0600
From: "William T. Powers" <POWERS_W%FLC@VAXF.COLORADO.EDU>
Subject: Re: predictive control: rate plus proportional perception

[From Bill Powers (950413.1825 MDT)]

Rick Marken (950413.1145) --

Your two-level system with "predictive control" is not only feasible, it suggests a generalization of the idea of the "predictive stimulus." At the moment this generalization is a hazy blob in my head, but perhaps if I just start writing it will take shape.

Let's look at the input and comparison relationships in your diagram:

```
         |-->|ct+(ct-pt)| --r
         |                  |
         |                  v
         |             ---->|C| ----e
         |             p            |
        [t]            |
                      [c]
```

The first-level error signal e is (ct + (ct - pt)) - cc where cc means current cursor position. Note that because the current cursor position is subtracted, the output should be a positive number times the error signal.

Now let's look at another (one-level) system, where the perceptual signal is ct - cc and the reference signal is zero. If we add a first-derivative component to the perceptual signal, we have

p = (tc - cc) + d/dt(tc - cc).

As in your model, the first derivative is roughly equal to the current value minus the past value (one iteration ago). So we have

p = tc - cc + (tc - tp) - (cc - cp), or

```
p = tc + (tc - tp) - cc - (cc - cp)
                        ^^^^^^^^^^^
```

Since the cursor position is being subtracted in the input function, the comparator needs an output that is positive when the cursor goes negative, to preserve negative feedback. So the comparator function must

be r + p, and a positive reference signal means cursor less than target position. In this system, therefore, e = p rather than -p when the reference signal is zero.

The only difference between your system and the second one, therefore, is that we have the first derivative of the cursor position (marked with ^^^^^^) as well as the cursor position being subtracted.

What all this leads up to is that you have modeled (nearly) the case in which the perceptual function represents the controlled variable PLUS its first derivative. The first derivative puts a phase advance into the control loop, which partially cancels the phase lag in the integrating output function. This is why the RMS error declines. It would decline even more if you used a one-level system, as in the second equations, where the perceptual variable is tc - cc + Kd*d/dt(tc - cc), Kd being an adjustable constant. In principle, if you took the derivative in a more precise way (using first, second, etc. differences), you could adjust the amount of derivative feedback to just cancel the integrative lag in the output function, and have the error corrected in one or two iterations.

Ok, that much wasn't blobby to start with.

Now the blobby part. What about a perceptual variable that always precedes the controlled variable, but isn't directly derived from it as a first derivative would be? Here's the cute twist. In effect, the preceding variable is analogous to the first derivative of the other variable. More or less. It provides a signal that rises before the main variable rises. Being added to the perception of the main variable, it gives the effect of a first derivative in the perceptual path. This would be more obvious if the variables were all continuous variables instead of step-functions or impulses, as they are in typical conditioning experiments. But if you can see the discontinuous case merely as an extension of the continuous case, I think the principle turns out to be the same. Sort of.

By adding only the derivative of the target position, what you did was to find a case that is almost like first-derivative feedback from the controlled variable, but which applies only to one of the two variables, the target position but not the cursor position. If you used an adjustable Kd for the difference signal, I think you could make the RMS error even smaller than what you've found, although I don't know how close to zero you could get it.

I guess the generalization is still mostly a blob, but worth taking out and looking at now and then.

Best, Bill P.

End of CSG-L Digest - 12 Apr 1995 to 13 Apr 1995
****

Date: Sat Apr 15, 1995 9:11 pm PST
Subject: CSG-L Digest - 14 Apr 1995 to 15 Apr 1995

There are 11 messages totalling 662 lines in this issue.

Topics of the day:

1. physical reality
2. Classical Conditioning (3)
3. physical reality; models (2)
4. Anticipating Gray Poupon
5. Control vs Anticipation
6. Predictive control
7. Pedictive control system
8. MIME encoding; why?

Date: Sat, 15 Apr 1995 14:17:50 -0500
From: Bruce Buchanan <buchanan@TOR.HOOKUP.NET
Subject: Re: physical reality

[From Bruce Buchanan (950415.14:20 EDT)--

[Bill Powers (950414.1830 MDT)]

> A great many of my arguments will make more sense to you if you simply assume that I am NEVER talking about real reality the way it really is.

Understood. In any case it is impossible to talk about "real reality" because it is not something to which language can be applied. I thought that I was very clear that this was basic to any discussion of this topic, including mine.

> To human beings, perceptions ARE the world.

Agreed.

> The hardest thing to realize is that the "external world," the "perceptual world" and "the intellectual world" are ALL IN THE SAME PLACE, in our heads. They are all ideas that exist in human experience; they are simply different subdivisions of experience. They are ways of classifying experiences, but otherwise they are all of a piece.

This is "hard to realize" for me because I do not think it fits the facts as I see them. Indeed, I think it is the product of a mistaken or default metamodel of the _relationships_ of these worlds. Since there are radical distinctions which make each of them unique, I do not see how they can be "all of a piece". I will not repeat the reasons I have already given for a view which helps resolve some of the conundrams.

>You say

>> This statement by Bill seems to me to assume that the external (unknowable) world is identical to or indistinguishable from the concepts of physical science. I think that this assumption of identity is untenable.

> I hope you will now see that this is a misinterpretation of my view.

Well, I see your statements as a clarification, but that you make no reference or allowance for a relationship between the concepts of physical science and the external world, and that you see these as occupying the same place "in our heads". So I see you make a distinction, but I do not see any differences in this formulation which clarifies the relationship or the limits of applicability of concepts as such.

I do think you make an operational distinction in your experimental work, so I find this apparent inconsistency between metatheory and practice confusing. However, if I am the only one who is confused about this it is probably of little importance.

> When I speak of the concepts of physics, I am treating them in their own terms, not in relation to the Boss Reality.

Well, as I understand the concepts of physics they do not exist simply on their own terms. Their _relationship_ to Boss Reality may be considered, and I have tried to present something of Popper's ideas on this, but I am unable to see why we should not make our best efforts to elucidate that _relationship_. There are too many people who have gone a long way in examining that relationship (e.g. Popper, Margineau, Rosen, Casti) for me to accept a default position of ignorance.

> I don't have any idea what the Boss Reality is like, and I don't know anyone who does. So there's not much point in speculating about it. All we can do is use our best models.

I do not think the question is one of speculating about the nature of Boss Reality. The question is the assumptions which must be made, whether actively or by default, about the relationship between unknowable external reality and our perceptions and knowledge. In this regard I have tried to consider alternative models that may have advantages, e.g. in clarifying the processes involved in anticipation, in making it unnecessary to take the view that the unknowable external world is simply "all in our heads", and in other ways also.

Cheers! Bruce B.

Date: Sat, 15 Apr 1995 15:15:00 -0500
From: Bruce Abbott <abbott@CVAX.IPFW.INDIANA.EDU
Subject: Re: Classical Conditioning

[From Bruce Abbott (950415.1510 EST)]

>Bill Powers (950414.1055 MDT)]
>>Wayne Hershberger (950414)

>> Pavlov called the bell a conditional stimulus (CS) because its ability to elicit an increased salivary output was conditional upon its being temporally paired with the puff of powder, a stimulus which does so unconditionally

(UCS). The best CS-UCS interval for Pavlovian (classical) conditioning is generally about 1/2 second, as I recall: .5 not 5.

> Pavlov called the bell a conditional stimulus (CS) because its ability to elicit an increased salivary output was conditional upon its being temporally paired with the puff of powder, a stimulus which does so unconditionally (UCS). The best CS-UCS interval for Pavlovian (classical) conditioning is generally about 1/2 second, as I recall: .5 not 5.

The notion that "the" optimal CS-US interval for classical conditioning is about 0.5 sec is due to Kimble (1961, p. 166), but more recently others have noted evidence that the optimal interval depends on the response system involved and on the way in which the CR is measured (e.g. Mackintosh, 1972) Pavlov often used a CS-US interval of 5 to 10 seconds in salivary conditioning. In _Conditioned Reflexes_, Pavlov (1927) reports that showing food to a dog produces salivation in about 5 seconds, whereas placing food directly in the mouth produces salivation in about 1-2 seconds (Pp. 22-23). In some of my own work (Abbott & Badia, 1979) I showed that tones preceding shock by less than 1.5-2.0 seconds were less effective than longer tones in maintaining preference for a signaled over unsignaled shock schedule. However, short-latency responses such as the rabbit's nictitating membrane (eyeblink) response condition best with CS-US intervals of 250-500 milliseconds.

> This is confusing: you seem to be saying that once a conditional response had been established for a bell 0.5 second before the puff of powder, the bell could then be sounded up to seconds or even minutes before the puff, and the salivation would occur just as before, just before the puff. Wasn't some learning period required before the new conditions became effective? If the bell was sounded, how could the dog know that the puff wasn't going to occur for many seconds, instead of 1/2 second later as usual?

Initially salivation would occur soon after CS onset (and would tend to occur throughout the CS presentation), but with further training at the new CS-US interval the beginning of salivation became more and more delayed until, with sufficient training, it would occur just prior to the US. As Wayne noted, Pavlov attributed this change in response latency to a process of inhibition, in which time-correlated stimuli corresponding to the early parts of the CS-US interval developed the ability to suppress the salivation that otherwise would be elicited by the CS. As evidence he noted that a distracting stimulus presented during the early part of CS presentation could temporarily "restore" (disinbibit) the salivation.

>Well, that makes me suspect more than Pavlov's experimental methods.

I believe the phenomenon described by Pavlov as "inhibition of delay" is reliable. I don't know who told you that Pavlov was sloppy; I have a different impression. Prior to his involvement with "conditioning," Pavlov was awarded the Nobel prize in physiology for his work on digestion. All tests were conducted in a laboratory especially designed for the purpose and employed some the most advanced equipment of the day. And it was all "single-subject" research--no group averages.

>> Learning to remove a teapot from a stove before it spits is an example of instrumental conditioning: the output is instrumental in preventing the

disturbance. In classical conditioning the output prevents the disturbance's altering the controlled perception, but it does not prevent the disturbance.

> This is true, if you interpret the spitting of the teakettle (not teapot, Mary insists) as a negative reinforcer. However, if you don't know that something caused by the spitting is being controlled all you will see is that when the teakettle spits, you respond by removing the teakettle from the stove burner. This looks like an unconditional response. Whenever the teakettle spits, you remove it from the burner. There is no particular reason for removing the teakettle from the burner; you do it when the spitting-stimulus makes you do it.

> As I understand classical conditioning, the removal of the teakettle from the stove is the unconditional response; the unconditional stimulus is the spitting of boiling water which always causes the unconditional response. When a previous sound becomes able to cause the removal of the teakettle before the spitting starts, this sound is then seen as a conditional stimulus. When the conditional stimulus occurs, the teakettle is removed from the burner even if the spitting doesn't happen. That's the evidence of conditioning. Under this interpretation, the controlled variable (perhaps a mess on the stove, with a reference level of zero) isn't even considered.

The question whether classical and instrumental conditioning can be subsumed by one underlying process is not new, and at this point in our modeling efforts I would rather not worry too much about the distinction between classical and operant conditioning. As I noted some time ago (I don't want to take the time right now to dredge up the post), Bill's model of classical conditioning is a preparatory response model; such models essentially reduce classical to instrumental conditioning.

The two forms of conditioning CAN be distinguished procedurally: in classical conditioning the appearance of the US is independent of the subject's behavior; in instrumental conditioning the equivalent stimulus (reinforcer or punisher) does depend on the subject's behavior. Technically, the teakettle example fits the procedural definition of instrumental conditioning. Also, note that an unconditional stimulus is supposed to produce an unconditional response reflexively, without learning. Clearly the spitting of the teakettle does not do this. The situation would be more correctly described as one involving a discriminated operant.

Rather than concern ourselves with these distinctions and with observations that do not _appear_ to "fit" the basic PCT model (e.g., inhibition of delay), I would like to see the model developed to a state where it appears to do a satisfactory job in the basic situations (whether these are described as classical or instrumental) involving apparent "anticipation." I'd like to see that the model CAN do before we become overly concerned with trying to make it account for all sorts of additional facts.

Regards, Bruce

Date: Sat, 15 Apr 1995 15:26:02 -0600
From: "William T. Powers" <POWERS_W%FLC@VAXF.COLORADO.EDU
Subject: Re: physical reality; models

[From Bill Powers (950415.1345 MDT)]

Bruce Buchanan (950415.14:20 EDT)--

Me:
>> The hardest thing to realize is that the "external world," the "perceptual world" and "the intellectual world" are ALL IN THE SAME PLACE, in our heads.

You:
> This is "hard to realize" for me because I do not think it fits the facts as I see them. Indeed, I think it is the product of a mistaken or default metamodel of the _relationships_ of these worlds. Since there are radical distinctions which make each of them unique, I do not see how they can be "all of a piece".

The facts as you see them are, of course, as you see them. You're speaking of your own experiences, aren't you? As far as we know, everything that is part of experience must be the product of brain activity; I don't know of any other assumption that's consistent with all we know, or more properly, all I know.

It's true that with respect to the divisions of experience you mention, we talk about them in quite different ways: different vocabularies, different rules, different kinds of action. But whichever one of them we are talking about, or what relationship among them we're talking about, we're TALKING, using the same brain and generally the same types of mental operations. And furthermore, we know we are doing this because we can observe these mental activities. The only time we fail to know what we are doing is when we become so identified with the subject matter and the way of thinking that goes with it that we begin to believe the subject matter is outside us, in the world somewhere. Then we begin to get annoyed, because if these things have the obvious, self-evident, objective existence that we can so plainly see, why do other people keep coming up with other ideas? What is wrong with these other people -- are they unable to see what's right under their noses?

> Well, I see your statements as a clarification, but that you make no reference or allowance for a relationship between the concepts of physical science and the external world, and that you see these as occupying the same place "in our heads".

I do make such an allowance; in fact I have described quite a few times the reasoning that convinces me of the existence of a regular outside reality. Perhaps my reasoning seems unsatisfactory, because it has not led me to a way of saying what that outside reality IS -- what it really looks like, what its rules really are, and so on. I deduce the outside reality from the fact that there are constraints on my behavior which as far as I know (which is, after all, all I can know) are not being imposed by me. When I want to control some perception to bring it to some remembered or invented state and keep it there, I find that some actions will work and others will not. I don't know why, but that seems to be the case. There is something beyond my experience that makes one kind of actions effective and another kind ineffective. Some people have claimed that I might just be fooling myself, playing a game in which I pretend not to know that I am causing these constraints to exist. Others have proposed that there are capricious demons out there who not only change the rules all the time, but make me believe that the rules are staying the same. I find these proposals rather

silly, since their point seems to be that we can't support or deny ANY proposal, including those. I play by simpler rules.

>> When I speak of the concepts of physics, I am treating them in their own terms, not in relation to the Boss Reality.

> Well, as I understand the concepts of physics they do not exist simply on their own terms. Their _relationship_ to Boss Reality may be considered, and I have tried to present something of Popper's ideas on this, but I am unable to see why we should not make our best efforts to elucidate that _relationship_.

In order to consider the relationship between A and B, much less elucidate it, you must be able to perceive A independently of B. The concepts of physics, however, can only reveal relationships between B1 and B2 -- the A1 and A2 to which they are imagined to refer, the Boss Reality, can't be observed independently of the B's. Meter readings, the B's, show us their outputs, but not the inputs that are causing them. We can only derive laws through which we can use one set of meter readings to predict another set of meter readings.

When I say that I treat the concepts of physics in their own terms, I mean that I define force in terms of mass and acceleration, mass in terms of force and acceleration, and acceleration in terms of length and time. All the basic variables of physics are measures of a perceived world, not of the world itself. However, the basic experiences are highly reproducible and hence easily communicated through demonstration, so anyone can verify the basic relationships among these experimentally defined variables. The reproducibility means that we must be tapping into some real regularity, although it may not be in one-to-one correspondence with the regularities we experience through human senses.

> I do not think the question is one of speculating about the nature of Boss Reality. The question is the assumptions which must be made, whether actively or by default, about the relationship between unknowable external reality and our perceptions and knowledge.

That is how I understand modeling. A model makes explicit what we are assuming about the Boss Reality. A properly constructed model can behave in one and only one way, the way that follows from the qualitative and quantitative assumptions we have put into it. Once the model is defined, anyone can operate it through calculation or simulation or even just reasoning and it will do exactly the same thing every time. The operation of the model is no longer a matter of opinion, because all opinions that could affect what it does are laid out in public view as parts of the model.

What many people refer to as a "model" is, in my opinion, only a rough sketch of a possible model. We can't even know what a proposed model is until we have brought out every assumption required to make it work and made each assumption an explicit public part of the model. This is why I often say, when people ask what PCT says about some complex behavior, "I don't know how to model that yet." This sounds somewhat strange, since PCT is suppose to be a model of behavior. But what I mean is that I divide PCT into two parts: models that we can run, and models that are only proposals under development and have never been brought into a runnable form. When a model runs, I can see before me the implications of the assumptions I have made, being acted out. When I propose a model that hasn't ever been made to run, I'm only guessing what such a model would really do.

This does not mean that runnable models are necessarily right. It means only that they can generate behavior out of their own rules with no external help or interpretation. If you have a model that isn't runnable, then you have no way of knowing whether it would actually behave the way you think a system organized that way would behave. You have nothing to compare with your observations of the real system. You're only guessing. And from my personal experience I can say that such guesses mostly turn out to be wrong.

On top of that, even when a model finally runs, its behavior may be quite different from what was wanted and expected, and quite different from that of the real system (i.e., the observed system).

When you say "In this regard I have tried to consider alternative models that may have advantages... " I have to say that I haven't seen any runnable models of this sort from you. It's not hard to come up with alternative models if you're only considering one aspect of the fit to behavior that might be improved. But every time you change a model, you affect ALL aspects of the behavior of the model to some degree. What seems to be a localized improvement may well ruin the remainder of the model. There are very few aspects of the HPCT model that could be changed without ruining the explanatory power in some other respect. Each chapter of B:CP lays out many observations and requirements that have to be accounted for. That's why it took me so long to write it, something like 13 years. And even so, there is no guarantee that as we attempt to make runnable models of more and more aspects of behavior, we will not run into internal contradictions and the need for changes that will have far-reaching effects on the whole model. It's fine to consider alternative models. But the alternatives have to be evaluated in terms of ALL the phenomena that the overall model is supposed to address.

Best, Bill P.

Date: Sat, 15 Apr 1995 16:37:39 -0500
From: Bruce Abbott <abbott@CVAX.IPFW.INDIANA.EDU
Subject: Re: Anticipating Gray Poupon

[From Bruce Abbott (950415.1635 EST)]

>>Bruce Abbott (950413.1650 EST) --

>> This model handles a slightly different situation from the one I've been talking about . . . here the "second" perception is a property of the disturbance itself, the velocity of the target, which is used by the model to compute (anticipate) the future position of the target.

>>>Bill Powers (950413.1825 MDT) --

>>> By adding only the derivative of the target position, what you did was to find a case that is almost like first-derivative feedback from the controlled variable, but which applies only to one of the two variables, the target position but not the cursor position.

>Rick Marken (950414.1215) --

> This has been dawning on me independently; my system is basically one that controls a perception of position + change.

Didja ever think you were talkin', an' no one was listenin'? I didn't notice any acknowledgement of my prior and very similar comment. You go on to say:

> Nice. No anticipation; just control of perception. That's what was dawning on me as I lay there last night wondering how I could have said that Bruce Abbott is right;-)

The use of the target velocity to predict future target position can be considered a form of anticipation, since you are using the linear equation to anticipate where the target will be at time tn+1 from its position and velocity at time tn. Sorry Rick, I'm STILL right. (;-

Regards, Bruce

Date: Sat, 15 Apr 1995 15:27:00 -0700
From: Richard Marken <marken@AEROSPACE.AERO.ORG
Subject: Re: Classical Conditioning

[From Rick Marken (950415.1520)]

Bruce Abbott (950415.1510 EST) --

> Bill's model of classical conditioning is a preparatory response model; such models essentially reduce classical to instrumental conditioning.

You do say some of the most peculiar things for a PCTer. How in the world could Bill's model of classical conditioning be a "preparatory response" model? It's a control of perception model. The actions of the PCT model do whatever is necessary to keep perceptual variables under control. Sometimes this may look like "response preparation", but, of course, it's not.

> Rather than concern ourselves with these distinctions and with observations that do not _appear_ to "fit" the basic PCT model (e.g., inhibition of delay)

I think you mou missed the point of Bill's comments about "inhibition of delay". Bill said:

> Well, that makes me suspect more than Pavlov's experimental methods.

Bill was expressing suspicion, not only about Pavlov's experimental methods, but about his approach to theory too.

> Prior to his involvement with "conditioning," Pavlov was awarded the Nobel prize in physiology for his work on digestion.

A prior Nobel prize seems to be a guarantee that a person will be clueless about the nature of behavior. Do the names Edelman, Crick, Simon, Sperry, or Lorenz ring a bell? Nobel winners (PCT losers) all.

Best Rick

Date: Sat, 15 Apr 1995 15:57:32 -0700
From: Richard Marken <marken@AEROSPACE.AERO.ORG
Subject: Control vs Anticipation

[Rick Marken (950415.1600)]

Bruce Abbott (950415.1635 EST) --

> **The use of the target velocity to predict future target position can be considered a form of anticipation, since you are using the linear equation to anticipate where the target will be at time tn+1 from its position and velocity at time tn.**

In the "predictive control" system that I built (and that Bill elaborated) a present time perception, p = (t-c) + Kd/dt(t-c), is under control. There is no "anticipation" in the sense in which I thought you were using ther term. I thought "anticipation" occurs when a system produces an output before an anticipated disturbance. That is, o(t) is an output that begins to compensate for d(t+delta t). There is no anticipation of this sort n the system that controls (t-c) + Kd/dt(t-c). But if you would like to call it "anticipation" then feel free. That way, you will be "right" and, lord knows, you could use some of that;-)

Best Rick

Date: Sat, 15 Apr 1995 21:01:21 -0400
From: bleach@BIX.COM
Subject: Re: Predictive control

<[Bill Leach 950415.10:52 U.S. Eastern Time Zone]
>[Rick Marken (950413.1145)]

Great Rick. I don't know how well things will go for me over the next week or so - limited accuracy in my "prediction" control system. :-)

I will try getting a machine set up for some code generation and at least now have the physical means of doing so (though still no PC or Mac boxes).

Will be interested in see the results of your model. The idea that "anticipation" exists as a phenomenon is something that is not really argueable. Given a little thought, we all "know" that something is going on where we "alter" control based upon something different then just the perception of the CEV.

Your explaination of how your pursuit tracking model works is I think, an adequate explaination for the appeerent phenomenon. I also think that a 'simple' extension (I almost gagged at using the word simple here) would demonstrate 'predictive' control where the 'cue' is NOT necessarily "tightly" related to the CEV.

The fact that humans are "adaptive controllers par excellance" is pretty well appreciated at the Macro level but except for PCTers I don't think that the capability at the Micro level is even at a beginning stage of appreciation. PCTers, in their modeling, have already had some experience with the amazing capability of simple control loops. The 'extensions' that I am thinking of are NOT, in my mind anyway, an extension to the theory but rather an extension of the application of the theory. It is still all control and control in the PCT sense. Any "anticipation" or "prediction" that is observed is still just the effect of a control system controlling current perception to current reference.

-bill

Date: Sat, 15 Apr 1995 21:01:31 -0400
From: bleach@BIX.COM
Subject: Re: Pedictive control system

<[Bill Leach 950415.11:13 U.S. Eastern Time Zone]
>[Bruce Abbott (950413.1650 EST)]

Yes, I think that the essence of what you are talking about is that there are uncontrolled perceptions whose values can be used to 'improve' the control of a controlled perception.

The velocity of the ball is probably NOT a good example of such since it is likely to be the relative velocity that is the perception of import and that one is under control. I can not at the moment think of a good example (too many other duties pressing just now).

-bill

Date: Sat, 15 Apr 1995 22:59:49 -0500
From: Bruce Buchanan <buchanan@TOR.HOOKUP.NET
Subject: Re: physical reality; models

[From Bruce Buchanan (950415.23:00 EDT)]

Bill Powers (950415.1345 MDT)

Thank you for your very thoughtful notes.

You write:

> When I say that I treat the concepts of physics in their own terms, I mean that I define force in terms of mass and acceleration, mass in terms of force and acceleration, and acceleration in terms of length and time. All the basic variables of physics are measures of a perceived world, not of the world itself. However, the basic experiences are highly reproducible and hence easily communicated through demonstration, so anyone can verify the basic relationships among these experimentally defined variables. The reproducibility means that we must be tapping into some real regularity,

although it may not be in one-to-one correspondence with the regularities we experience through human senses.

I agree with this, and I think that some apparent differences in views as stated stem from inherent difficulties in communication, based in part, as you point out, on different individual experience. I may be more inclined to attach importance to significant metaphors that will never make the grade as formal models, but obviously such choices stem from evaluations based upon personal experience.

Cheers and best wishes.

Bruce B.

Date: Sun, 16 Apr 1995 00:38:24 -0400
From: bleach@BIX.COM
Subject: Re: MIME encoding; why?

<[Bill Leach 950416.00:16 U.S. Eastern Time Zone]
>Message: 77541 on Fri, 14 Apr 1995 17:37:13 -0500, in reply to:
>Author : Wayne A Hershberger <tj0wah1@CORN.CSO.NIU.EDU

> Not because I want to!!!

Ok. MIME is a rather common mail format and transfer protocol and is becoming more common. However, it is intended to be used between MIME aware host systems. In my particular case, my system and the host that I use for internet access both understand MIME.

Thus, my system always requests that my host send mail in MIME format. I NEVER send mail to the host in MIME because my system itself is not a host system. I am not even sure that I could do so without experiencing the same results that you obtained.

In any event, the only way to send MIME encoded mail to a host is to be sure that the host system realizes that it is about to receive MIME encoded mail so that it will 1) not re-encapsulate the MIME posting into yet another MIME message and 2) will automagically decode the MIME format when sending to non-MIME aware systems.

Bill P. mentioned 8 bit but I think that MIME actually is capabile of handling full international character sets.

-bill

Date: Sun, 16 Apr 1995 00:38:38 -0400
From: bleach@BIX.COM
Subject: Re: Classical Conditioning

<[Bill Leach 950416.00:23 U.S. Eastern Time Zone]
>[Rick Marken (950415.1520)]

Rick to Bruce A.
> You do say some of the most peculiar things for a PCTer. ...

I may be wrong (and hope that Bruce will straighten me out if so...) but I believe that Bruce was refering to Bill's description of "classic conditioning" and not Bill's PCT models.

> inhibition of delay

Seems to me that the "observed inhibition" of delay is something that control theory suggests will happen. That is, if the experimental subject is given a "cue and then food" and experiences this with sufficient frequency at a regular interval AND then the interval is lengthened (but again experienced frequently enough) a control system would be expected to "hold off" on salivating until just prior to the "anticipated" arrival time of food.

As to the additional but unrelated disturbance... it seems reasonable that such disturbance could disrupt whatever "timing mechanism" existed but the perception of the arrival of food would still exist.

-bill

End of CSG-L Digest - 14 Apr 1995 to 15 Apr 1995
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Date: Sun Apr 16, 1995 9:56 pm PST
Subject: CSG-L Digest - 15 Apr 1995 to 16 Apr 1995

There are 8 messages totalling 439 lines in this issue.

Topics of the day:

Date: Sun, 16 Apr 1995 08:49:00 EST
From: Hortideas Publishing <0004972767@MCIMAIL.COM
Subject: New View, indeed!

[From Greg Williams, Easter '95]

Ed Ford kindly sent me a copy, for the CSG Archive, of the new catalog (said to be going out to 50,000 addresses) from New View Publications (P.O. Box 3021, Chapel Hill, NC 27515, phone 800-441-3604). NV is now handling distribution of several books by PCTers.

What an Easter Resurrection -- Ideas I thought long moribund arising and gaining new life cloaked in the garb of others! But I'm getting ahead of the story ...

Judging by the NV catalog, somehow in gathering materials for the CSG Archive, I apparently managed to miss many publications related to organismic control. Maybe (I wondered at first) not _THE_ control theory known and loved (or at least debated) by all of those frequenting CSG-L -- but my doubts were put to rest when I read on page 2 of the NV catalog: "_Control Theory_ is a theory of human motivation and behavior." OK, so they say there's really only one Control Theory (sorry I haven't been capitalizing the words; I stand corrected). Whew! Glad be set straight. I think.

Back to my lament. Somehow I managed to miss, among other Control Theory titles, _Find Your Natural Weight the Nectar Way_, by Judith McFadden, which NV says "applies the principles of Control Theory to help you look at your relationship with food." Funny that hasn't been talked about on the net. _And_ I missed several Control Theory books by William Glasser (there _was_ talk of a Glasser on the net, but the conclusion seemed to be that he didn't write books about _real_ control theory (ooops, Control Theory)); nevertheless, the William Glasser books in the NV catalog are _claimed_ to be about Control Theory, so maybe I'd better check them out! I _didn't_ miss yet another Control Theory book, _Freedom from Stress_ by Ed Ford, in the same catalog section as the Glasser books. I _know_ Ed's book is about _real_ Control Theory (see, I'm learning about capitalization), since I helped edit its second edition; and since Ed's book is just across the gutter (no pun intended) from a couple of Glasser books in the NV catalog, how can there be any doubt that the Glasser books are about _real_ Control Theory, too? But, on the other hand, I've never even _heard_ of Robert and Laurie Sullo's _Teach Them To Be Happy_ and _I'm Learning To Be Happy_, two more books "using the concepts of Control Theory." Why don't folks on the net spend some time discussing such interesting-sounding Control Theory works, instead of devoting so much time to arcane experimental psychology? (Hey, why beat up dead academics when you can laud living entrepreneurs? After all, it's the Republican Era!)

In the NV catalog, books originally published by CSG, as well as a new book by Dag Forssell, are under a separate "Control Systems Group" heading. I _have_ seen all of these, but I am surprised that they are pitched as follows: "Anyone interested in the origins of Control Theory will find these books a useful addition to their knowledge." I personally have found the books of much greater than just historical interest, but maybe I'm biased, and I don't have a good idea of the probable reference signals of many in the NV catalog's target audience.

Enough |-

In all seriousness, where is the PCT policeman when we really need him? Occifer, is there a law against using the words "control theory" (OK, "Control Theory") without a license? Without an understanding? Without a clue? Oh, what's that? You say that New View is distributing _your_ book? The catalog description of _Mind Readings_ by Rick Marken says that it is about "Perceptual Control Theory." Well, that's the same as Control Theory, isn't it? Must be a good read ... just the thing to peruse in parallel with _The Nectar Way_?

Just one more |-> I apologize in advance to those easily offended. I find the cartoons on the front and back of the NV catalog highly appropriate: birds with black glasses on, looking at each other. The blind not even able to lead the

blind? The irony is in the cartoon's caption: "We Apply Control Theory to Real Life." (Not trademarked -- an oversight?)

Yours for truth in advertising.

As ever (back to lurkdom),

Greg

Date: Sun, 16 Apr 1995 11:47:44 -0600
From: "William T. Powers" <POWERS_W%FLC@VAXF.COLORADO.EDU
Subject: Re: misc stuff

[From Bill Powers (950416.1045 MDT)]

Bruce Abbott (950415.1635 EST)--
Rick Marken (950414.1215) --

[Rick]

> This has been dawning on me independently; my system is basically one that controls a perception of position + change.

[Bruce]
> Didja ever think you were talkin', an' no one was listenin'? I didn't notice any acknowledgement of my prior and very similar comment.

Rate feedback is common in control systems. If you're tracing priority, you'll have to go back at least to the MIT Radiation Labs series of books on control systems (1940s).

What with controlling for being right and being first, is anybody working on models?

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

Gravitational acceleration can be considered a form of affinity; momentum can be considered a form of impetus; atmospheric pressure forcing air into an evacuated vessel can be considered a form of nature's abhorrence of a vacuum; the chemical combination of oxygen and heated mercury to form mercuric oxide can be considered a form of phlogiston expulsion by heat resulting in the transmutation of pure mercury into a calx; the conic sections followed by orbiting bodies can be considered a form of epicyclic motions on linked spheres; the Lorenz transformation can be considered a form of ether drag; the reception of light by the eye can be considered a form of emitting looking-rays; the blocking of light by an opaque object can be considered a form of shadow-casting. Control of consequences by behavior can be consider a form of control of behavior by consequences.

I guess you're right, Bruce.
--

(Bill Leach 950416.00:16 U.S. Eastern Time Zone)..

RE: MIME

I don't know if the Fort Lewis system converts to MIME before sending on the internet; all I know is that I don't have to use it.

As to translating from ASCII to MIME, the MIME version of Wayne's post was about 50% longer than the ASCII version. I think this is because MIME is treating all ASCII characters as 8 bits long (or from what you say, maybe longer). The author's writeup that came with my downloaded MIME program says

> WHAT MIME64 IS: MIME64 is an encoding described in RFC1341 as MIME base64. Its purpose is to encode binary files into ASCII so that they may be passed through e-mail gates. In this regard, MIME64 is similar to UUENCODE. Although most binaries these days are transmitted using UUENCODE, I have seen a few using MIME64, and I have had requests from friends that I decode MIME64 files that have fallen into their hands. As long as some MIME64 continues to exist, a package such as this one is useful to have.

The date on that README file was 08/09/94.

--

(Bill Leach 950416.00:23 U.S. Eastern Time Zone)--

> ...if the experimental subject is given a "cue and then food" and experiences this with sufficient frequency at a regular interval AND then the interval is lengthened (but again experienced frequently enough) a control system would be expected to "hold off" on salivating until just prior to the "anticipated" arrival time of food.

That's easy for you to say. Modeling this hold-off, however, is no simple matter (unless you just arbitrarily put in a delay that is just long enough, which isn't exactly modeling). We can come up with words that fit what happens easily enough, but turning them into models is another matter. Pavlov's analysis could just as easily be stated "Don't bodduh me, kid, it just works."

--

Greg Williams (Easter '95) --

I haven't seen the whole New View catalogue yet. The juxtapositions do sound rather odd.

I comfort myself with the knowledge that many people who have come to PCT started out by reading Glasser's stuff and wondering where it came from. Those who don't wonder aren't very likely to come our way anyhow, so I don't think we are losing anything.

The Dumbing of America continues. When you simplify things so everybody can understand them, you raise a new generation which considers today's easy stuff to be the hard stuff, and they simplify some more for the next generation that complains that it's too hard, and so it goes.

This is my last post from home for about 10 days; I'll be unsubscribing until I get back from the AERA convention and some travelling in Californicatia afterwards.

See ya Bill P.

Date: Sun, 16 Apr 1995 12:25:58 -0700
From: Richard Marken <marken@AEROSPACE.AERO.ORG
Subject: Re: New View, indeed!

[From Rick Marken (950416.1220)]

(Greg Williams, Easter '95) --

> Somehow I managed to miss, among other Control Theory titles, _Find Your Natural Weight the Nectar Way_, by Judith McFadden

I haven't seen the catalog yet but thanks for the tip; I'm going to order that one first;-)

> Funny that hasn't been talked about on the net.

Haven't I mentioned how I found my natural weight using prune juice?

> In all seriousness, where is the PCT policeman when we really need him?

I'm only in it for the money;-) See below.

> I apologize in advance to those easily offended.

You sure don't have to apologize to me; I thought your comments were right on target!

I have only one excuse: money. The four "real" PCT books that were beautifully published by you as CSG Press (LCS I & II, IMP, & MR) have not made a penny -- or, at least, not many pennies. That means that there has been a lot of unpaid, high quality work (by the authors AND by you). I think there is virtually no chance that these books will "pay-off" by becoming part of the academic curriculum in the near future, unless we manage to pass PCT off as an just another theory of behavior -- a strategy, up with which I will not put. Therefore, I was (and am) in favor of listing with NV because there's an audience of people who might at least pay for these books.

I have no illusions that all the people who buy PCT books through NV will read them or, if they do, that they will understand them. I just wanted the authors (and, in my case, I just wanted you -- who was carrying a rather large debt against my book) to get paid. Yes, I am cynically taking advantage of the acceptance of "control theory" (even if it's not "real" control theory) by the NV readership. But there is always the chance that we will reach one or two people who will not only buy the PCT books but will understand them as well. There is no chance of this at all if no one is buying the books.

Happy Easter Rick

Date: Sun, 16 Apr 1995 13:04:10 -0700

From: Richard Marken <marken@AEROSPACE.AERO.ORG
Subject: Re: Classical conditioning

[From Rick Marken (950416.1300)]

Me to Bruce Abbott:

>You do say some of the most peculiar things for a PCTer. ...

Bill Leach (950416.00:23) replies:

> I may be wrong (and hope that Bruce will straighten me out if so...) but I believe that Bruce was refering to Bill's description of "classic conditioning" and not Bill's PCT models.

I hope Bruce straightens me out on this, too, since what he said was:

> Bill's model of classical conditioning is a preparatory response model

It sounded to me like Bruce was saying that Bill's PCT model of classical conditioning is a preparatory response model. And it struck me as being a version of "nothing but-ism". A PCT model is not like any conventional of model of classical conditioning (or any other phenomenon) because it is based on control of perception. not on control, preparation, prediction, regulation, modulation or anticipation of response.

This is why this whole debate about anticipation and prediction is going on. Anticipation implies a _computation of output_ based on estimation of the future value of an input (or a disturbance to the input). A control system does not control by computing outputs; it varies outputs as necessary to control perception. The perception may be _interpreted_ as "predictive" (as it is in the case where the current perception includes a measure of the derivative of the controlled variable) but it's still a present time perception that is controlled in the "usual" way; there is no prediction of anticipation involved in this process of control.

I think the phenomenon that we refer to as "anticipation" is really control of imagined perceptions. For example, I anticipate going to the store later today. What I am doing is controlling , in imagination, some of the variables involved in "going to the store". I might even control some present time variables in anticipation of controlling the imagined variables "for real" in the future; I throw out some old food and clear space for the new stuff, for example This could be called "anticipatory" control; but the actual control (of space for food) is happening in the usual way; control of a present time perception relative to the current reference setting for that perception.

Best Rick

Date: Mon, 17 Apr 1995 00:49:12 -0400
From: bleach@BIX.COM
Subject: Interesting

<[Bill Leach 950417.00:17 U.S. Eastern Time Zone]

This is sort of off topic but cute. I don't know how many of you log into systems that do or can provide "cookies" but I do and these often are a comic relief. The one just received was, I think, worthy of posting:

A Severe Strain on the Credulity

As a method of sending a missile to the higher, and even to the highest parts of the earth's atmospheric envelope, Professor Goddard's rocket is a practicable and therefore promising device. It is when one considers the multiple-charge rocket as a traveler to the moon that one begins to doubt ... for after the rocket quits our air and really starts on its journey, its flight would be neither accelerated nor maintained by the explosion of the charges it then might have left. Professor Goddard, with his "chair" in Clark College and countenancing of the Smithsonian Institution, does not know the relation of action to re-action, and of the need to have something better than a vacuum against which to react ... Of course he only seems to lack the knowledge ladled out daily in high schools.

-- New York Times Editorial, 1920

Mon Apr 17 00:15:43 1995

-bill

Date: Mon, 17 Apr 1995 00:49:26 -0400
From: bleach@BIX.COM
Subject: Re: misc stuff

<[Bill Leach 950417.00:25 U.S. Eastern Time Zone]
>[Bill Powers (950416.1045 MDT)]

I don't know if you will get this before you leave or not -- suspect not, but hope you have a safe and enjoyable trip. You will most certainly be missed. Speaking of the "dumbing of America"... be very careful were you are going, Califusion is the leading center of such nonsense.

> MIME

MIME64 is the "clincher". The international ASCII _IS_ 64 bits. MIME is NOT intended to be used between most users and their "gateways". It is intended to be used between systems that are limited to 7 bit transfers. If one has a system that understands MIME mail then it _might_ off-load a little work from the gateway if you use it (though probably not since I suspect that most mail is not relayed in MIME format yet). Though it will work, MIME is not intended for the sending of binaries, UUENCODE is provided for that function and is more efficient.

> Pavlov's delay

There are lots of control of perception kinds of things that we believe that we understand but can not model. Since salivation is a controlled perception related to the perception of receiving food with its attendant controlled perception of ingesting same; it seems perfectly reasonable to me that one should expect that as Pavlov's experiement is conducted and the time between the "cue" and the arrival of food is extended that the control systems of the subject would correctly delay salivation _IF_ the conditions are reliable.

-bill

Date: Mon, 17 Apr 1995 00:49:40 -0400
From: bleach@BIX.COM
Subject: Re: Classical conditioning

<[Bill Leach 950417.00:36 U.S. Eastern Time Zone]
>[Rick Marken (950416.1300)]

Well, I guess we will both have to wait and see which (if either) is right but I really did not take Bruce to be taking about PCT but rather about one of Bill's description of "classic conditioning".

> Anticipation and prediction

I do believe that these terms need be delt with. It is not, in my mind, a question of exception to control of perception but rather dealing with a phenomenon that we all experience as a day to day matter.

All of us "predict" and all of us "anticipate". Just dismissing these rather than showing how they are still control of current perception is haughty. We tend to "sense" these things personally and universally and thus there are a subject for PCT.

I do recognize that these terms have a bit in common with another "universal" experience such as "you make me angry" but just like "you make me angry" these need specific treatment by PCT to show how to recognize what really is going on.

-bill

Date: Mon, 17 Apr 1995 00:54:44 -0400
From: bleach@BIX.COM
Subject: Prediction reliability, example

<[Bill Leach 950417.00:50 U.S. Eastern Time Zone]

Recession is when your neighbor loses his job. Depression is when you lose your job. These economic downturns are very difficult to predict, but sophisticated econometric modeling houses like Data Resources and Chase Econometrics have successfully predicted 14 of the last 3 recessions.

Mon Apr 17 00:48:44 1995

-bill

End of CSG-L Digest - 15 Apr 1995 to 16 Apr 1995
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Date: Mon Apr 17, 1995 9:57 pm PST
From: Control Systems Group Network
EMS: INTERNET / MCI ID: 376-5414
MBX: CSG-L%UIUCVMD.BITNET@uga.cc.uga.edu

TO: * Purposeful Leadership / MCI ID: 474-2580
Subject: CSG-L Digest - 16 Apr 1995 to 17 Apr 1995

There are 11 messages totalling 844 lines in this issue.

Topics of the day:

Date: Mon, 17 Apr 1995 06:26:00 EST
From: Hortideas Publishing <0004972767@MCIMAIL.COM
Subject: Re: New View, indeed

[From Greg Williams (day after Easter '95)]

Interesting how arguments about purity on behalf of one's cause become diluted when personal financial gain is involved, isn't it? I've thought I've noted a double standard regarding purity among some PCTers in the past, but this is the first time it seems economically motivated.

As ever, Greg

Date: Mon, 17 Apr 1995 09:28:02 -0500
From: Bruce Abbott <abbott@CVAX.IPFW.INDIANA.EDU
Subject: Words

[From Bruce Abbott (950417.0925 EST)]

I know Bill will be out of touch with the net for a bit, but I thought I'd reply while things are still fresh.

>Bill Powers (950416.1045 MDT)

>[Bruce]

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

> Gravitational acceleration can be considered a form of affinity; momentum can be considered a form of impetus; atmospheric pressure forcing air into an evacuated vessel can be considered a form of nature's abhorrence of a vacuum; the chemical combination of oxygen and heated mercury to form mercuric oxide can be considered a form of phlogiston expulsion by heat resulting in the transmutation of pure mercury into a calx; the conic sections followed by orbiting bodies can be considered a form of epicyclic motions on linked spheres; the Lorenz transformation can be considered a form of ether drag; the reception of light by the eye can be considered a form of emitting looking-rays; the blocking of light by an opaque object can be considered a form of shadow-casting. Control of consequences by behavior can be consider a form of control of behavior by consequences.

A very nice recitation of outdated scienfific ideas, but Bill has missed the point. I am using "anticipaton" as a name for a common phenomenon everyone experiences, which deserves to be explained; all those terms he mentioned were theoretical constructs whose function was to explain. Anticipation is what we're explaining when we offer a PCT model showing how a higher-level system can alter the reference of a lower-level system so as to begin the action that will counter a disturbance which is about to happen to the variable being controlled at the lower level. When people (whether behavioral scientists who believe in phlostogen or just interested laypeople) ask "how does PCT account for anticipation?" they deserve a better answer than the one given by Bill above.

Bill's refusal in this instance to grant me the use of the ordinary meaning of an ordinary word reminds me of something I once read, long ago:

"There's glory for you!"
"I don't know hwat you mean by 'glory,' " Alice said.
"Humpty Dumpty smiled contemptuously. "Of course you don't--till I
tell you. I meant 'there's a nice knock-down argument for you!' "
"But 'glory' doesn't mean 'a nice knock-down argument,' " Alice objected.
"When _I_ use a word," Humpty Dumpty said, in a rather scornful tone,
"it means just what I choose it to mean--neither more nor less."

Through the Looking Glass by Lewis Carroll

I've been feeling a lot like Alice lately.

Regards, Bruce

Date: Mon, 17 Apr 1995 10:02:51 -0500
From: Bruce Abbott <abbott@CVAX.IPFW.INDIANA.EDU
Subject: Acknowledgement

[From Bruce Abbott (950417.1000 EST)]

>Bill Powers (950416.1045 MDT)

>[Rick]
> This has been dawning on me independently; my system is basically one that controls a perception of position + change.

>[Bruce]
> Didja ever think you were talkin', an' no one was listenin'? I didn't notice any acknowledgement of my prior and very similar comment.

> Rate feedback is common in control systems. If you're tracing priority, you'll have to go back at least to the MIT Radiation Labs series of books on control systems (1940s).

Priority was the last thing on my mind. Let me try to illustrate.

[Three kids are playing on a sandlot.]

Freddie: Hey, wadaya wanna do now?

Johnny: Well, how about a game of tag?

Jimmie: I dunno, let's see. Umm, tag might be fun.

Freddie: Ya know, Jimmie, I was thinkin' the same thing. As always, you're way ahead of me; good thinkin'! Hey Johnny, Jimmie 'n me wanna play a game of tag.

Johnny: Hey, is anybody listening? That's what I said, too!

Jimmie: Jimmy, if you want credit for the idea, you're gonna haf ta go back a long way. You weren't the first guy who ever thought to play tag.

I don't want credit for being first. But I get to hear so often how wrong I am; here was a point on which all three of us--just about simultaneously--came to the same conclusion, which would seem to indicate that I'm reasoning through the PCT implications correctly. Would it have hurt to acknowledge that? We can all use a pat on the head from time to time.

Regards, Bruce

Date: Mon, 17 Apr 1995 11:02:00 EST
From: Hortideas Publishing <0004972767@MCIMAIL.COM
Subject: More New View

[From Greg Williams (day after Easter '95 - II)]

>Bill Powers (950416.1045 MDT)

> I haven't seen the whole New View catalogue yet.

So you didn't preview the representation of "Control Theory" in the catalog?

> The juxtapositions do sound rather odd.

I would prefer the word "misleading."

> I comfort myself with the knowledge that many people who have come to PCT started out by reading Glasser's stuff and wondering where it came from. Those who don't wonder aren't very likely to come our way anyhow, so I don't think we are losing anything.

Nothing except our credibility, perhaps.

> The Dumbing of America continues. When you simplify things so everybody can understand them, you raise a new generation which considers today's easy stuff to be the hard stuff, and they simplify some more for the next generation that complains that it's too hard, and so it goes.

I hope that PCTers won't be contributing to the Dumbing by ignoring the problem, selectively.

As ever, Greg

Date: Mon, 17 Apr 1995 09:52:38 -0700
From: Richard Marken <marken@AEROSPACE.AERO.ORG
Subject: PCT Purity, Anticipation

[From Rick Marken (950417.0950)]

Greg Williams (day after Easter '95) --

> Interesting how arguments about purity on behalf of one's cause become diluted when personal financial gain is involved, isn't it?

I don't know if I have ever argued for "purity" on behalf of my "cause" (it sounds a bit scary when you put it that way). I am interested in people getting the basic ideas of PCT right but this is not a "purity" issue; it's a factual issue. For example, control systems select and control the consequences of their actions; that's a fact. The consequences of actions do not select the actions of a control systems; also a fact. People who think that "selection by consequences" is consistent with control theory don't have "impure" thoughts; they have incorrect thoughts.

Perhaps you are saying that listing PCT books along with books that misrepresent PCT (like many of those in the NV catalog) is impure. Perhaps it is-- but I have never argued against this kind of impurity. Heck, I would be for listing the PCT books in the MIT Press catalog even though this would be impure too because most of the books in that catalog that even deal with control theory present a mistaken view of how to apply the theory to behavior.

> I've thought I've noted a double standard regarding purity among some PCTers in the past, but this is the first time it seems economically motivated.

What double standard? And what's wrong with economic motivation? I like it when people get paid for doing what I consider good work. PCT science is VERY good work; what's wrong with wanting people to be able to eat while they do their work?

On that note, I would like to say that I am sorry that the PCT books published by CSG Press did not sell a LOT better. I think they would have if all of the PCT people currently teaching in academic settings had assigned those books as textbooks. PCT people teaching intro classes should have used "Intro to Modern Psychology" as the text. PCT people teaching research methods classes should have assigned "Mind Readings" as a supplement to Phil Runkel's "Casting Nets..." text. Living Control Systems I and II should have been required in all classes. This can (and should) still be done -- but it wasn't (at least, not on a large enough scale) so we had to look for another way to sell the books. Indeed, I don't understand how a PCTer in academia could, in good conscience, assign anything other than a PCT text in any class.

If you want to get mad at someone for the fact that we have decided to list the CSG books in the NV catalog, why not get mad at all the academic PCTers who could have been making bulk orders of all the CSG Press books for the last five (seven?) years?

Bruce Abbott (950417.0925 EST) --

> **Anticipation is what we're explaining when we offer a PCT model showing how a higher-level system can alter the reference of a lower-level system so as to begin the action that will counter a disturbance which is about to happen to the variable being controlled at the lower level.**

But control systems don't control their outputs. If the reference output of a higher level system begins action that will counter a disturbance which is about to happen this is happening as part of the process of controlling the higher order perception. The control system is not designed to change it's reference output in anticipation of the disturbance. Control systems don't control their output. If, for example, something (like the output of another system) kept the reference output of the higher level system from beginning action to counter a disturbance which is about to happen, the higher level system would not do anything to try to get its reference output to do it's usual anticipation. Control systems control input; outputs, even when they appear anticipatory, are not controlled; they are whatever is demanded by circumstance to keep controlled perceptions under control.

> **When people (whether behavioral scientists who believe in phlostogen or just interested lay people) ask "how does PCT account for anticipation?" they deserve a better answer than the one given by Bill above.**

Bill has been explaining how PCT accounts for the phenomenon you call "anticipation" over and over again; it is side effect of controlling a perception that includes the "predictive" disturbance. Apparently you just don't like that explanation. So feel free to show that it's wrong;-)

Best Rick

Date: Mon, 17 Apr 1995 13:03:27 -0500
From: Bruce Abbott <abbott@CVAX.IPFW.INDIANA.EDU
Subject: Preparatory Response

[From Bruce Abbott (950417.1300 EST)]

> Rick Marken (950415.1520)]

>>Bruce Abbott (950415.1510 EST)

>>>Bill Leach 950416.00:23 U.S. Eastern Time Zone

>> Bill's model of classical conditioning is a preparatory response model; such models essentially reduce classical to instrumental conditioning.

> You do say some of the most peculiar things for a PCTer. How in the world could Bill's model of classical conditioning be a "preparatory response" model? It's a control of perception model. The actions of the PCT model do whatever is necessary to keep perceptual variables under control. Sometimes this may look like "response preparation", but, of course, it's not.

>>> I may be wrong (and hope that Bruce will straighten me out if so...) but I believe that Bruce was refering to Bill's description of "classical conditioning" and not Bill's PCT models.

Bill, you are right on the mark. Traditional analyses of classical conditioning fall into one of two broad categories: stimulus substitution and CS-as-signal. In the latter category is the "preparatory response analysis" first described by Zener and elaborated by Charlie Perkins, which proposes that the CS becomes conditioned because it allows the organism time to make responses which "prepare" the organism for the US. For example, a bell ringing just prior to dry food powder being placed into a dog's mouth would allow the dog to get the saliva flowing and thus have its mouth in an optimal state for the receipt of the food. This is, of course, the way Bill P. described his model as working, although he used the term "action" rather than "response" to describe the change in output due to the disturbance provided by the bell, so I am justified in suggesting that Bill's (Wayne's) model falls into the "preparatory response" category. However, beneath that surface similarity lies a world of difference between the traditional preparatory response model and the PCT model. The traditional model asserts that salivation is reinforced by whatever effect the "preparation" has (e.g., wet food powder tastes better than dry), thus "strengthening" the salivary response to the CS. PCT views the CS as a disturbance to a perception, which generates error, which, via the output transfer function, generates salivation, which then starts to counter the error produced by the arrival of dry food powder in the mouth just about the time the powder arrives.

> I think you mou missed the point of Bill's comments about "inhibition of delay". Bill said:

>> Well, that makes me suspect more than Pavlov's experimental methods.

> Bill was expressing suspicion, not only about Pavlov's experimental methods, but about his approach to theory too.

If I am confused, it's because the reference is vague. Suspect what? Pavlov's theory? His sanity? His motives? His intelligence? His observations? His honesty? All the above?

Pavlov didn't have PCT to guide his theorizing; he had only his observations, his intelligence, and the current findings and theories of his day to go on. I note that you criticize Pavlov, not for his theory, but for his "approach" to theory. What does that mean? In the context of what was known at the time, what was wrong with his approach?

I am not defending Pavlov's theory, mind you, but Pavlov's methods and his ability to create a theoretical framework of the highest scientific caliber, one that elegantly fit the available data. Yeah, we can see where he went wrong, but we have the benefit of 20-20 hindsight.

>> Prior to his involvement with "conditioning," Pavlov was awarded the Nobel prize in physiology for his work on digestion.

> A prior Nobel prize seems to be a guarantee that a person will be clueless about the nature of behavior. Do the names Edelman, Crick, Simon, Sperry, or Lorenz ring a bell? Nobel winners (PCT losers) all.

So who was talking about theory? You have taken my statement out of context, which was as a reply to Bill P.'s comment that he had heard that Pavlov's experimental work was sloppy. It was intended to support the argument that Pavlov was a good experimentalist; it had nothing whatever to do with his abilities as a theorist. I'm familiar with the work of several of those Nobel winners you listed above, and judging from what I've seen, these folks have done excellent empirical work, regardless of what you may think of their theories. If I were developing a list of credentials for identifying those who have done good (as opposed to sloppy) empirical work, I'd say having a Nobel prize for that work would be a definite keeper.

On the other hand, I agree with you that having a Nobel prize does not mean that you understand the essential nature of behavior. Roger Penrose is another (very recent) example [see "The Emperor's New Mind."] But I'd leave Simon and Lorenz off your list of the "clueless."

Regards, Bruce

Date: Mon, 17 Apr 1995 12:04:33 -0700
From: Richard Marken <marken@AEROSPACE.AERO.ORG
Subject: Go ask Alice

[From Rick Marken (950417.1200)]

Bruce Abbott (950417.0925 EST) --

>I've been feeling a lot like Alice lately.

I understand. Things will become a lot easier when you realize that you are, indeed, in wonderland -- PCT wonderland, that is. Many people never do realize it

and they become quite resentful and angry; that's why people come and go so quickly here;-

Best

Rick (yes, that was a cheshire smile) Marken

Date: Mon, 17 Apr 1995 15:53:21 -0500
From: Bruce Abbott <abbott@CVAX.IPFW.INDIANA.EDU
Subject: Re: anticipation

[From Bruce Abbott (950417.1550)]

>Rick Marken (950417.0950) --

>>Bruce Abbott (950417.0925 EST) --

>> Anticipation is what we're explaining when we offer a PCT model showing how a higher-level system can alter the reference of a lower-level system so as to begin the action that will counter a disturbance which is about to happen to the variable being controlled at the lower level.

> But control systems don't control their outputs.

This is really getting ridiculous. I never said they did.

> If the reference output of a higher level system begins action that will counter a disturbance which is about to happen this is happening as part of the process of controlling the higher order perception.

Yeah, that's what I'm talking about. This is supposed to be news to me?

> Control systems control input; outputs, even when they appear anticipatory, are not controlled; they are whatever is demanded by circumstance to keep controlled perceptions under control.

Show me where I said anything about control systems controlling their outputs. This notion that this is what I am saying has been your idea from the start (as I am now saying to you for at least the third time). Please go back and read my pior posts on this subject if you are having trouble recalling my prior descriptions of this system.

> Bill has been explaining how PCT accounts for the phenomenon you call "anticipation" over and over again; it is side effect of controlling a perception that includes the "predictive" disturbance. Apparently you just don't like that explanation.

This is a TOTAL mischaracterization of the discussion, completely unfair. Go back and re-read everything that has transpired on the topic of "anticipation" since the post that started it off [Bruce Abbott (950407.1025 EST)]. Neither you nor Bill P. even wanted to discuss the topic; you both thought I was talking about "feedforward" and "control of output." It took me quite some time to realize that

this was your perception, but when I did, I categorically denied that I had anything like this even remotely in mind.

Let's compare statements:

>>Bill P. (950408.1420) --

The higher-order control system is sending changing reference signlas to several lower-order systems, some of which are SUPPOSED TO RESULT IN CANCELING FORCES. [emphasis mine]

Me:

>> .. a PCT model showing how a higher-level system can alter the reference of a lower-level system so as to begin the action that will counter a disturbance which is about to happen to the variable being controlled at the lower level.

You (950408.2015):

> Control based on prediction of the future state of a variable would allow a variable representing, say, the predicted future error signal, to be used to drive (or to contribute to driving -- these are the implementation problems I'm having) the output of a control system;

This is your statement; I never said anything remotely like it.

Me (950409.1100 EST), responding to Bill P.'s statement copied above:

> In this case the timing problem disappears because the same system is initiating both the disturbances to the lower-level systems and the changes in reference levels to counter them. The notion that a higher-level system initiates lower-level reference changes is exactly what I had in mind in my earlier post (950408.1805 EST) in which I said:

>>> Perhaps the predictive cues are inputs to another control system, whose output sets the reference for the lower-level system monitoring the perceptual variable in question whose changes the predictive cue is predicting.

You (950407.1450):

>> This is a myth that probably originated with the "generated output" cult, particularly the sect that worships "feedforward control."

You (950409.1115):

> What looks like "anticipation based on predictive cues" is probably always an observed side effect of controlling a higher order variable.

Me (950409.1500 EST):

My thoughts exactly, as I have expressed before.

The fact that you, Bill P. and I were all saying the same thing was recognized by Wayne Hershberger (040995):

> It appears to me that you all implicitly agree that PCT can account for "anticipation" in one manner or another, most likely in terms of a shift of the reference value of the control loop whose input is about to be altered by the impending disturbance. Piece of cake!

Bill P (950410.1720):

> [Bruce]

> Who said they don't predict? 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.

> Perhaps this will make the point:

> I am looking at an empty coffee cup that has been sitting beside my monitor all day. I have a feeling that this is a predictive cue concerning something I am going to do. However, even though I have been watching this coffee cup for over 12 hours, on and off, so far it hasn't predicted anything. Is there something wrong with it?

This is the point at which I finally understood that you guys thought I was talking about cues being used to calculate an output. The marginal note I placed in the hardcopy of this says:

"Bill thinks I am trying to use the cue to _generate behavior_ on the basis of the cue's prediction. Wrong!!"

My reply (9504.1100 EST):

> No, it's only a perceptual signal, and it is only in relationship to some control system of yours that the state of that signal has any relevance to what you may do. So when, for whatever reason, the reference of the perceptual system that "wants" to perceive a full cup of hot coffee goes high, the current perceptual state of that cup (i.e., empty) will, when compared to the reference, generate an error signal that will initiate a program whose ultimate result will be (if conditions permit) a full cup of hot coffee (not necessarily that cup, either). If the cup were already full of hot coffee, the likely result is that you would glance at the cup to confirm that it is indeed at the reference state, and the lower-level program would remain inactive, there being no need to go for a refill.

> But this is, it seems to me, a different situation from the ones I have in mind when I refer to "predictive cues" and "anticipation." Let's say that promptly at 3 pm each day Mary comes into the room bearing a tray containing a steaming pot of hot coffee and two clean cups. By mutual agreement, this is your "break" time; you put aside the writing or program and the two you enjoy each other's company and the coffee for a few minutes. It is now five minutes to three. Even though you are on the brink of getting that last subroutine to work on ARM3, you click the "save" button and begin to clear the papers off the coffee table to make room for the tray. But Mary's not here yet. Is there something wrong with you? Or more to the point, what does a clock reading of 2:55 indicate about Mary? It's now 3:10 and no Mary. What will you do? Why? I'm probably missing your point, but then again

perhaps you are missing mine. Why is this so difficult? On consideration, it seems to me that you believe that I think the "predictive cue" in your senario would directly generate a specific behavioral output--that it would predict that you would get up for a refill. That's not my viewpoint at all, as I hope I've made clear.

Then there was this exchange, which revealed that you STILL didn't get it (Marken, 950410.2145; Abbott, 950410.1100):

>>You
>Me

>> If control only works when the "cues" are predictive then we are really not dealing with control at all, are we?

> What? I don't think I said anything remotely resembling the idea that "control only works when the 'cues' are predictive."

>> Is this model consistent with your notion of control based on a predictive cue (in this case, the variable x)? The model appears to be controlling z-y (keeping it at zero). Is this what you had in mind as predictive control? If not, please diagram your predictive control model.

> Actually I have several possible models in mind. I'd guess the one that comes closest to your description would be the one presented a short while ago by Bill Powers, proposed as a model for classical conditioning. You can look it up.

You (950410.2145):

> 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.

Me (950411.1620):

> 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?

I think you get the drift. So, cutting to the chase:

You:

> Bill has been explaining how PCT accounts for the phenomenon you call "anticipation" over and over again; it is side effect of controlling a

> perception that includes the "predictive" disturbance. Apparently you just don't like that explanation. So fell free to show that it's wrong;-)

As I've documented above, it's what _I'VE_ been saying all along. Apparently I don't like this explanation? Bill's (Wayne's) classical conditioning model was in my mind as an explanation of this phenomenon from the time I first mentioned the topic of anticipation. Unfortunately for the ensuing discussion (and my sanity), you and Bill P. have been off pursuing your own agenda, off fighting the mythical S-R dragon that was never a shadow of an inkling of a thought of my explanation. To continue to characterize my position as being in opposition to this proposal after I have repeatedly stated otherwise is unfair in the extreme. Wake up and smell the coffee.

Regards, Bruce

Date: Mon, 17 Apr 1995 16:54:17 -0500
From: Wayne A Hershberger <tj0wah1@CORN.CSO.NIU.EDU
Subject: classical conditioning

[from Wayne Hershberger 950417]
Bruce, Bill, Mary, and Rick:

(Rick Marken 950415.1600)

> In the "predictive control" system that I built (and that Bill elaborated) a present time perception, p = (t-c) + Kd/dt(t-c), is under control. There is no "anticipation" in the sense in which I thought you were using ther term. I thought "anticipation" occurs when a system produces an output before an anticipated disturbance. That is, o(t) is an output that begins to compensate for d(t+delta t). There is no anticipation of this sort n the system that controls (t-c) + Kd/dt(t-c).

This is why I thought Bruce was talking about conditioned reflexes not instrumental responses.

(Bruce Abbott 950415.1510 EST) to Bill P.

> I don't know who told you that Pavlov was sloppy; I have a different impression. Prior to his involvement with "conditioning," Pavlov was awarded the Nobel prize in physiology for his work on digestion. All tests were conducted in a laboratory especially designed for the purpose and employed some the most advanced equipment of the day.

That is my impression as well, Bruce. Pavlov, as I recall (it has been 35 years since I read Anrep's translation) was particularly concerned about careful experimental control and willing to go to great expense (I'm not sure whose; a Nobel laureate has prerogatives) to achieve it. For example, his experimental rooms had double walls (rooms within rooms).

> Technically, the teakettle example fits the procedural definition of instrumenta{ conditioning....The situation would be more correctly described as one involving a discriminated operant.

OK, but not a conditioned reflex. When C. C. Perkins characterized conditioned reflexes as prepartatory responses he was identifying their utility (offsetting the sensory consequences of the UCS, or disturbance), but he was not suggesting that they were "instrumental responses." In PCT terms it is the difference between controlling environmental disturbances and controlling their sensory consequences.

A conditioned reflex appears to be an endogenous disturbance which offsets an exogenous disturbance. If the exogenous disturbance does not materialize, the reflex is dysfunctional; it serves as a self-generated disturbance. If a cup of liquid is not as heavy as anticipated, I may throw the liquid into my face. If reference signals always specify the intended value of some input, not output, the conditional reflex is not an intended output. But, neither is the consequence of the reflex an intended input--o{herwise I should be pleased with the acceleration of the cup, if not the wetness of my face. If a classically conditioned reflex is mediated by a reference signal it is an odd sort of reference signal-- which Rick hasn't modeled yet. This is what I thought you were driving at.

Regards to all, Wayne

Date: Mon, 17 Apr 1995 18:35:00 EST
From: Hortideas Publishing <0004972767@MCIMAIL.COM
Subject: No sympathy for _any_ devils!

[From Greg Williams (day after Easter '95 - III)]

>From Rick Marken (950417.0950)

> If you want to get mad at someone for the fact that we have decided to list the CSG books in the NV catalog

I'm not mad about that decision (after all, I got an emptied -- of books -- office and a new computer out of the deal), but I _will_ be mad if everybody just shrugs off how the NV catalog treats PCT and some quite different ideas as all-of-a-piece "Control Theory." After myriad complaints down the years on CSG-L about R. Beer, Carver and Scheier, various control engineers and human factors researchers, conventional psychologists, popular-press authors, etc., etc. not getting their PCT-facts straight, silence about the NV catalog's representation of Control Theory would surely beg the question from anyone so heavily critiqued: I wonder how much I'd have to pay for them to stop criticizing _me_?

You could do much more than simply criticize the NV catalog. You could try to influence future catalogs and other publicity. Maybe you could even set some of the NV folks straight on the facts of living control systems. Then you wouldn't be on the devils' payroll. ;-

As ever, Greg

Date: Mon, 17 Apr 1995 20:36:54 -0500
From: Bruce Abbott <abbott@CVAX.IPFW.INDIANA.EDU

Subject: Re: Classical Conditioning

[From Bruce Abbott (950417.2035 EST)]

>Wayne Hershberger (950417) --

> When C. C. Perkins characterized conditioned reflexes as prepartatory responses he was identifying their utility (offsetting the sensory consequences of the UCS, or disturbance), but he was not suggesting that they were "instrumental responses."

Yes, perhaps you're right (I'll have go back and look it up; it's been a while for me, too), but I think Zener was. However, when Perkins applied his analysis to preference for signaled over unsignaled shock schedules, he spoke of the signals as permitting the rats to make responses which reduced the aversiveness of the shocks (e.g., by reducing their perceived intensity). I spent a lot of time in graduate school trying to rule out this analysis, which in one form suggested that the rats were making some kind of postural adjustment on the grid during the signal to reduce their contact with the shock when it was presented at the end of the signal.

> In PCT terms it is the difference between controlling environmental disturbances and controlling their sensory consequences.

Yes, the "preparatory response" does not affect the presentation of, e.g., the food powder, only something like how good it tastes or how easy it is to swallow.

> If a classically conditioned reflex is mediated by a reference signal it is an odd sort of reference signal-- which Rick hasn't modeled yet. This is what I thought you were driving at.

The "anticipation" model Rick presented does not include a second level whose reference level would be affected by the CS; in fact it has no CS at all. It is not a model of classical conditioning but does involve "anticipation" (although Rick doesn't agree with my use of the term for it) in that something present now (the current state and rate-of-change of a perception) is used to predict (anticipate) its immediate future state and thus "get the ball rolling" in the output transfer function slightly earlier than would be the case if the first derivative were not included in the input function. This has the effect of reducing the lag inherent in the integrative output transfer function.

The "anticipation" models I have in mind for classical conditioning may include only one level (as in Rick's model) or at least two. The first example, which was diagramed by Bill P. earlier and was said to be based on your conception, had both the CS and controlled variable represented in the input transfer function of a single-level control system. Here is the diagram Bill (950312.2145 MST) presented:

```
>                          | REF SIG
>                          |
>            perceptual    V
>            sig   ----> COMP ----->--
>                 |                   |
>                SUM               OUTPUT
>               /   \             FUNCTION
>             /       \               |
>            |         |              |
>          INPUT     INPUT            |
>         FUNCTION  FUNCTION          |
>            |         |              V
>            |        C.V. <------- ACTION
>            |         ^
>            |         |
>            |    DISTURBANCE
>            |         ^
>         SIGNAL       |
>            ^       delay
>            |         |
>             -<---PRECURSOR
```

In Bill's diagram the US acted as a disturbance to the controlled variable, but the US was preceded in time by a "precursor" which produced an immediate perceptual effect (the CS). Through some kind of reorganization process, the CS's influence within the input transfer function increased so that the CS was rendered capable of disturbing the perceptual variable; the system would then begin an action to counter the disturbance produced by the CS before the main disturbance had begun. As Bill put it:

> The precursor causes a signal that is summed with the perceptual signal from the cv to yield a net perception. This perception will depart from the reference level because of the signal, and cause an error signal to appear. The output action will begin to build up as if toward the level that would counteract a disturbance of some particular magnitude and sign.

>If the precursor occurs at just the right time in advance of the ensuing
>disturbance, when the disturbance occurs it will find the action already
>beginning to increase in the appropriate direction and with the
>appropriate sign. The error-cancelling action will therefore occur
>sooner than it would without the signal from the precursor, and the
>integrated error will be smaller.

[There were some additional considerations I am not reporting, but this is the basic idea.]

One of the second (two-level) "anticipation" models I have in mind has the upper-level system acting through a set of lower-level systems by altering their references. This one looks less like classical conditioning; some of its outputs adjust the lower-level references in such a way as to control (via these systems) the upper-level system's perception (as usual). Other outputs adjust the lower-level references so as to counteract (usually) disturbances to those systems arising from from the action of the higher-level system. When those disturbances do not arise, these reference-level changes result in an unopposed action which itself acts as a disturbance to the system involved. The example Bill gave was pressing the accelerator of your car and simultaneously contracting muscles in your arms or abdomen so as to counteract the effect of the car's acceleration. If the acceleration fails to occur (the engine stalls), you lurch forward against the wheel. The appearance is that you lurched forward "in anticipation" of the acceleration, but of course it's all merely present-time control of present-time perceptions.

In speaking of anticipation as a phenomenon to be explored, I was not restricting myself to classical conditioning, as should be apparent now. If at times I seem to be talking about different things, its because I am. "The" phenomenon of anticipation is really a number of different phenomena, for which different control models will need to be developed.

Regards, Bruce

End of CSG-L Digest - 16 Apr 1995 to 17 Apr 1995
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Date: Mon, 17 Apr 1995 22:17:04 -0700
From: Richard Marken <marken@AEROSPACE.AERO.ORG
Subject: Words, Models, Sympathy for the Devil

[From Rick Marken (950417.2215)]

Me:

> What looks like "anticipation based on predictive cues" is probably always an observed side effect of controlling a higher order variable.

Bruce Abbott (950417.1550) --

> My thoughts exactly, as I have expressed before.

Ok. Great.

> you and Bill P. have been off pursuing your own agenda, off fighting the mythical S-R dragon that was never a shadow of an inkling of a thought of my explanation...Wake up and smell the coffee.

Ok. Ok. Sorry. Somehow we are apparently not communicating. How
about just building a simple model of a classically conditioned
organism (forget the "conditioning" phase) and that will give us
a better idea what we're ally talking about.

I said:

> I thought "anticipation" occurs when a system produces an output before an anticipated disturbance...There is no anticipation of this sort in the system that controls (t-c) + Kd/dt(t-c).

Wayne Hershberger (950417) --

> If a classically conditioned reflex is mediated by a reference signal it is an odd sort of reference signal--which Rick hasn't modeled yet. This is what I thought you were driving at.

I don't know if this was presicely what Bruce was driving at but I agree that I have not modelled the behavior of a classically conditioned organsm yet. But I am starting to see what you guys are getting at and I think we have to model classical conditioning in order to understand the PCT view of it. Of couse, we can't really understand the PCT view of classical conditioning until we know what variable(s) a classically conditioned organism is controlling. Until people start testing this we'll just have to model what we imagine to be controlled to see if the resulting behavior looks at least qualitatively like what we see. But I bet that it can all be done with a good old-fashioned, single level control system with a regular old perceptual, reference and error signal. y

Greg Williams (day after Easter '95 - III) --

> After myriad complaints down the years on CSG-L about R. Beer, Carver and Scheier, various control engineers and human factors researchers, conventional psychologists, popular-press authors, etc., etc. not getting their PCT-facts straight, silence about the NV catalog's representation of Control Theory would surely beg the question from anyone so heavily critiqued: I wonder how much I'd have to pay for them to stop criticizing _me_?

I am certainly not reserving my criticism of the books in the NV catalog; I just haven't read any of them (that I know of; still no catalog). I criticized the works you mention above because they are about topics where I think I have some expertise. I have certainly criticized Glasser's work on the net. The other works you mentioned (like the "nectar" book) are probably not even worth criticizing.

As far as the credibility implications of having "real" PCT books sitting in the NV catalog; first, I think the credibility of PCT should depend only on the demos, experiments and models that we make available to anyone who is interested; second, to the extent that PCT credibility does depend on the company it keeps, then the quality of the company is in the eye of the beholder and there's not much anyone can do about that; the books that are one person's "quality company" are another's embarassment. I, for example, consider my "Mind Readings" book to be one of the best (and most "real") PCT books around; but it nearly always appears in "official" lists of recommended PCT readings along with books that, I think, reduce its credibility. So I have already had to deal with the NV problem. I bet my list of the books that DON'T reduce the credibility of my book is a lot shorter than yours (not often you'll hear a guy say that, eh;-)

Best Rick

Date: Tue, 18 Apr 1995 01:41:49 EDT
From: "CHARLES W. TUCKER" <N050024@UNIVSCVM.BITNET
Subject: Anticipation, again or When does the act begin?

FROM CHUCK TUCKER 950418

It seems to me just glancing through all of these posts on the issue of anticipation that there is still the problem of "what is the best word to use?" and "when does the act begin?"

The first question always seems to bring on many posts since since words such as "stimuli," "cue," and "predict" remind Rick, Bill (and Tom, if he were on the net) of the old battles with S-R or S-O-R. As I have said before, all the terms used in PCT (including the three indicated by those letters!) are specified differently than dictionary use or uses in all formulations in the so-called "life sciences." Let us get back to the glossary idea and preface all posts with "I mean by X ....." so such silly discussions can be avoided.

The notion of anticipation and predictive sensing clearly depend upon when one arbitrarily begins an act. As Dewey pointed out in his critique of the S-R formulation (1896) a part of the act appears to be a "stimulus" because one "begins" an act at a certian point in the "stream of activity." If we parse any act we have to begin somewhere in that stream but we should not forget that the organism was doing something before we decided where we started the act to parse. So it we start with a "cup being lifted toward the mouth" (for an act of drinking) we should not forget that prior to that there was "a hand grasping the handle of the cup" and prior to that "a hand reaching toward the handle of the cup" and prior to that "the forming of the hand to grasp the handle of the cup" and prior to that ...... You should get the point. All of these parts of the act can be parsed in PCT terms. If you don't wish to label the action prior to when you start the act 'anticipation' then call it by another name but let's parse the act first so we have a notion of what action we have under analysis.

Regards, Chuck

Date: Tue, 18 Apr 1995 06:58:00 EST
From: Hortideas Publishing <0004972767@MCIMAIL.COM
Subject: Anticipating police activity

[From Greg Williams (950418)]

> I am certainly not reserving my criticism of the books in the NV catalog

Great! I hope that you will continue to act in ways tending to dispel impressions that the PCT-incorrect might be able to buy relief from your critiques. How about some comments on the net when you've had a chance to peruse the NV catalog (especially with regard to how the devils might be wrong about the PCT facts of life)?

Go to it, officer! I am pleased to hear that you are above taking a bribe, and I look forward to your police actions in support of that claim.

As ever, Greg

Date: Tue, 18 Apr 1995 11:23:29 -0500
From: Bruce Abbott <abbott@CVAX.IPFW.INDIANA.EDU
Subject: Modeling Classical Condtioning

[From Bruce Abbott (950418.1120 EST)]

>Rick Marken (950417.2215) --

> Ok. Ok. Sorry. Somehow we are apparently not communicating. How about just building a simple model of a classically conditioned organism (forget the "conditioning" phase) and that will give us a better idea what we're ally talking about.

Whew! Now we can get back to doing something useful! Building a model of a classically conditioned organism is an excellent proposal (we can worry about HOW the organism got that way at some future date), and I'm already on it. I've been working through the requirements of the model and am closing in on a specific implementation which I think you will find very much to your liking. Stay tuned.

Regards, Bruce

Date: Tue, 18 Apr 1995 17:06:36 +0000
From: CZIKO Gary <g-cziko@UIUC.EDU
Subject: Away for a while; Powers's AERA paper

[from Gary Cziko 950418.1329 GMT]

I will be away from home CSGnet starting tomorrow (Wednesday, 04/19) through next Sunday evening 04/23). I trust that the net will stay alive until I get back.

I will be at the American Educational Research Association meeting in San Francisco along with Hugh Petrie, Bill Powers, Ed Ford, and Dag Forssell. Should be fun!

Bill Powers's AERA paper can be had on the World Wide Web at http://www.ed.uiuc.edu/csg/people/powers/docs/nature_of_pct.html.

--Gary

Date: Tue, 18 Apr 1995 12:28:08 -0700
From: Richard Marken <marken@AEROSPACE.AERO.ORG
Subject: Re: Modelling Classical Conditioning

[From Rick Marken (950418.1230)]

Bruce Abbott (950418.1120 EST) --

> Building a model of a classically conditioned organism is an excellent proposal (we can worry about HOW the organism got that way at some future date), and I'm already on it.

Excellent!!

> I've been working through the requirements of the model and am closing in on a specific implementation which I think you will find very much to your liking. Stay tuned.

I just finished my model of classical conditioning. I'll show you mine if you show me yours;-)

Unfortunately, my model is written in HyperCard but I think it can be translated to Pascal VERY easily.

By the way; it's a one level model and I can find no prediction or anticipation in it. But I bet you can find some;-)

Best Rick

Date: Tue, 18 Apr 1995 15:21:39 -0500
From: Bruce Abbott <abbott@CVAX.IPFW.INDIANA.EDU
Subject: Re: Classical Conditioning Model

[From Bruce Abbott (950418.1515 EST)]

>Rick Marken (950418.1230) --

> I just finished my model of classical conditioning. I'll show you mine if you show me yours;-)

And Bill Powers said _I_ was a fast programmer! Mine's not done yet. The more I think about it, the more complexities I see in _realistically_ modeling a particular system, and I've been spending time thinking about those issues rather than just sitting down and coding a basic model. It's now about 3 pm and my brain, as has become its custom of late, is shutting down for its afternoon nap. I'll try to get the model done soon, maybe later this evening or sometime tomorrow.

> By the way; it's a one level model and I can find no prediction or anticipation in it. But I bet you can find some;-)

Yeah, so's mine. I like Chuck Tucker's (950418) suggestion, copied below. For the sake of clarity, what DO you mean by "prediction" and "anticipation?"

>>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.

Amen.

Back to Rick:

> my model is written in HyperCard

Remember that last stack you sent? I've gotten hold of a copy of Stuffit and several other utilites. (Actually, they turned out to be on my LC II all the time--I just didn't know they were there. Then one day I noticed this folder labeled "Utilities" and went "hmm, what's this?) What do I do to convert the code I received from you via email into Mac-readable format?

Regards, Bruce

Date: Thu Apr 20, 1995 1:21 am PST
Subject: CSG-L Digest - 18 Apr 1995 to 19 Apr 1995

There are 7 messages totalling 475 lines in this issue.

Topics of the day:

Date: Wed, 19 Apr 1995 11:58:00 EDT
From: "Cynthia R. Ernst" <crernst@EDCEN.EHHS.CMICH.EDU
Subject: Communication on perceptual control theory.

We are interested in communicating with researchers who employ the strategies of Ed Ford.

Cindi

Date: Wed, 19 Apr 1995 09:04:34 -0700
From: Richard Marken <marken@AEROSPACE.AERO.ORG
Subject: Language, Models, New View

[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 seems 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 langauge (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 understaood) 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 of predicting anything.

Bruce Abbout asks:

> I've gotten hold of a copy of Stuffit...What do I do toconvert the code I received from you via email into Mac-readable format?

Just run StuffIt and run "Decode BinHex" from the "Other" Menu and select my file as the one to be decoded. I think that's all you need. I might have archived it too; in that case just "open" the ".sit" file from the Stuffit "file" menu and "Extract" it.

If that works, I can send you a copy of my embarassingly simple "classical conditioning" model. It turns out that it is basically the same as Bill Power's model shown in his diagram. It still needs some tuning up, but it shows the basic phenomenon of classical conditioning; after the CS there is an immediate CR, which is "in progress" when the US occurs (if it occurs). The "anticipatory" CR is just the error driven output, R, of a control system that is controlling a perception of (CS+US+R).

Greg --

The New View catalog has arrived! I like the colors and the birdies are cute. A more detailed critique will be forthcoming.

Best Rick

Date: Wed, 19 Apr 1995 13:15:28 -0500
From: Bruce Abbott <abbott@CVAX.IPFW.INDIANA.EDU
Subject: Undefined

[From Bruce Abbott (950419.1310)]

>Rick Marken (950417.2215)

>Somehow we are apparently not communicating.

>>Bruce Abbott (950418.1515 EST)

>> For the sake of clarity, what DO you mean by "prediction" and "anticipation?"

>Rick Marken (950419.0900)

> The notion that these "silly discussions" are based on a failure to define terms strikes me as misleading.

O.K. Humpty, you just keep me guessing. I understand full well how the model bases its actions only on present-time perceptions, from which you conclude that anticipation is not involved in the model. The problem is entirely definitional.

ALL anticipation is based on present-time states, which are used to project what is likely to happen in the future. In your first "anticipation" model, the current state of the controlled perception and its first derivative at time tn are added to predict what state the controlled percption would be in at time tn+1 if no action were taken to prevent it; the result of p + dp/dt is just another current-time perception, but it is also a projection of p into the future. It is an estimate of where p will be at time tn+1. Our only disagreement, it seems to me, is whether to call this particular use of current states "anticipation."

Of course, it's not _necessary_ to use the term "anticipation" at all when describing this system. But then there is this phenomenon everyone has experienced personally. People would like to know how it works, how the phenomenon can be accounted for by PCT. My answer is that it depends on the situation; in this one it works like THIS [demonstrate model]. Your answer is that there is no such thing. Which approach do you think will generate more interest in PCT?

Regards, Bruce

Date: Wed, 19 Apr 1995 15:50:25 -0500
From: Bruce Abbott <abbott@CVAX.IPFW.INDIANA.EDU
Subject: Glory

[From Bruce Abbott (950419.1550 EST)]

>Rick Marken (950419.0900)

Just a couple of additional replies to Rick's post...

> which elicited the following beauty from Bill Powers:

Beauty is in the eye of the beholder. I thought they were neat, too, but argued that they apply to a different case--where the terms are used in an explanatory mode, not as mere names for phenomena. To return to our favorite example, one might ask how the Ptolemaic and Copernican systems explain the retrograde motions of the planets. The Ptolemaic theorist would answer by talking about epicycles; the Copernican theorist would answer by talking about the relative motions of the planets, including the Earth, as they orbit the sun, and the PCT theorist, I gather, would answer by saying that retrograde motion does not exist because the planets in fact never back up: it's just a "side effect" of the relative motions. The problem with that last explanation is that there are two "retrograde" motions: real and apparent. The real does not exist, but the explanation is supposed to apply to the apparent, which does.

> There are ways to talk about "information" (and "prediction" and "anticipation", etc) that ARE consistent with PCT. . . . The output of a control system sometimes does anticipate the disturbance to a controlled variable.

Yeah, the phenomenon I've been talking about. And HOW does it do this? You give the answer next:

> But this is just another way of saying that o = -d and that d(t) = f(d(t-dt).

Which is to say that it generates a perception, based on current perceptions, of the likely state of the variable in the next time-cycle, and begins to act on that perception in this time-cycle.

> The control system itself does not operate by anticipating or predicting anything.

That, my friend, is the very ESSENCE of prediciton. You see, the problem with this:

> 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.

is that the advantage of adding target velocity to the perceptual input function resides precisely in the predictiveness of the equation. If target position at time tn+1 did not tend to be closer to its tn + velocity value than to its tn value (i.e., the "prediction" always failed) then adding velocity would make control worse, not better, for a sluggish control system.

> 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".

These are just words. Humpty Dumpty doesn't wish to call p + dp/dt a predicted target position, but most of us, I suspect, wouldn't have the problem he does with doing so. Saying that control of p + dp/dt is "just control" fails to communicate how p + dp/dt improves control (when it does) relative to p alone. Saying that p + dp/dt predicts where p likely will be at time tn+1 makes it abundently clear, which is why I prefer the latter. When you come down to it, the two statements are equivalent, thus neither is "inconsistent" with PCT.

"There's glory for you!"

Regards, Bruce

Date: Wed, 19 Apr 1995 13:53:19 -0700
From: Richard Marken <marken@AEROSPACE.AERO.ORG
Subject: Re: Communication on perceptual control theory

[From Rick Marken (950419.1400)]

Cindi says:

> We are interested in communicating with researchers who employ the strategies of Ed Ford.

Communicate away!

Who are you? Where are you? How did you find us?

Best Rick

Date: Wed, 19 Apr 1995 20:11:20 EDT
From: mmt@BEN.DCIEM.DND.CA
Subject: Re: information, anticipation

[Martin Taylor 940419 19:30]

I've spent much of today plowing through nearly 300 messages, half of them from CSG-L. A weird and wonderful "discussion";-(on "anticipation" with Bruce Abbott

and Bill Powers both saying correct and mutually irrelevant things, with Rick chiming in in his usual inimitable way. The sequence starting April 8 or thereabouts is a real classic of misunderstanding piled on misunderstanding, or the substitution of what one anticipates reading for what is actually written. Maybe I'll comment when my brain gets a bit unfuzzed.

But I can't let this, from today's mail, pass uncommented, because it is an absolute falsehood.

>Rick Marken (950419.0900)

> 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.

"Information" was and is defined in one way and one way only, as reduction in uncertainty. Uncertainty has its own precise and technical definition, based on a subjective probability distribution. As used in the discussion, the definitions were those provided by Claude Shannon 50 years ago, and they were not (at that time or later) chosen so as not to contradict PCT.

Only you chose to redefine "information" in strange ways, making claims such as that if one was not able precisely to specify a value, one had no information about that value. "Information" never was redefined "in the hopes that one could talk about it in a way that did not contradict PCT," any more than "Laplace Transform" can be redefined in such a way that it does not contradict PCT. The discussion of information was an attempt, derailed by you whenever possible, to see how usefully the concepts of information theory could be deployed in considering problems of control.

I don't know why you have such a knee-jerk response to the notion of "information". It almost makes one believe in S-R theory.

======================

> Perception just doesn't have any information about the cause of the state of the controlled variable.

And, so far as I remember, not a single person in the "information" discussion ever suggested that it might. Perception knows not a whit, not a jot or tittle, of causes.

========================

> The control system itself does not operate by anticipating of predicting anything.

No, but EVERY control system depends on the fact that the real world in which its feedback loop exists is to some degree predictable. And if the real world were totally predictable there would never be a need for ANY control system. The construction of the control system, and in particular of its perceptual input

function, embodies its anticipation or prediction, as you yourself said in a well written posting (950410.0920).

It's all a matter of information rate. Too high and control is impossible, too low and control is unnecessary.

I have a puzzle that perhaps Bill Powers can resolve when he returns.

In (Bill Powers 950408.0715 MST) he talks about controlling a signal that varies upward from DC with a high-frequency cutoff, and compares it with controlling a signal that is a slow modulation of a high-frequency carrier.

At the end of it, he says:

> That was aimed more toward Martin Taylor than Bruce Abbott.

I am puzzled as to why. Bill, did you believe I thought you were unaware of the difference between bandwidth and high-frequency cutoff? I assure you that I assumed you to know the difference.

There is, in this passage, some discussion of a "pattern-matching system" as opposed to a "moment-by-moment" system. I did not follow the argument, because EVERY perceptual input function can properly be called a "pattern matching system" whose output is the degree to which the incoming data matches the pattern defined by the input function. If this is what was aimed at me, then I am more confused than ever. If you had some particular "pattern matching system" in mind, it was not clear from the context, and it did not ring any bells with me as something we had previously discussed.

> The pattern-matching system actually has a much narrower bandwidth than the MBM system, although the center of its bandwidth can be at a higher frequency. The PM system is designed to control the match between regular recurring patterns, but it can correct errors only slowly.

If any PIF is defined to have a narrow bandwidth, then of course it can correct errors only slowly. That's what narrow bandwidth means: low rate of variation, or, in other words, low information rate. A control system with a narrow-band PIF is a slow control system. If you thought I didn't believe you to know that, you were wrong again. I give you credit for knowing a lot more than me about how control systems work, as I would have thought you knew, given some of our earlier discussions.

All in all, I remain confused.

More later. Martin

Date: Wed, 19 Apr 1995 19:30:28 -0700
From: Richard Marken <marken@AEROSPACE.AERO.ORG
Subject: Belt or Cravat?

[From Rick Marken (950419.1930)]

Bruce Abbott (950419.1310) --

>O.K. Humpty, you just keep me guessing.

As you wish;-)

> I understand full well how the model bases its actions only on present-time perceptions, from which you conclude that anticipation is not involved in the model. The problem is entirely definitional.

> ALL anticipation is based on present-time states, which are used to project what is likely to happen in the future.

You're not thinking in circles yet;-)

The control model's actions are based on its present time perceptions but its present time perceptions are also (and simultaneously) based on it present time actions. Anticipation (of the future state of a variable from its present state) makes no sense in such a closed loop. The present time value of a perceptual signal cannot function as a basis for prediction of future values because, as soon as the prediction is made, it is invalidated by the present time perceptual signal's effect on itself.

> In your first "anticipation" model, the current state of the controlled perception and its first derivative at time tn are added to predict what state the controlled percption would be in at time tn+1 if no action were taken to prevent it;

That is the way I described it but Bill P. politely explained that my explanation was wrong. The "prediction" component of my model, dp/dt, did not function as a predictor; it was just the derivative of the input variable.

> the result of p + dp/dt is just another current-time perception, but it is also a projection of p into the future.

You can think of it as a projection of a future p but it doesn't function that way in the control model. From the model's perspective, p + dp/dt is NOT a prediction. Bill and I have been going ballistic precisely because you seem to be saying that an input variable like p + dp/dt _functions_ as a predictor variable in a control loop. It DOESN'T -- EVER. Not because we don't want it too but because this is impossible in a closed loop. The notion of a variable functioning as a "predictor" is an open-loop, cause effect notion. There is no "prediction" in a control loop.

> Our only disagreement, it seems to me, is whether to call this particular use of current states "anticipation."

Not quite. Our disagreement is whether the "current use" of dp/dt is as a "predictor". We are saying that the perceptual signal is never used as a predictor of ANYTHING. "Prediction" and "anticipation" make no sense in a closed loop where there is no before and after, no beginning and end. Each variable in a control loop is at any instant BOTH cause and effect, independent and dependent variable, "predictor" and "criterion" variable.

> Of course, it's not _necessary_ to use the term "anticipation" at all when describing this system. But then there is this phenomenon everyone has experienced personally. People would like to know how it works, how the phenomenon can be accounted for by PCT.

If "anticipation" refers to the observation that o(t) = -d(t+dt) then this is readily accounted for by phase lags and leads in control loops. But I think "anticipation" usually refers to our subjective experience of "thinking ahead". As I said earlier, I think this kind of "anticipation" is control of imagination (replayed perceptual signals).

> My answer is that it depends on the situation; in this one it works like THIS [demonstrate model]. Your answer is that there is no such thing. Which approach do you think will generate more interest in PCT?

I have never denied the existence of the _appearance_ of "anticipation"; the response to the CS in classical conditioning certainly looks like anticipation. What I deny is that the appearance of anticipation actually involves anticipation (in the sense that it is based on prediction of a future event). The control model of phenomena (like classical conditioning) that seems to involve anticipation shows that anticipation is an illusion. So here is Humpty's position on anticipation:

Appearance of anticipatory behavior: si.
Explanation of this behavior based on anticipation: no.

How we doin', Alice dear;-

Best H. Dumpty

End of CSG-L Digest - 18 Apr 1995 to 19 Apr 1995
****

Date: Fri Apr 21, 1995 7:21 pm PST
Subject: CSG-L Digest - 19 Apr 1995 to 20 Apr 1995

There are 11 messages totalling 774 lines in this issue.

Topics of the day:

1. Going Ballistic (2)
2. Glory, information, anticipation (2)
3. Communication on perceptual control theory.
4. New View Review
5. Internet/BBS Directory
6. Attention Sysops & Enterpeneurs.
7. Simultaneity and anticipation
8. Posting Code
9. Looking glass words

Date: Thu, 20 Apr 1995 11:10:26 -0500

From: Bruce Abbott <abbott@CVAX.IPFW.INDIANA.EDU
Subject: Going Ballistic

[From Bruce Abbott (950420.1105 EST)]

>Rick Marken (950419.1930) --

> You're not thinking in circles yet;-)

> The control model's actions are based on its present time perceptions but its present time perceptions are also (and simultaneously) based on it present time actions. Anticipation (of the future state of a variable from its present state) makes no sense in such a closed loop. The present time value of a perceptual signal cannot function as a basis for prediction of future values because, as soon as the prediction is made, it is invalidated by the present time perceptual signal's effect on itself.

It's rather strange--all these exchanges with Rick, and it takes Humpty Dumpty to make some sense out of it all. From now on, I'm only going to converse with H. D., who turns out to have quite a head on his, er, shoulders?

I see your point, H. D., but I have to think about it. It is clear that the model does behave as I described, if only because it does everything in discrete steps. But I'm not quite sure that your continuous-time view of the loop is quite right either as it ignores loop propogation delays.

> I have never denied the existence of the _appearance_ of "anticipation"; the response to the CS in classical conditioning certainly looks like anticipation. What I deny is that the appearance of anticipation actually involves anticipation (in the sense that it is based on prediction of a future event).

Well, you sure had me fooled. In fact, I was not talking about anticipation "based on prediction of a future event" either, until it came up in your specific model and I was drawn into that debate. I was talking about the appearance of anticipation, in that the control system begins an action to counter a disturbance prior to (or simultaneous with) the appearance of the disturbance, and the obvious effect of this action when the disturbance fails to appear. I said that this anticipatory action was possible because of the relationship between changes in one perception (which I termed the "predictive cue") and a subsequent disturbance. The time-delay between the cue and disturbance permit the control system, using only the current state of the cue, to begin an action ahead of the appearance of the disturbance. There are no computations of future states being made here.

Unfortunately, the terms "predictive cue" and "anticipation" set off a trip-wire of some kind and we were back to playing "who's on first?". You and Bill thought I was talking about some kind of S-R system in which the state of the cue was used to compute a response. I wasn't.

> Bill and I have been going ballistic precisely because you seem to be saying that an input variable like p + dp/dt _functions_ as a predictor variable in a control loop.

I never said anything like that until _you_ introduced it, but you guys "went ballistic" long before. It's an appropriate term--"ballistic" refers to an open-loop system. As my attempts to correct your misunderstanding (negative feedback) had no effect, it certainly would appear that you had, in fact, "gone ballistic."

>How we doin', Alice dear;-

My classical conditioning simulation is moving along slowly (I've had to work on other things), but perhaps I'll have something to post over the weekend. I'm attempting to build some detail into the model and allow parameter changes to be made "on the fly," which require a bit more time to implement than a simple version might.

It's a cravat, isn't it?

Regards, Alice

Date: Thu, 20 Apr 1995 09:18:02 -0700
From: Richard Marken <marken@AEROSPACE.AERO.ORG
Subject: Re: Glory, information, anticipation

[From Rick Marken (950420.0800)]

Greg -- I will try to get my review of the New View catalog out tonight but I can't resist responding to my two favorite disturbances -- Bruce Abbott and Martin Taylor.

Me:

> The output of a control system sometimes does anticipate the disturbance to a controlled variable.

Bruce Abbott (950419.1550 EST) --

> Yeah, the phenomenon I've been talking about. And HOW does it do this? You give the answer next:

>> But this is just another way of saying that o = -d and that d(t) = f(d(t-dt).

> Which is to say that it generates a perception, based on current perceptions, of the likely state of the variable in the next time-cycle, and begins to act on that perception in this time-cycle.

It is not to say that at all. The system does not "generate a perception based on current perceptions of the likely state of the variable in the next time-cycle". First, a control loop does NOT work in time cycles; all variables in the loop are changing continuously and SIMULTANEOUSLY. Second, no estimated perception is "generated" by the control loop; the derivative is not an "estimated perception"; it is the actual rate of change in the perceptual signal at a particular instant. You would see apparent anticipation in a control system (o(t) = -d(t+dt)) even if the variable controlled contained no derivative; as long as d(t) is approximately equal to d(t+dt) (which is true with the disturbances we use) then there will appear to be predictive control as long as there is good control (ie, p=r and o = -d)

> the advantage of adding target velocity to the perceptual input function resides precisely in the predictiveness of the equation.

The advantage has to do with the dynamics of the control system itself, not with the "predictiveness" of the derivative. Putting the derivative into p improves control by compensating for integral lag; if the control system is not an integral controller there will be no advantage to adding dp/dt.

> "There's glory for you!"

Looks more like "wish fulfillment" to me;-)

Your interpretation of how control works is still based on an S-R conception of behavior. You keep imagining that perception can "help" the controller when it has "predictivness". In PCT, perception is part of a control loop; it is both "helper" and "helpee" at the same time. The perceptual input isn't the start of a control "cycle"; and it doesn't "predict" anything; it is a controlled variable. If anything, the perceptual variable in a control loop is a DEPENDENT variable, with the reference signal as the independent variable. Try to think of it that way and and you will see that there is no reason for a dependent variable to predict its own future state. The perceptual variable does what it is told to do by the reference variable; and the perceptual variable has no way of predicting what the reference variable will make it do.

Martin Taylor (940419 19:30)--

> I don't know why you have such a knee-jerk response to the notion of "information". It almost makes one believe in S-R theory.

I'm just responding to disturbances to my perception of a correct represetnation of the nature of controlling. My behavior looks like S-R, doesn't it? That's why most people, including yourself, still believe in S-R.

Since you still don't seem to have been convinced by Bill Powers, Tom Bourbon and myself (to say nothing of all the demos and analyses) that your view of information is completely inconsistent with PCT, why not just give up on us. We aren't going to change our minds and apparently you're not going to change yours. I suggest that you go off and start your own list: bit.info.pct- taylor

Me:

> Perception just doesn't have any information about the cause of the state of the controlled variable.

Martin:

> And, so far as I remember, not a single person in the "information" discussion ever suggested that it might.

Aren't disturbances one cause of the state of a perceptual variable? Weren't you arguing that there is information about disturbances in perception?

> EVERY control system depends on the fact that the real world in which its feedback loop exists is to some degree predictable.

It may depend on predictability of the "real world" but it doesn't depend on the predictability of future states of perceptual variables (which is what Bruce and I have been arguing about). The predictability of the real world is irrelevant to the question of whether a control system ever operates by "predicting" future states of control loop variables.

Best Rick

Date: Thu, 20 Apr 1995 19:21:00 MET
From: Lars-Christian SMITH +352 67287 <LCSMITH@RESTENA.LU
Subject: Re: Communication on perceptual control theory.

[Lars Christian Smith (042095 1915 CET)]

To: Cindi

Re: Communications with PCT researchers

What would you like to know? One of the things I do is work in business organizations. I work with managers on strategies, scenarios, and how to implement change.

What do you do?

Best, Lars

Date: Thu, 20 Apr 1995 12:03:57 -0700
From: Richard Marken <marken@AEROSPACE.AERO.ORG
Subject: New View Review

[From Rick Marken (950420.1200)]

The New View catalog begins with the following definition of Control Theory:

> Control theory is a theory of human motivation and behavior...Control theory is based on the belief that motivation comes from within ourselves. We are always behaving to meet the five basic needs of love, power, fun, freedom and survival.

What is missing from this definition is any mention that control theory is about controlling. Without this, the definition is misleading at best. The belief that motivation comes from within ourselves is certainly not unique to control theory; what is unique if the idea that purposive human behavior involves the control of perceptual consequences of action. The stuff about "love, power, fun..." is, of course, nonsense and should be deleted.

I think a catalog of books on Control Theory should include an accurate description of both the theory AND what the theory is about: control.

The catalog then list 10 reasons for using control theory. I take serious exception to at least two of these purported reasons:

> 4. To gain more effective control of your class, your job and yourself

I think the last thing PCT would suggest is that you would want to get in control of a class (of kids) or of yourself. The whole point of PCT is that conflict results when one control system tries to control another control system (even when that other control system is in yourself). One reason for learning PCT is that it explains the difference between things that you can control and those you can't. People who try to control people (including themselves) just don't understand that human behavior happens to be one of the things you can't control; you can't control controllers.

> 5. To teach personal responsibility

You can't teach a control system personal responsibility; it simple IS responsible for the state of the variables it is controlling. It is NOT responsible for the state of variables it is NOT controlling, even if it has an effect on those variables. So you can't teach a control system to be responsible for what it is not responsible for. "Teaching responsibility" is similar to "holding people responsible" or "giving them responsibility". PCT shows that responsibility can be taken, but it can't be given away. A good reason to learn PCT is so that one can see what responsibility IS -- and so that one can tell the difference between behaviors (results of actions) for which a person is responsible and those for which he is not.

The catalog contains a whole lot of books that I will probably never read. But I have no problem with the fact that good PCT books are listed along with books of lesser quality. I DO have a problem with the definition of control theory and some of the reasons given for learning control theory. But perhaps we can work with New View to find a way of describing control theory and the reasons for learning it that are both accurate and intelligible to a lay audience.

Best Rick

Date: Thu, 20 Apr 1995 12:17:09 EDT
From: mmt@BEN.DCIEM.DND.CA
Subject: Re: Simultaneity and anticipation

[Martin Taylor 950420 11:00]
>Rick Marken (950419.1930)

>How we doin', Alice dear;-

Pretty badly, today. Not so badly other days, but then we all have our off days.

You get two ideas of "simultaneity" badly and importantly mixed up in your response to Bruce Abbott. That throws off your whole argument and invalidates your point.

(Bruce)

>> ALL anticipation is based on present-time states, which are used to project what is likely to happen in the future.

> You're not thinking in circles yet;-)

> The control model's actions are based on its present time perceptions

Yes, allowing for the inevitable delay in processing between perceptual function, comparator, output function, and environemental feedback path, and (with an integrator output function) the prolonged effects of any past error values.

> but its present time perceptions are also (and simultaneously) based on it present time actions.

Sorry. Should say "based on the actions at such past times as now are affecting the perceptual signal." Its present time actions have not yet affected the perceptual signal. You are mixing up the fact that all functions in the loop are acting simultaneously (which they do) with the notion that all signals in the loop have simultaneous effects all around the loop (which they don't).

> Anticipation (of the future state of a variable from its present state) makes no sense in such a closed loop. The present time value of a perceptual signal cannot function as a basis for prediction of future values because, as soon as the prediction is made, it is invalidated by the present time perceptual signal's effect on itself.

That's where the argument goes wrong, because of the mixup in the two notions of simultaneity. The present time perceptual signal does not have a present time effect on itself around the loop. There is ALWAYS transport lag, and in addition, most control systems contain elements such as integrators that extend the effect of an input over significant durations. Indeed, EVERY physical element of a control system has a finite bandwidth, and therefore extends and delays the effect on its output of any input.

One can and must think in circles, but signals take time to go round and round the circle, even though the whole circle is there all the time.

(Bruce)
>> In your first "anticipation" model, the current state of the controlled perception and its first derivative at time tn are added to predict what state the controlled percption would be in at time tn+1 if no action were taken to prevent it;

> That is the way I described it but Bill P. politely explained that my explanation was wrong. The "prediction" component of my model, dp/dt, did not function as a predictor; it was just the derivative of the input variable.

Bill P put it in another way, but you were not wrong the first time. The two statements are not mutually exclusive. You were predicting one time-step into the future, and your computational procedure ensured that the signal delay was at least that one time step. (I hope it was more, because if it was not, you left yourself open to computational artifacts that would spoil your simulation of the continuous control loop).

What you were doing was a rudimentary form of what is conventional called "linear prediction" as in "linear predictive coding" of speech. In linear prediction, the value of a function at time (t+delta) is obtained from the first N derivatives of the function at time t, as follows:

f(t+delta) = f(t) + a1*df(t)/dt + a2* d2f(t)/dt^2 + a3*.... + error

where all the derivatives are taken at time t. The values of the aN are the "coding" of the signal. There is always error in such a prediction, but proper choice of the aN for a particular signal makes the error very small compared with simply taking f(t+delta) to be f(t) + error. The term "error" is unpredictable, and with effective choice of aN it has an average value of zero. Getting the values of aN is a matter of adaptation (learning, reorganization), and is irrelevant to this discussion.

In a control loop f(t) is a perceptual signal, which includes both the results of the action of the control system and the disturbance. If the disturbance is truly random and the control loop including the feedback path is truly fixed, then the aN reflect entirely the properties of the control loop, and the "error" term reflects the disturbance. But if the disturbance has some regularity (as does a speech waveform), then the aN represent jointly properties of the control system and of the disturbance. In either case, the "error" term is what will dominate the difference between the perceptual signal and the reference, if the reference level is static.

Obviously, if all effects occurred instantaneously and there was no transport lag the effective delta would be zero, and prediction would be pointless, making all the aN zero. But as you found, adding even one aN term helps the control in a simulated control loop with finite transport lag and a finite gain integrator output stage (that was what you had, wasn't it?). Linear prediction helped.

Linear prediction is not always the best way to anticipate, but it is often useful, as you have found. It isn't useful when the "predictable" events are discontinuous, as in some of the examples used in the last couple of weeks' discussion.

If there were no loop delay and no integrator in the output function, and all elements had infinite bandwidth and infinite resolution, you would be correct that:

> The notion of a variable functioning as a "predictor" is an open-loop, cause effect notion. There is no "prediction" in a control loop.

If ANY of those conditions fail (as all but one of them do in any physical system), then prediction is an ESSENTIAL aspect of the control loop, whether it be explicit (in the perceptual function) or implicit (as in Bill P's artificial cerebellum).

In other words, this is wrong:

> "Prediction" and "anticipation" make no sense in a closed loop where there is no before and after, no beginning and end.

But this is right:

> Each variable in a control loop is at any instant BOTH cause and effect, independent and dependent variable, "predictor" and "criterion" variable.

> But I think "anticipation" usually refers to our subjective experience of "thinking ahead". As I said earlier, I think this kind of "anticipation" is control of imagination (replayed perceptual signals).

The subjective experience of "anticipation" is probably exactly this. One has little subjective experience of anything relating to a well-functioning control loop. Bill P, in a posting while I was away, said something to the effect that prediction had to be a logical, conscious, computation. I see no more reason to restrict the word to that sort of prediction than to restrict the word "perception" to one's conscious experience.

Words in everyday use ordinarily refer to everyday experience. If one restricts their uses to the everyday, one has to invent whole new languages when talking about things that happen in models that are not part of everyday experience. Bill P chose to use "perception" to refer to something in a theoretical model he devised. The "something" had useful correspondences with the everyday notion of "perception" but was not the same thing. We, who understand the model, have no problem with this, but neophytes do (I did, for the first few days of learning about PCT). Likewise, it seems perfectly legitimate to use "prediction" and "anticipation" to refer to effects within a model when those effects have valuable correspondences with the everyday uses of the words.

I remain, sir, in anticipation, Yours Sincerely (but, with apologies, not your most Humble and Obedient Servant)

Martin

Date: Thu, 20 Apr 1995 16:43:30 -0500
From: Peter Burke <pburke@BLUE.WEEG.UIOWA.EDU
Subject: Re: Going Ballistic

[From Peter Burke (950420.1645 CDT)]

> [From Bruce Abbott (950420.1105 EST)]

> My classical conditioning simulation is moving along slowly (I've had to work on other things), but perhaps I'll have something to post over the weekend. I'm attempting to build some detail into the model and allow parameter changes to be made "on the fly," which require a bit more time to implement than a simple version might.

Bruce, when you have something to post, I hope you will post it generally, as I for one, and I imagine there are otheres, would like to see it and play with it too.

Regard, Peter

Date: Thu, 20 Apr 1995 17:20:52 -0500
From: Bruce Abbott <abbott@CVAX.IPFW.INDIANA.EDU
Subject: Posting Code

[From Bruce Abbott (950420.1720 EST)]

> Peter Burke (950420.1645 CDT) --

>> [From Bruce Abbott (950420.1105 EST)]

>> My classical conditioning simulation is moving along slowly (I've had to work on other things), but perhaps I'll have something to post over the weekend. I'm attempting to build some detail into the model and allow parameter changes to be made "on the fly," which require a bit more time to implement than a simple version might.

> Bruce, when you have something to post, I hope you will post it generally, as I for one, and I imagine there are otheres, would like to see it and play with it too.

Sure will, as I have in several other instances in the past.

Regards, Bruce

Date: Thu, 20 Apr 1995 19:12:31 EDT
From: mmt@BEN.DCIEM.DND.CA
Subject: Re: Glory, information, anticipation

[Martin Taylor 950420 18:20]
>Rick Marken (950420.0800)

I told Bruce Abbott you'd roast him over the coals on "time-cycle," and he told me that he had already started the fire and was cooking nicely.

He knows. Don't worry.

>>Martin Taylor (940419 19:30)--

>> I don't know why you have such a knee-jerk response to the notion of "information". It almost makes one believe in S-R theory.

> I'm just responding to disturbances to my perception of a correct represetnation of the nature of controlling. My behavior looks like S-R, doesn't it? That's why most people, including yourself, still believe in S-R.

One of the great mysteries of life is why you believe this. It's been quite some many years since I did believe in S-R. But it's true, your behaviour does tempt me to reverse my belief in PCT in favour of S-R :-)

Actually, I stopped believing in cause-effect relationships long before I heard of PCT, perhaps 10-15 years before. I think there's more cause-effect taken seriously by the PCT old hands than there is in my mind. As for S-R, I don't think I had heard of PCT at the time I wrote a BBS review (1990) of The Emperor's

New Mind, in which I criticized Penrose for essentially asserting an S-R view of the mind. He ignored the criticism in his response article, but I think it is a totally devastating criticism, independent of whether you are a believer in PCT.

> Since you still don't seem to have been convinced by Bill Powers, Tom Bourbon and myself (to say nothing of all the demos and analyses) that your view of information is completely inconsistent with PCT, why not just give up on us.

Whether you choose to take note of the demos and analyses is up to you, but that's irrelevant to whether you misrepresent what is said about information, except that your misrepresentation allows you to say:

> We aren't going to change our minds

Speak for yourself, not for Bill P or Tom. Bill has repeatedly said that given evidence he would change his mind. He says he just hasn't seen adequate evidence, and for that I can't fault him. But I can believe that you intend not to change your mind, because you never respond to what is said in respect of "information," in favour of responding to your own imaginary notions of what you would like me and others to have said.

>> Perception just doesn't have any information about the cause of the state of the controlled variable.

Martin:

>> And, so far as I remember, not a single person in the "information" discussion ever suggested that it might.

> Aren't disturbances one cause of the state of a perceptual variable? Weren't you arguing that there is information about disturbances in perception?

You are carrying sophistry a bit far, even for you. A "cause" is something like, for example, a waveform generator that puts a signal on a line, or a gust of wind that pushes the proverbial car. The "disturbance effect" is the influence of that cause on something that IS perceived. The cause is not. The argument is that the perceptual signal passes (not contains) information about the disturbance effect, and indeed that it is ONLY this fact that allows the control system to operate at all.

You insist on saying that I talk about the information "in" the perceptual signal when I have tried several dozen times to correct that misconception. The perceptual signal in a functioning control loop never "contains" information about the disturbance. The use of that information by the control system destroys it, so far as the perceptual signal is concerned.

You continue to insist that I say that the information passed by the perceptual signal is about the cause of the disturbance effect, although that misunderstanding had been cleared up (at least between Bill P and me) within the first few messages a couple of years ago.

You continue to insist that if it is impossible to disentangle the contributions of the disturbance and the control system output as influences on the CEV, then the perceptual signal passes information about neither.

In other words, you set up straw men in fields quite distinct from the field I try to farm, and when you have destroyed those straw men you assert that I therefore cannot grow vegetables in my own field on the other side of the fence. I don't even SEE your straw men in my field, but I hear you shouting that you have happily destroyed them. But of course, you won't change your mind about whether nutritious vegetables might be growing, because in your field, you can't find any. Too bad.

>> EVERY control system depends on the fact that the real world in which its feedback loop exists is to some degree predictable.

> It may depend on predictability of the "real world"

Good. There's a basis for agreement.

> but it doesn't depend on the predictability of future states of perceptual variables (which is what Bruce and I have been arguing about). The predictability of the real world is irrelevant to the question of whether a control system ever operates by "predicting" future states of control loop variables.

Your own work shows that it can.

There's something here that seems awfully difficult for you to grasp. You think a computation is required for there to be a prediction. But the dynamics of the control system's operation IS its prediction for the future. Suppose that the disturbance made a step change and then stopped changing. The control system's output, and thus the perceptual signal, spend some time either approaching a steady state or performing a damped oscillation around it. That approach or oscillation is built into the parameters of the control loop. It represents a prediction of how the world acted on by the control system changes.

If, instead of a dynamic that depends on the gain and delays in the loop, you had a "predictive computation" in the PIF (or in the output function), making explicit the "anticipation" or "prediction" of the control loop, you would get the same result (if the disturbing influence had up to that point been a Gaussian noise). There's no way to tell the difference between a computed prediction and a prediction built into the parameters of the control loop. Adaptive control is all about modifying the parameters so that they better predict. The same could be done in the algorithms of an explicit predictor. You would never know the difference.

When you talk (as you have) about the "predictive" changes in a reference signal from a higher level as being only the results of that higher level's control of its own perception, you are absolutely correct. But that "only" makes the result no less predictive, from the more restricted viewpoint of the lower-level control that receives these apparently magical advance warnings of disturbances yet to happen.

"It's all perception" -- right? And whether something is prediction or observation is a matter of viewpoint. What is prediction, as seen from within one control loop, is observation as seen from some other control loop (which may include the other's perceptual signal as one of its inputs). It's the same old game: do you take the viewpoint from within the control system, or the external (analyst's or observer's) viewpoint.

Bill P had this all correct. You say that you agree with him. Why don't you also have it right? Is it that you imagine his words always to confirm yours, in the same way that you imagine mine always to have meanings with which you can disagree?

> I suggest that you go off and start your own list: bit.info.pct- taylor

Actually, we did, but not under that name. It's called "Inpercon" (Information in Perceptual Control), but the list is moribund now--hasn't been a posting in perhaps a year. No disturbances to people's perceptions. Posting here is more fun. One never knows how a rational statement will be interpreted.

=====================
Hey--how's your IJHCS paper coming along?

Martin

Date: Thu, 20 Apr 1995 19:32:07 -0700
From: Richard Marken <marken@AEROSPACE.AERO.ORG
Subject: Looking glass words

[From Humpty Dumpty (950420.1930)]

Bruce Abbott (950420.1105 EST) --

> It's rather strange--all these exchanges with Rick, and it takes Humpty Dumpty to make some sense out of it all.

Pay no attention to that "Rick" behind the curtain -- oops, mixing fantasies;-)

Now, just what was it you were saying about "anticipation" and "prediction"?

> I was talking about the appearance of anticipation, in that the control system begins an action to counter a disturbance prior to (or simultaneous with) the appearance of the disturbance

Yes. A fascinating phenomenon. Rather like "reinforcement" and "reacting" in its ability to beguile one into thinking something is actually going on.

> The time-delay between the cue and disturbance permit the control system, using only the current state of the cue, to begin an action ahead of the appearance of the disturbance. There are no computations of future states being made here.

Ok. I see what you're getting at. You are just using S-R words to describe it; let me try to do it in PCT words since I don't have to pay them extra;-)

In PCT, there are no cues; there are only disturbances to perceptual variables. What you are saying (translated into PCT) is that there is a time delay between two disturbances to a variable that is under control; as the on-going output of the control system begins to change to correct for the first disturbance, the second disturbance appears for which the changed output is appropriate. The

change in output occasioned by the effects of the first disturbance on the controlled variable looks like an "anticipatory response" to the second disturbance. But, of course, there is really no anticipation or prediction; just control of input. I think it's best to call this phenomenon "coincidence" rather than "anticipation" or "prediction", but, then I'm very stingy when it comes to paying words.

So the appearance of anticipation in classical conditioning is, like the appearance of reinforcement in operant conditioning, an illusion. We can save ourselves a lot of money (and intellectual confusion) if we just stop applying expensive words like "anticipation" and "reinforcement" in situations where they are inappropriate (I do anticipate reinforcing the foundation of my wall to prevent it from falling in an earthquake -- have I told you about looking glass earthquakes? they shake backwards, of course;-).

> It's a cravat, isn't it?

Yes. Isn't it obvious;-)<

Best H. Dumpty

End of CSG-L Digest - 19 Apr 1995 to 20 Apr 1995
****

Date: Sat Apr 22, 1995 2:16 am PST
Subject: CSG-L Digest - 20 Apr 1995 to 21 Apr 1995

There are 10 messages totalling 645 lines in this issue.

Topics of the day:

Date: Fri, 21 Apr 1995 06:10:00 EST
From: Hortideas Publishing <0004972767@MCIMAIL.COM
Subject: Re: New View Review

Thanks, officer!

As ever,

Greg Williams [940421]

Date: Fri, 21 Apr 1995 14:54:00 MET
From: Lars-Christian SMITH +352 67287 <LCSMITH@RESTENA.LU
Subject: Re: Away for a while; Powers's AERA paper

[Lars Christian Smith (012195 1440 CET)]

To: Gary Cziko

Re: Your book _Without Miracles: Universal Selection and the Second Darwinian Revolution_.

1. What is "universal selection theory"?

2. By the 'second Darwinian revolution", do you mean the Williams- Hamilton-Trivers etc. stuff on inclusive fitness?

3. What is the argument of the book (I probably want to buy it, but I would like a sneak preview)?

Best, Lars

Date: Fri, 21 Apr 1995 09:50:01 EDT
From: "CHARLES W. TUCKER" <N050024@UNIVSCVM.BITNET
Subject: Anticipation and an act

FROM CHUCK TUCKER 950421

What about this comment from my 950418 post?

> The notion of anticipation and predictive sensing clearly depend upon when one arbitrarily begins an act. As Dewey pointed out in his critique of the S-R formulation (1896) a part of the act appears to be a "stimulus" because one "begins" an act at a certian point in the "stream of activity." If we parse any act we have to begin somewhere in that stream but we should not forget that the organism was doing something before we decided where we started the act to parse. So it we start with a "cup being lifted toward the mouth" (for an act of drinking) we should not forget that prior to that there was "a hand grasping the handle of the cup" and prior to that "a hand reaching toward the handle of the cup" and prior to that "the forming of the hand to grasp the handle of the cup" and prior to that ...... You should get the point. All of these parts of the act can be parsed in PCT terms. If you don't wish to label the action prior to when you start the act 'anticipation' then call it by another name but let's parse the act first so we have a notion of what action we have under analysis. ....

The above, it seems to me, speaks to the question of "anticipation" quite directly. The point is: a single closed loop process does NOT involve "anticipatition" BUT an action as part of a sequence of actions (which can be designated as an act) can be an action which can "anticipate" another action.

Parse any act in very small action units and you will make this feature of acts, e.g., do you "cover your brake pedal" [hold your foot above but on a parrallel plane to it] just prior to "putting your foot on the brake pedal" which is just prior to your action of "pushing on the brake pedal" [or "mashing it" as is said down here] when you attempt to stop your vehicle? Some actions "anticipate" or are "prior and preparatory to others."

In addition to definitions (which may not be a problem for Rick but appears to be a problem for most of the people who answer his posts!) I would think that many of the confusions could be reduced by describing an act and parsing it so we have some example to discuss.

An example will be posted soon.

Regards, Chuck

Date: Fri, 21 Apr 1995 09:51:56 EDT
From: "CHARLES W. TUCKER" <N050024@UNIVSCVM.BITNET
Subject: Favorite examples of partially parsed acts

ACTS-WTP.DOC

A WAY OF POURING A GLASS OF JUICE AND PUTTING PITCHER IN REFRIGERATOR

BEHAVIOR	MEANS	VARIABLE	REFERENCE
COMMON-LANGUAGE DESCRIPTION OF "BEHAVIOR" OR "ACTION"	BEHAVIOR OR ACTION THAT TRANSPIRES	VARIABLE AFFECTED BY BEHAVIOR OR ACTION	INTENDED STATE OF THE VARIABLE
open door of refrigerator	grasp handle, pull	angle of door	80-90 degrees
take pitcher from refrigerator	grasp, lift	distance of pitcher from refrigerator	2 feet from refrigerator
close refrigerator door	push	angle of door	zero degrees
place pitcher on counter	grasp, lower	distance of pitcher to counter	zero distance
open cupboard door	grasp, pull	angle of door	80-90 degrees
take glass from cupboard	grasp, lift	distance of glass from cupboard	2 feet from cupboard

put glass on counter	grasp, lower	distance of glass from cupboard	zero distance
close cupboard door	push	angle of door	zero degrees
pour juice in glass	grasp pitcher lift, tilt	amount of juice in glass	(?) 1/2 " from lip of glass (?)
open refrigerator	grasp handle, pull	angle of door	80-90 degrees
put pitcher in refrigerator	grasp, lift	distance between pitcher and refrigerator	zero distance
close refrigerator door	push	angle of door	zero degrees

CONTROL ACTIONS INVOLVED IN GETTING READY (FIXIN') TO BACK A STICK-SHIFT TRUCK OUT OF THE DRIVEWAY *

BEHAVIOR	MEANS	VARIABLE	REFERENCE
COMMON-LANGUAGE DESCRIPTION OF "BEHAVIOR" OR "ACTION"	BEHAVIOR OR ACTION THAT TRANSPIRES	VARIABLE AFFECTED BY BEHAVIOR OR ACTION	INTENDED STATE OF THE VARIABLE
Open door	Grasp, pull	Angle of door	80 degrees
Get in	Bend, sit, slide	Relationship to seat	Seated
Shut door	Grasp, pull	Angle of door	0 degrees
Fasten belt	Push together	Distance between fastners	Zero distance
Adjust rear view mirror	Grasp, twist mirror	Displacement rear window image	Zero displacement
Depress clutch	Push with left leg	Extention of leg	Fully extended
Insert key	Extend arm	Distance, key to keyhole	Zero distance

Start engine	twist	Sound of starter and engine	Whirrrrr, vroom
Shift to reverse	Grasp, push	Position of shift lever	Coordinates of reverse gear

*From W. T. Powers (1979)

Date: Fri, 21 Apr 1995 10:46:41 -0500
From: Bruce Abbott <abbott@CVAX.IPFW.INDIANA.EDU
Subject: Paying Extra

[From Alice (950421.1045 EST)]

>Humpty Dumpty (950420.1930) --

> Ok. I see what you're getting at. You are just using S-R words to describe it; let me try to do it in PCT words since I don't have to pay them extra;-)

(I never thought of "cue" as an S-R word. Please continue...[your cue!])

> In PCT, there are no cues; there are only disturbances to perceptual variables. What you are saying (translated into PCT) is that there is a time delay between two disturbances to a variable that is under control; as the on-going output of the control system begins to change to correct for the first disturbance, the second disturbance appears for which the changed output is appropriate. The change in output occasioned by the effects of the first disturbance on the controlled variable looks like an "anticipatory response" to the second disturbance. But, of course, there is really no anticipation or prediction; just control of input. I think it's best to call this phenomenon "coincidence" rather than "anticipation" or "prediction", but, then I'm very stingy when it comes to paying words.

Ah, but I find that when I use words like "disturbance" in this context, I have to pay them double. They insist on it, because they have to do so much extra work. "Disturbance" in the above description is called upon to play two distinct roles, first as cue and second as the main disturbance to the controlled perception. To keep the audience from becoming entirely confused, the poor word has to hire support-words like "first" and "second" to carry the distinction. And sad, overworked "disturbance" still has trouble communicating the implicit meaning that "cue" communicates in such stentorian tones--the role the "first disturbance" plays in our little scene.

But let's not bicker about words (they bicker enough among themselves); I mean by "cue" exactly what you mean by "first disturbance," as I indicated previously (950417.2035) when discussing classical conditioning (in which the "cue" is called the "CS"):

>> Through some kind of reorganization process, the CS's influence within the input transfer function increased so that the CS was rendered capable of disturbing the perceptual variable; the system would then begin an action to counter the disturbance produced by the CS before the main disturbance had begun.

Sound familiar?

> So the appearance of anticipation in classical conditioning is, like the appearance of reinforcement in operant conditioning, an illusion.

Responding first to what I believe you MEAN, yes. Responding next to what you WROTE, that would depend how you define those terms--as names for observable phenomena or as explanatory constructs. If you wouldn't be so stingy about paying your words, they probably would communicate your intent more clearly.

But then, here in Wonderland, _everything_ is illusion...

Your Most Humble and Faithful Disturbance

Alice

Date: Fri, 21 Apr 1995 10:03:44 -0700
From: Richard Marken <marken@AEROSPACE.AERO.ORG
Subject: Prediction, information and PCT

[From Rick Marken (950421.1000)]

Martin Taylor (950420 18:20) --

> the dynamics of the control system's operation IS its prediction for the future.

Where is the future in a circle of cause and effect? Is the perception that is happening now the current cause or the future result of itself? I can see thinking of current and future in terms of the state of all variables in the loop taken simulataneously. The control loop is a wheel of causation; all points on this wheel change at the same time. So all points have their present values and all points will change and have differnt values some time in the future. It makes no sense (to me) to pick some point on this wheel and say that what happens past that point is future and what happens before that point is past. The points that are after any point are before it too.

I am interested in why you want to think that there is "prediction" in a control loop? I can tell you why I don't like this imagery. The reason is that it seems to focus attention on perception as a basis for future action. To me, the important (and revolutionary) message of PCT is that perceptions (some of them) are under control; the organism is viewed as a collection of reference signals that demand (and get) particular values of the perceptual signals they control. The main goal of PCT research is to determine the environmental correlates of the perceptual variables that are actually being controlled.

Conventional psychology focuses on perception as a basis for action, rather than as something that is controlled. The notion of "prediction" seems to focus attention on perception as a basis for output; that is, it orients one's perspective toward a conventional view of behavior. One starts to ask questions about what variables might serve as "cues" or "predictors" of future events

instead of asking the questions that are actually relevant to control -- ie. what variables are under control.

> The argument is that the perceptual signal passes (not contains) information about the disturbance effect, and indeed that it is ONLY this fact that allows the control system to operate at all.

Again, I wonder why you want to think this is the case. Besides the fact that there is no evidence that it is the case (other than you're claim that this is the only thing that allows control systems to operate at all) it seems that such a notion once again focuses attention on perception as a basis for output rather than as a variable under control.

Again, the central tenet of PCT is that perception is controlled. Even if the perceptual signal does "pass" information about the disturbance effect, this can only be of incidental interest to PCT (in fact, we have still been shown no way to detect or measure the information about the disturbance effect that is being "passed" by the perceptual signal; which is why Bill and Tom and I are not convinced that there is any such information being "passed").

The facus of our interest should be on determining the variables that are controlld when we see organisms behaving in various ways. It is hard to maintain that focus when people claim (quite incorrectly, I maintain) that there is information being "passed" by the perceptual signal. The idea that information about disturbance effects is "passed" by perception is compatible with conventional ideas about the role of perception in behavior. It is not the kind of thing that will help conventional psychologists realize that they should be looking for controlled perceptions instead of doing what they are currently doing -- looking for the information in perception that guides behavior.

> Hey--how's your IJHCS paper coming along?

It's not.

Best Rick

Date: Fri, 21 Apr 1995 13:28:31 -0500
From: Bruce Abbott <abbott@CVAX.IPFW.INDIANA.EDU
Subject: Anticipation Debate Summary

[From Bruce Abbott (950421.1325 EST)]

Our discussion of anticipation, prediction, and classical conditioning has covered quite a bit of ground; perhaps now would be a good time to stop, take a look around, and see where we are. I'll try to summarize what I perceive to be the current issues, positions, and conclusions.

By now, nearly everyone probably has forgotten what kicked this whole discussion off, so I'll start by reprinting some of that post (Bruce Abbott [950407.1025 EST]):

> Bill, you mentioned the fact that a person will stiffen both the biceps and triceps when anticipating the impact of an object being dropped into the

> hand. A similar phenomenon occurs when you have been, say, lifting a series of fully packed, heavy boxes. If, unknown to you, the next box is only a quarter full, you will nearly throw the box into the air as you lift it. Because of the your prior experience with the heavy boxes, you will have set your reference for initial force exerted on the box to a much higher level than that actually required. This again reveals an anticipatory action that depends, not on current feedback from the low-level sensors, but on predictive cues whose significance depends on prior experience.

I think we agree that the type of anticipatory action I described above can be accounted for by a two-level model in which the higher-level system responds to a disturbance that is regularly followed in time by a disturbance acting on the lower-level system's controlled perception. The action of the higher-level system alters the value of the reference to the lower-level system, thus producing an action of the lower-level system that precedes or coincides with the second disturbance. The action of the lower-level system thus appears to anticipate its own disturbance. This is, of course, the same analysis I offered above.

As the discussion developed, it soon included classical conditioning. In classical conditioning, a CS regularly precedes a second event, the US, and the US appears to trigger an action, the UR. After the regular pairing of CS and US, the CS also appears to trigger an action, the CR. I think we agree that the classically conditioned organism can be modeled as follows. The US can be viewed as producing a disturbance to a one-level control system, and the UR is just the action of the system to oppose the effect of the disturbance. After conditioning, the CS produces a disturbance to the same one-level system; because the CS precedes the US by a fixed interval, the action of the system to oppose the disturbance appears just prior to or coincident with the disturbance produced by the US and thus seems anticipatory. [Note: this is just a simple, preliminary model.] This is the basic model given by Bill Powers and attributed by him to Wayne Hershberger.

A third example of anticipation surfaced in Rick Marken's "anticipation" model, which used both the current state of the perceptual variable and its first derivative to improve control of a "sluggish" control system. Currently there is disagreement as to whether this system does or does not behave in an "anticipatory" fashion. This disagreement appears to center around whether the action of the system should be viewed as anticipating the future value of the disturbance (via a linear equation) or just offsetting integral lag in the output transfer function.

There is also disagreement about the use of terms. Bill Powers has argued that terms like "predictive cue" are misleading because they appear to embue the events with properties that in fact they do not have. For example, "predictive cue" suggests that the cue predicts something, but Bill pointed out that the prediction is being done, if at all, by the organism using the cue; the cue itself does not do the predicting. I suggested that the relationship between the predictive cue (event) and the subsequent disturbance is an empirical fact and that this is all that I meant by the term "predictive," i.e., that a change in one perception regularly precedes another. Such a relationship, I noted, is a necessary condition for the empirical effect we have been labeling "anticipation" to occur. Martin Taylor noted that the legitimacy of the use of such terms depends on whether you are describing the system from within the system or from the point of view of an outside observer.

There was also an initial disagreement that appeared because of a mistaken perception that my concept of anticipation involved an S-R mechanism in which outputs were computed from inputs. I believe this problem has been cleared up, and that we now believe ourselves to be talking about the same models, even if we do not always agree on the words used to describe them. At least I hope so.

If anyone has a different opinion of the current state of these issues, you are welcome to offer it. But rather than continuing to argue about words, I would like to focus on the modeling problem, specifically with respect to classical conditioning.

Regards, Bruce

Date: Fri, 21 Apr 1995 12:42:43 EDT
From: mmt@BEN.DCIEM.DND.CA
Subject: Re: Looking glass words

[Martin Taylor 950421 12:20]
>Humpty Dumpty (950420.1930)

> So the appearance of anticipation in classical conditioning is, like the appearance of reinforcement in operant conditioning, an illusion.

It's all a question of viewpoint, I think. If you take the animal/person/control-system-hierarchy as a skin-bound black box, then you can see anticipation all over the place. A noise happens, the person goes and opens a door, "anticipating" that someone will be standing behind it. The dog salivates some time after a bell, but before the food arrives. And so forth. These are clearly anticipations in the sense that an appropriate action occurs to match a disturbance that has not yet happened.

If "you" are the control system that executes the action that will (as it happens) compensate for the future disturbance, so that "you" have then less active control to do, it seems that "you" have been given advance warning through your changing reference signal by a "superior officer" who "knew" what was going to happen. The "superior officer" was able to anticipate what "you" could not.

If "you" are that "superior officer" control system, "you" are simply controlling a present time perception based on your own PIF, with no prediction or advance anticipation at all. The "prediction" is only seen from outside "you." An analyst might be justified in saying that it is hidden in the form of "your" PIF (or perhaps "your" output function).

What Rick says, and has said in prior postings, seems quite plausible to me (I do have a little problem with the notion that the two distinct disturbances are to the same CEV--I'd be happier with the idea that there is one disturbance to a CEV extended in time, but that's a whole 'nother discussion):

> In PCT, there are no cues; there are only disturbances to perceptual variables. What you are saying (translated into PCT) is that there is a time delay between two disturbances to a variable that is under control; as the on-going output of the control system begins to change to correct for the first disturbance, the second disturbance appears for which the changed

output is appropriate. The change in output occasioned by the effects of the first disturbance on the controlled variable looks like an "anticipatory response" to the second disturbance. But, of course, there is really no anticipation or prediction; just control of input.

But this is correct only from one of the many possible viewpoints. This is presumably what is actually happening within one ECU (Elementary Control Unit) out of the many in a hierarchy. From other places in the hierarchy, and from outside the hierarchy, there IS anticipation and prediction, which IS just control of input.

When you are listening to a favourite piece of music, you (I mean music-lover Rick) can probably quite accurately judge just when a particular sforzando chord will occur after a pause, and if you wanted, you could play conductor to give the downbeat for it a few tens of msec before it occurs. That's "anticipation" by any definition, everyday or precise. That it occurs because of simple control of input inside your control hierarchy is an interesting fact, which you know because you know HPCT to be a correct description of your functioning. Doesn't stop it being anticipation, or stop the words "anticipation" and "prediction" being useful in a more than casual sense.

> I think it's best to call this phenomenon "coincidence" rather than "anticipation" or "prediction", but, then I'm very stingy when it comes to paying words.

"Coincidences" that happen time after time rather stretch the everyday meaning of the word, though I grant that it is properly used as a technical term in this context, meaning "events that occur together." There's less confusion if we use "prediction" or "anticipation" when we are dealing with viewpoints in which that is what is seen, "coincidence" or simply "control of input" when we are dealing with the ECU in which this happens.

>> It's a cravat, isn't it?

> Yes. Isn't it obvious;-)<

I thought it was "happily speaking with forked tongue" ;-)

Martin

Date: Fri, 21 Apr 1995 20:57:58 -0500
From: Bruce Abbott <abbott@CVAX.IPFW.INDIANA.EDU
Subject: Here We Go Loop Dee Loop

[From Bruce Abbott (950421.2055 EST)]

>Rick Marken (950421.1000)

> Where is the future in a circle of cause and effect? Is the perception that is happening now the current cause or the future result of itself? I can see thinking of current and future in terms of the state of all variables in the loop taken simulataneously. The control loop is a wheel of causation; all points on this wheel change at the same time. So all points have their

present values and all points will change and have differnt values some time in the future. It makes no sense (to me) to pick some point on this wheel and say that what happens past that point is future and what happens before that point is past. The points that are after any point are before it too.

A step disturbance at time t changes x, which changes i, which changes p, which changes e, which changes o, which then changes x in a direction opposite to the effect of the disturbance, thus beginning to counter the effect of the disturbance on x. At that same moment, x, i, p, e, and o were already changing, continuously, each at its own rate. But the changes due the step disturbance did not occur at x, i, p, e, and o simultaneously, even though all variables were in simultaneous "motion." This is true even in electronics, where these effects may propagate around the loop at nearly the speed of light. The "wave" of change due to the step disturbance goes around the loop at finite speed; it does not "hit" all variables simultaneously. The value of p at the moment prior to the step input is not its value after the wave has made one complete cycle of the loop; the VARIABLES that are after any VARIABLE are before it too; the VALUES of each variable that come after each value are NOT before it, too, and this is the only relevant consideration to this discussion of past and future in the loop. These dynamic changes have a past and a future. As I look at the display of disturbance and mouse movements, I do not see a repeating cycle in those values.

> Conventional psychology focuses on perception as a basis for action, rather than as something that is controlled. The notion of "prediction" seems to focus attention on perception as a basis for output; that is, it orients one's perspective toward a conventional view of behavior. One starts to ask questions about what variables might serve as "cues" or "predictors" of future events instead of asking the questions that are actually relevant to control -- ie. what variables are under control.

Well now at least I understand the motive for your objection; you are still fighting the S-R dragon. That dragon doesn't worry me at all--he's as good as dead, so far as I'm concerned. He just doesn't know it yet. This effort to construct a barrier between PCT and conventional psychology is in my opinion misguided and self-defeating. As you may have noticed by now, I'm more interested in building bridges than motes.

In PCT, asking questions about what variables might serve as "cues" or "predictors" of future events is the same as asking what variables might act as disturbances to the controlled perception, and to answer that question, you must first know what perception is being controlled. In classical salivary conditioning, for example, you must identify the controlled perception (taste intensity?). After that, you might ask how that perception is affected by food-in-mouth, what the reference level is, how error is translated into salivation, what effect salivation has on the controlled perception. You must then identify how the CS comes to act as a disturbance to that system, if indeed that's how it works. You must develop a testable model and then put it to test. All of this is pure PCT research; no sign of the dragon anywhere, no danger of running off willy-nilly in a fruitless search for the perceptual causes of behavior. I do not share your concern.

> The focus of our interest should be on determining the variables that are controlled when we see organisms behaving in various ways.

That's a beginning. But it's also important to determine how.

Regards, Bruce

End of CSG-L Digest - 20 Apr 1995 to 21 Apr 1995
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Date: Sat Apr 22, 1995 9:22 pm PST
Subject: CSG-L Digest - 21 Apr 1995 to 22 Apr 1995

There are 2 messages totalling 163 lines in this issue.

Topics of the day:

1. Pointing at phenomema
2. Bridges

Date: Sat, 22 Apr 1995 09:04:18 -0700
From: Richard Marken <marken@AEROSPACE.AERO.ORG>
Subject: Pointing at phenomema

[From Rick Marken (950422.0900)]

Alice (950421.1045 EST) --

> I never thought of "cue" as an S-R word.

"Cue" means the same thing as "stimulus"; it is a signal or prompt for behavior. In PCT, there are no external prompts to behavior; behavior is the control of perception.

> "Disturbance" in the above description is called upon to play two distinct roles, first as cue and second as the main disturbance to the controlled perception.

No. It only plays the role of disturbance -- always.

> And sad, overworked "disturbance" still has trouble communicating the implicit meaning that "cue" communicates in such stentorian tones--the role the "first disturbance" plays in our little scene.

But the disturbance doesn't play the role of cue; it is just a disutrbance. It doesn't cue anything; cueing is your _interpretation_ of the effect of the disturbance.

Me:

> So the appearance of anticipation in classical conditioning is, like the appearance of reinforcement in operant conditioning, an illusion.

Alice:

> Responding first to what I believe you MEAN, yes.

Great!

> Responding next to what you WROTE,

But I wrote what I meant.

> that would depend how you define those terms--as names for observable phenomena or as explanatory constructs.

The words "anticipation" and "reinforcement" are not just names for observable phenomena; they include implicit explanations of those phenomena. You don't see "anticipation" in classical conditioning; you see saliva flowing before food enters the mouth. Calling that anticipation is like saying that the clouds that typically form before it rains "anticipate" the rain. Nor do you see "reinforcement" (which means "somthing that strengthens") in operant conditioning. What you see is an organism that continue to produce certain consequences.

Calling these phenomena "anticipation" or "reinforcement" is not just a matter of "pointing" at them; there is an implicit interpretation that goes along with these words. I believe that these implicit interpretations are a problem becuase they direct our attention away from what is common to all these phenomena; the fact that there is a perceptual variable under control.

Best Humpty

Date: Sat, 22 Apr 1995 14:34:50 -0700
From: Richard Marken <marken@AEROSPACE.AERO.ORG>
Subject: Bridges

[From Rick Marken (950422.1440)]

Bruce Abbott (950421.2055 EST) --

> The "wave" of change due to the step disturbance goes around the loop at finite speed; it does not "hit" all variables simultaneously.

Now you have stepped out of the loop to trace the time course of the effect of a known external event (the disturbance) on variables in the loop. I still maintain that it makes no sense to speak in terms of past and future for variables within the loop. The distrubance does start a change in one variable in the loop (i, not x) but this is happening while i is being effected by the loop variable, o. The value of i at any instant can still be viewed as both the future result of o and the past cause of o.

But this is getting weird. I ask you what I asked Martin; why do you want to believe that there is a meaningful temporal course of cause and effect in a closed loop? Are you sure that you have completely abandoned the S-R view of behavior?

> you are still fighting the S-R dragon. That dragon doesn't worry me at all--
> he's as good as dead, so far as I'm concerned.

Many people clearly maintain an S-R (really, cause-effect) perspective on behavior while adamently denying their belief in S-R psychology. Cognitive psychologists are a good example.

> This effort to construct a barrier between PCT and conventional psychology is
> in my opinion misguided and self-defeating. As you may have noticed by now,
> I'm more interested in building bridges than motes.

Who has been building a barriers between PCT and conventioal psychology? I've been trying to get conventional psychologists to see behavior from a PCT perspective. It's the conventional psychologists who have been unwilling to see behavior this way; the barrier is on their side, not mine. I have also been trying to build bridges by communicating the basic perspective of PCT and how this relates to conventional psychology (the "Blind Men" paper is one example). The bridges have been built so that the conventional psychologists can come over to the PCT side but the conventional psychologists seem to view the bridges as a means for us to return to their side. Again, that seems more like their problem than mine.

> In PCT, asking questions about what variables might serve as "cues" or
> "predictors" of future events is the same as asking what variables might act
> as disturbances to the controlled perception, and to answer that question,
> you must first know what perception is being controlled.

Once you know what variable is controlled, the answer to the question "what variables act as disturbances to it" is trivial. In fact, psychologists never try to determine the controlled variable because they don't even know what such a thing might be. How many psychological studies do you know of where people have asked what variables might serve as "cues" or "predictors" AFTER they have already determined what variable is under control?

> In classical salivary conditioning, for example, you must identify the
> controlled perception (taste intensity?). After that, you might ask how that
> perception is affected by food-in-mouth, what the reference level is, how
> error is translated into salivation, what effect salivation has on the
> controlled perception. You must then identify how the CS comes to act as a
> disturbance to that system, if indeed that's how it works. You must develop
> a testable model and then put it to test. All of this is pure PCT research;
> no sign of the dragon anywhere

But you are describing research that has never been done! The invisible S-R dragon has seen to that. Or do you know of a lot of research on "classical conditioning" that involved testing for controlled variables?

Me:

> The focus of our interest should be on determining the variables that are
> controlled when we see organisms behaving in various ways.

Bruce:

> That's a beginning. But it's also important to determine how.

But there has been no beginning. There is no (count them, NO) conventional psychological studies aimed at determining the variables controlled when orgaisms behave. Why start to determine how organisms behave when you don't even know what they are doing?

Best Rick

End of CSG-L Digest - 21 Apr 1995 to 22 Apr 1995
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Date: Sun Apr 23, 1995 9:58 pm PST
Subject: CSG-L Digest - 22 Apr 1995 to 23 Apr 1995

There is one message totalling 334 lines in this issue.

Topics of the day:

1. list

Date: Sun, 23 Apr 1995 14:08:04 -0400
From: DForssell@AOL.COM
Subject: list

[From Dag Forssell (950423 1100)]

The AERA presentation 4/20 was a success in the opinion of those who participated. We had an attentive audience of about 35 people. Hugh Petrie introduced us. Bill introduced the phenomenon of control with rubber band demos and a volunteer who worked out very well. There was lively interaction with the audience. Gary illustrated problems with Independent Variable / Dependent Variable with variations on the rubber band demonstration (calling on his sister as volunteer). Ed provided lots of information on the discipline program with stories and results. I commented on "What do we mean by the word THEORY." I expect to announce the availability of a two hour video tape of our conference presentation in about two weeks. (Delay due to my loaning my video camera to my daughter Lisa for a few days).

Here is Bill's prepared paper. Gary notified CSGnet a few days ago that he put this paper on WWW. (Bill's actual talk followed the general principles outlined here).

Following Bill's talk I will attach a comment on the life sciences my daughter Lisa sent me. It is a nice illustration to my talk on "What do we mean by the word THEORY."

Subject: My paper for AERA

From Bill Powers (950416)

The nature of PCT

William T. Powers

In the next twenty minutes, I'm going to try to compress 40 years of work into a brief description of perceptual control theory, or PCT for short. PCT is about a phenomenon that you were not taught in school, that none of the mainstream theories of behavior even mention, that is not in most psychology textbooks. I hope that for this brief time you can listen as if you were scientists from some other universe, seeing a new life-form that behaves in ways you've never seen before. And of course I hope that by the time we finish, you may get the feeling that you've never really seen human behavior before, either.

The best way to talk about a theory is to talk about a phenomenon that needs a theoretical explanation. Fortunately, it's not hard to demonstrate the basic phenomenon behind PCT. I can do it with a very simple piece of equipment, a pair of rubber bands fastened together. And just to assure you that there's nothing up my sleeve, I'd like to invite a member of the audience to help me do the demonstration. [Obtain volunteer].

If the volunteer will take one end of this pair of rubber bands in the hand nearest the blackboard, I will take the other end, so we can stretch the rubber band between us, parallel to the blackboard. I hope you can make out the knot in the center where the rubber bands are joined.

We will hold the rubber bands stretched just in front of the blackboard, with the knot over a mark I have already made. Volunteer, your task is very simple. Just keep that knot exactly over the mark while I move my end of the rubber bands around. Let's practice for a moment.

As you can see, this is an easy task if I don't make my movements too fast or extreme. You can see that when I move my end, there's a tendency to move the knot, but the volunteer moves the other end to counteract what I'm doing so the knot remains in one place, right over the mark.

There is obviously some behavior by the volunteer going on here. You can see the volunteer's hand moving around over the blackboard. Let's get a record of that behavior, which the volunteer can make by holding a piece of chalk against the blackboard with the same hand while we do this some more. [I move my end around a large circle several times, and the volunteer's hand traces several times around a large circle].

Now, how would you describe the volunteer's behavior? If someone had just walked into the room, it would seem that the volunteer has just finished drawing a circle. But stop and remember: what was it that I asked the volunteer to do? Did I ask the volunteer to draw a circle? No, I asked that the knot be kept exactly over the mark on the blackboard.

We can see the behavior of the volunteer, but the behavior we see is not what the volunteer is doing. Volunteer, what were you doing? [Keeping the knot over the mark]. Did you mean to draw a circle? [No].

Just to show that this wasn't an accident, let's do it again. Volunteer, please keep the knot as exactly as you can over the mark. [I move my end of the rubber band slowly around a triangle]. Volunteer, were you still doing what I asked you

to do? [Yes]. Then why did you draw a triangle this time? [Rhetorical question]. Thank you for your help.

I hope you're all having some seriously new thoughts about this thing we call behavior. We've just seen some obvious behavior by a human being who claims that it was not what that person was doing. How can you claim you weren't drawing a circle, we ask, when everyone here saw you do it? I'm sure everyone here is starting to see the pattern, the form of what was going on, but it's hard to put into words because we haven't spent our lives developing a language for talking about this kind of situation. I hope, too, that everyone here is beginning to have a suspicion that situations analogous to what we have seen here may be rather common. It may be that when we watch people behaving, we are not really seeing what they are doing.

We need some language to use in describing this situation. Let's start with the position of the knot relative to the mark. This position is variable; it depends on where the two ends of the rubber bands are. The volunteer acted to keep the position of the knot the same as the position of the mark. There's a word for that kind of process: the word is CONTROL. The volunteer was controlling the position of the knot relative to a particular position. So we can say that the position of the knot relative to the mark is a CONTROLLED VARIABLE.

The means of control is also clear: the volunteer varied the position of one end of the rubber bands as a way of controlling the position of the knot. Note the verbs: the knot is controlled, but the position of the end of the rubber bands is varied. The ACTION of the volunteer is to vary the position of one end of the rubber bands.

My end of the rubber bands also varied its position. With my end in a given position, there was a certain force being applied to the knot. So the position of my end of the rubber bands relative to the knot is a measure of a DISTURBANCE. We have three terms: the DISTURBANCE, the ACTION, and the CONTROLLED VARIABLE.

Using these three terms, we can describe what was going on. The ACTION was always varied so that when its effects were added to the effects of the DISTURBANCE, the result was that the CONTROLLED VARIABLE stayed near some particular state. When the controlled variable stayed in that state, it must have been true that the effect of the ACTION on the knot was always equal and opposite to the effect of the DISTURBANCE on the knot. That, of course, is why when I moved my end in a circle, the volunteer drew a circle, and when I moved my end in a triangle, the volunteer drew a triangle -- both rotated by 180 degrees.

We need one more term: REFERENCE CONDITION. The volunteer was controlling the relationship of the knot to the mark relative to some reference condition, in this case knot-over-mark. But it would have been just as easy to establish some other reference condition, such as knot six inches above the mark, or a foot to the right of it. To say that the volunteer is controlling the relationship of the knot to the mark is to say that this relationship was being maintained close to some particular reference condition.

We can now define control. Control is a process by which a person can maintain some controlled variable near a reference condition by varying actions that oppose the effects of disturbances. That language is now general enough that we can apply it to situations where there are no rubber bands. But there is one more fact we have to establish, which I can do just by asking a question. Do you think

the volunteer could have controlled the position of the knot while wearing a blindfold?

All you have to do is imagine trying it yourself. It's impossible. If you can't perceive the variable, you can't control it. Obviously, perception plays an essential role in this process we call controlling. The more you consider that fact, the more you will come to appreciate why we call this theory not just control theory, but perceptual control theory.

When we see other people behaving, we see their actions, and sometimes we see disturbances to which the people seem to be reacting. It looks rather like stimuli causing responses. But when we look at our own behavior, we see something we can't see in other people's behavior: we see what we are controlling by means of our own actions.

So when we think of human behavior, what we notice depends on whose behavior we're thinking of: theirs, or our own. Our own behavior is seen in terms of perceived outcomes, what our actions accomplish. But other people's behavior is seen in terms of their actions and we know little of what perceptions those actions are supposed to be controlling. PCT gives us a way of understanding behavior that works both for ourselves and for other people, and it shows us that we need to understand something about other people that is not obvious. We need to understand that their behavior is not what they are doing. That simple understanding, and the questions it raises and the answers it leads us to seek, can greatly change the way we understand human nature.

My time is almost up, and I've just skimmed the surface of this subject. I haven't yet got to PCT. PCT is a theory of behavior, a model of how a human being must be internally organized to accomplish this process called controlling. It is a technical theory that involves neurology and physiology and mathematical theories of control systems developed some 60 years ago by engineers. I won't get into that here. What I hope has been accomplished in this short introduction is to bring to your attention a neglected phenomenon, the phenomenon of control. Once you have an orderly way to think about it, in terms of actions, disturbances, controlled variables, and reference conditions, you can start seeing it in every aspect of human behavior.

It isn't necessary to understand the technical side, the theory itself, to appreciate that there is a phenomenon here and that it needs an explanation. Nor is it hard to see that the mainstream theories going around today are inadequate to the job; they don't even recognize that this phenomenon exists. So even if you're hearing about this for the first time, and feeling overwhelmed by the implications and by your own ignorance of how to tackle this huge new scientific problem, you can at least be gratified to know you understand something about a new direction in psychology of which most psychologists know nothing at all.

More to the point, you will be happy to know that we up here at the podium don't really know a great deal more about this new subject than you do. We are very much feeling our way into new territory and wondering where it will lead. We haven't yet reached the time when the vast resources of mainstream science are brought to bear on this new approach; only the youngest of you here will see that day. All we can do is show you what we have found, and describe some applications that look very promising, and hope that you will join the effort by pondering the phenomenon of control as it shows up in your own work. We haven't yet reached the

point in the maturation of a science where we are jealous of others who beat us at our own game. We will be grateful for your company.

Durango, CO
April, 1995

Subject: college funny

DAVE BARRY ON COLLEGE

****
Many of you young persons out there are seriously thinking about going to college. College is basically a bunch of rooms where you sit for roughly two thousand hours and try to memorize things. The two thousand hours are spread out over four years; you spend the rest of the time sleeping and trying to get dates.

Basically, you learn two kinds of things in college:

1. **Things you will need to know in later life (two hours).**

2. **Things you will not need to know in later life (1,998 hours). These are the things you learn in classes whose names end in -ology, -osophy, -istry, -ics, and so on. The idea is, you memorize these things, then write them down in little exam books, then forget them. If you fail to forget them, you become a professor and have to stay in college for the rest of your life.**

It's very difficult to forget everything. For example, when I was in college, I had to memorize -- don't ask me why -- the names of three metaphysical poets other than John Donne. I have managed to forget one of them, but I still remember that the other two were named Vaughan and Crashaw. Sometimes, when I'm trying to remember something important like whether my wife told me to get tuna packed in oil or tuna packed in water, Vaughan and Crashaw just pop up in my mind, right there in the supermarket. It's a terrible waste of brain cells.

After you've been in college for a year or so, you're supposed to choose a major, which is the subject you intend to memorize and forget the most things about. Here is a very important piece of advice: Be sure to choose a major that does not involve Known Facts and Right Answers. This means you must not major in mathematics, physics, biology, or chemistry, because these subjects involve actual facts. If, for example, you major in mathematics, you're going to wander into class one day and the professor will say: "Define the cosine integer of the quadrant of a rhomboid binary axis, and extrapolate your result to five significant vertices." If you don't come up with exactly the answer the professor has in mind, you fail. The same is true of chemistry: if you write in your exam book that carbon and hydrogen combine to form oak, your professor will flunk you. He wants you to come up with the same answer he and all the other chemists have agreed on. Scientists are extremely snotty about this.

So you should major in subjects like English, philosophy, psychology, and sociology -- subjects in which nobody really understands what anybody else is talking about, and which involve virtually no actual facts. I attended classes in all these subjects, so I'll give you a quick overview of each:

ENGLISH: This involves writing papers about long books you have read little snippets of just before class. Here is a tip on how to get good grades on your English papers: Never say anything about a book that anybody with any common sense would say. For example, suppose you are studying Moby-Dick. Anybody with any common sense would say that Moby-Dick is a big white whale, since the characters in the book refer to it as a big white whale roughly eleven thousand times. So in your paper, you say Moby-Dick is actually the Republic of Ireland. Your professor, who is sick to death of reading papers and never liked Moby-Dick anyway, will think you are enormously creative. If you can regularly come up with lunatic interpretations of simple stories, you should major in English.

PHILOSOPHY: Basically, this involves sitting in a room and deciding there is no such thing as reality and then going to lunch. You should major in philosophy if you plan to take a lot of drugs.

PSYCHOLOGY: This involves talking about rats and dreams. Psychologists are obsessed with rats and dreams. I once spent an entire semester training a rat to punch little buttons in a certain sequence, then training my roommate to do the same thing. The rat learned much faster. My roommate is now a doctor. If you like rats or dreams, and above all if you dream about rats, you should major in psychology.

SOCIOLOGY: For sheer lack of intelligibility, sociology is far and away the number one subject. I sat through hundreds of hours of sociology courses, and read gobs of sociology writing, and I never once heard or read a coherent statement. This is because sociologists want to be considered scientists, so they spend most of their time translating simple, obvious observations into scientific-sounding code. If you plan to major in sociology, you'll have to learn to do the same thing. For example, suppose you have observed that children cry when they fall down. You should write: "Methodological observation of the sociometrical behavior tendencies of prematurated isolates indicates that a casual relationship exists between groundward tropism and lachrimatory, or 'crying,' behavior forms." If you can keep this up for fifty or sixty pages, you will get a large government grant.

Best, Dag

End of CSG-L Digest - 22 Apr 1995 to 23 Apr 1995
****

Date: Mon Apr 24, 1995 10:27 am PST
From: prohugh
EMS: INTERNET / MCI ID: 376-5414
MBX: prohugh@ubvms.cc.buffalo.edu
Subject: THANKS!!!

To all the AERA Participants,

I just wanted to thank you all once again for participating in the AERA session. I think it went quite well and maybe, just maybe, we have a couple of potential converts. I'll keep you posted on anything I hear in feedback.

If anything appears on the net about the conference, could someone please forward it to me? I will not be able to rejoin the net until late May given the pressures of the end of the semester.

Again, thanks, thanks, thanks.

Hugh

++++++++===========***********===========+++++++++++===========

Hugh G. Petrie 716-645-2491
367 Baldy Hall FAX: 716-645-2479
University at Buffalo
Buffalo, NY 14260
USA prohugh@ubvms.cc.buffalo.edu

Date: Mon Apr 24, 1995 6:13 pm PST
From: CZIKO Gary
Subject: PCT

Gwen Stephens:

Please try again but without the period after csg-l. If that doesn't work, let me know (it is better that you subsribe yourself to avoid problems with return addresses).

Hope you find PCT and the CSG of continued interest.--Gary Cziko

> Hi! I attended your AERA presentation, but couldn't attend the evening SIG group. I took Hugh Petrie's course on Perception which was based on PCT at Illinois around 1974. It was the best course I had in grad school, and I'd like to stay in touch now that I've found it again. I'm especially interested in the organizational implications of PCT.

> I tried to use the e-mail address LISTSERV@VMD.CSO.UIUC.EDU, but it came back with some message I couldn't decipher, so I thought I'd try your address with the same message.

> Subscribe CSG-L. Gwen Stephens.

> Thanks very much!

Gary Cziko

Date: Tue Apr 25, 1995 2:58 am PST
Subject: CSG-L Digest - 23 Apr 1995 to 24 Apr 1995

There are 5 messages totalling 482 lines in this issue.

Topics of the day:

1. Words
2. Classical Conditioning Model, v 1.0 (2)
3. Anticipation Summary, Parsing Behavior
4. Bridges

Date: Mon, 24 Apr 1995 10:21:54 +0200
From: Oded Maler <Oded.Maler@IMAG.FR>
Subject: Re: Words

[From Oded Maler (950424)]

>Bill Powers (950416.1045 MDT)

>[Bruce]

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

> Gravitational acceleration can be considered a form of affinity; momentum can be considered a form of impetus; atmospheric pressure forcing air into an evacuated vessel can be considered a form of nature's abhorrence of a vacuum; the chemical combination of oxygen and heated mercury to form mercuric oxide can be considered a form of phlogiston expulsion by heat resulting in the transmutation of pure mercury into a calx; the conic sections followed by orbiting bodies can be considered a form of epicyclic motions on linked spheres; the Lorenz transformation can be considered a form of ether drag; the reception of light by the eye can be considered a form of emitting looking-rays; the blocking of light by an opaque object can be considered a form of shadow-casting. Control of consequences by behavior can be consider a form of control of behavior by consequences.

How about: "the attractors of the dynamical system of neuro-chemical networks can be considered as control of perception"?

I mean to say that the distinction between the "real" and "as if" explanation is not always evident.

--Oded

Date: Mon, 24 Apr 1995 08:40:54 -0700
From: Richard Marken <marken@AEROSPACE.AERO.ORG>
Subject: Classical Conditioning Model, v 1.0

[From Rick Marken (950423.0800)]

Linda and I had a wonderful visit with the Bill and Mary Powers this weekend. The major accomplishment was the translation of my HyperCard classical conditioning program into the Pascal version, which is attached below. This is the state the

program was in at the time Bill decided that he was really on vacation -- at which point we all went off and had a great time in LA LA Land .

The model in the program below is basically the same as the one I built in HyperCard but there are some augmentations (not always for the best, I think) and it still needs some work. But I'm posting it so that Bruce and others who are interested can see the direction of our modelling efforts -- and correct them if necessary.

Here is a quick summary of how the program works:

When you run the program the first thing you see is a graph with five time traces. The cyan trace is the 'CS'; it is an impulse with an exponential fall off. The red trace is the 'US'; it is a step. The yellow trace is called 'Qc'; it is the "liquidity" of the stuff in the mouth (the sum of the salivary output and the current value of the 'US' (food); the blue trace is the controlled perception: Qc+UC. The green trace is the salivary output, 'o'.

In the upper right are five adjustable parameters. The 'CSUS lag' is the time (in seconds) between presentation of the CS and US. 'Gain' is the gain of the control system; 'slow' is the slowing factor; 'csinit' is the amplitude (in arbitrary units) of the CS; 'USinit' is the amplitude (in the same units) of the US. When the program is started, all five parameters are set to values that give "good" results in the sense that 'Qc' is zero when the US is presented; the salivation "anticipates" the US. I am not sure why Bill picked these parameters as the ones that are best for the demo since the system is controlling p, nor Qc. But I'm sure he will comment on this when he gets home (I see that Bill did a lot of revising on the program while I was off posting to the net; you just can't take your eyes off that guy when he's at the computer;-).

You can revise a parameter by placing the ">" next to it (using the arrow keys) and then making the number go up or down using the "+" or "-" key. The range of change in the parameters is limited; most can't be made < 0, for example. To see the effect of a parameter change, just press the space bar and the new result is plotted instrantly.

The model itself is embodied in the following code:

```
  o := o + slow * (-gain * p - o) * dt;
  del[inptr] := o;
  outptr := (inptr + round(olag)) and 1023;
  qc := del[outptr] + us;
  p := cs + qc;
```

The reference for p is implicitly 0 in the output function, which is a leaky integrator. There is an environmental lag (del[inptr]) in the effect of o on the US (food); this is the time it takes for the salivation to do it's thing on the food. The result of the combination of o and US is qc, an envionmental variable that represents the "liquidity" of food in the mouth. The controlled perception is CS + qc.

Best Rick

```
==================
program classical;
```

```
uses dos,crt,graph,grUtils,setparam,frameplt,mouse;

var lag,gain,slow,o,p,us,cs,sum,rms,usinit,
    csinit,time,dt,qc: real;
    n,maxx,maxy: integer;
    usstart: longint;
    ch: char;
    del: array[0..1023] of real;
    inptr,outptr,olag: integer;

param: paramlisttype;
frame: frametype;

procedure setgraphics;
begin
 initgraphics;
 maxx := getmaxx; maxy := getmaxy;
end;

procedure initvars;
begin
 o := 0;
 p := 0;
 us := 0;
 cs := csinit;
 inptr := 0;
 fillchar(del,sizeof(del),#0);
end;

procedure loadparams;  { Set up parameters }
begin
 with param[1] do    {phase}
 begin
  legend := 'CSUS lag';
  kind := 'r';
  rvinit := 2.1;
  rvmin := 0.0;
  rvmax := 200.0;
  rvstep :=0.1;
  rv := @lag;
 end;
 with param[2] do
 begin
  legend := 'Gain';
  kind := 'r';
  rvinit := 50.0000;
  rvmin := 0.0;
  rvmax := 100.0;
  rvstep :=1.0;
  rv := @gain;
 end;
 with param[3] do
 begin
  legend := 'Slow';
```

```
  kind := 'r';
  rvinit := 0.01;
  rvmin := 0.0;
  rvmax := 0.99;
  rvstep := 0.0001;
  rv := @slow;
 end;
with param[4] do
 begin
  legend := 'csinit';
  kind := 'r';
  rvinit := 300.0;
  rvmin := 0.0;
  rvmax := 2000.00;
  rvstep :=1.0;
  rv := @csinit;
 end;
 with param[5] do
 begin
  legend := 'USinit';
  kind := 'r';
  rvinit := 100.0;
  rvmin := 0.0;
  rvmax := 200.0;
  rvstep := 1.0;
  rv := @usinit;
 end;
end;

procedure loadframes;  {Set up plot of variables}
begin
 with frame do
  begin
   numyvars := 5;
   mx := maxx; my := maxy;
   xbase := 70;
   ybase := 20;
   xsize := 500;
   ysize := 380;
   numxgrid := 20;
   numygrid := 18;
   xzero := 0;
   yzero := 180;
   xmax  := 10.0 ;
   ymax[1] := 400.0;
   ymax[2] := 200.0;
   ymax[3] := 150.0;
   ymax[4] := 200.0;
   ymax[5] := 400;
   ylegend[1] := 'CS' ;
   ylegend[2] := 'US' ;
   ylegend[3] := 'Qc' ;
   ylegend[4] := 'p';
   ylegend[5] := 'o';
   xlegend := 'TIME, sec' ;
```

```
    color[1] := lightcyan;
    color[2] := lightred;
    color[3] := yellow;
    color[4] := lightblue;
    color[5] := lightgreen;
    yvar[1] := @cs;
    yvar[2] := @us;
    yvar[3] := @qc;
    yvar[4] := @p;
    yvar[5] := @o;
    xvar := @time ;
   end;
 end;

 begin
   dt := 0.01;
   time := 0.0;
   ch := #0;
   initmouse;
   setgraphics;
   gain := 50.0;
   slow := 0.001;
  loadparams;
  setupparam(400,0,5,param);
  loadframes;
  initframe(frame);
  initvars;
  cs := csinit;
  usstart := round(lag/dt);

    repeat

     usstart := usstart - 1;
     if usstart <= 0 then
     begin
      us := usinit;
      usstart := round(lag/dt);
     end;

     o := o + slow * (-gain * p - o) * dt;
     del[inptr] := o;
     outptr := (inptr + round(olag)) and 1023;
     qc := del[outptr] + us;
     p := cs + qc;
     inptr := (inptr - 1) and 1023;
     cs := cs - cs*dt*1;
     time := time + dt;
     if time > frame.xmax then
     begin
      time := 0.0;
      repeat
       if keypressed then ch := changeparam(param);
      until (ch = 'q') or (ch = ' ') or (ch = #27);
      if ch = ' ' then
      begin
```

```
      cs := csinit;
      us := 0.0;
      usstart := round(lag/dt);
      ch := #0;
     end;
     clrplot(frame);
     initvars;
   end;
    plotvar(frame);
 until (ch = 'q') or (ch = #27);
 closegraph;
end.
```

Date: Mon, 24 Apr 1995 13:12:28 -0700
From: Richard Marken <marken@AEROSPACE.AERO.ORG>
Subject: Anticipation Summary, Parsing Behavior

[From Rick Marken (950424.1300)]

Bruce Abbott (950421.1325 EST) --

> I think we agree that the type of anticipatory action I described above can be accounted for by a two-level model in which the higher-level system responds to a disturbance that is regularly followed in time by a disturbance acting on the lower-level system's controlled perception. A two-level model would work but a one level model (like the classical conditioning model) could probably do the trick too. I think your description of the two-level model of "anticipatory" muscle tensing is fine; I just want to be sure it is clear that the output of a control system, at any level of the hierarchy, depends only on the prevailing disturbances to the controlled variable and the prevailing feedback function relating output to controlled variable. The output, in other words, is busy varying as necessary to keep the perception under control. Under some circumstances these variations may look like "anticipations" but this is purely coincidental.

> I think we agree that the classically conditioned organism can be modeled as follows...After conditioning, the CS produces a disturbance to the same one-level system;

This is correct but I would prefer being a bit more precise; the CS is a disturbance to a perceptual variable that is _controlled_ by the one-level system; it is a disturbance because the organism has learned to control a perception that includes the CS as one of its components.

> A third example of anticipation surfaced in Rick Marken's "anticipation" model, which used both the current state of the perceptual variable and its first derivative to improve control of a "sluggish" control system. Currently there is disagreement as to whether this system does or does not behave in an "anticipatory" fashion.

Yes. I agree. I am arguing against the "anticipation" view because I think that control is always just control of perception. The variable that is controlled by a control system can include derivatives, integrals, convolutions and all kinds

of other functions that make the present time value of the perceptual representation of that variable actually represent a time "window" of variations. The width of the time "window" of the perceptual function will have an impact on the dynamics of the control loop. But calling any of these impacts "anticipation" just directs attention away from the fact that some present time perceptual variable (perhaps one defined over a long time window) is under control.

> If anyone has a different opinion of the current state of these issues, you are welcome to offer it.

An excellent summary! I had a few nits but I think you gave a very fair decription of where we are so far.

CHUCK TUCKER (950421) --

> The notion of anticipation and predictive sensing clearly depend upon when one arbitrarily begins an act...let's parse the act first so we have a notion of what action we have under analysis.

I think that "parsing" behavior is rather arbitrary if we look at behavior as output (as Dewey apparently did). In PCT, behavior is not a "stream of activity"; it is a collection of controlled inputs. The correct parsing of behavior (in PCT) means finding the variables that people are controlling. Parsing the behavior is a natural consequence of doing The Test for controlled variables; the perception controlled defines the relevant boundaries of any behavior. These boundaries will not be temporal;they will be perceptual. As long as a a perception is under control it is a behavioral "segment", even if (because of changing circumstances) there is currently no activity being done to control that perception.

For example, when I drive I control (among other things) the location of the gear shift; but once I get into a gear I am not necessarily done with the "shifting" behavior; that is, I am not necessarily done controlling gear shift position. Dewey might parse the stream of my gear shifting activity into shifts followed by other activities (eg. pressing on the gas, looking at the tach, etc) but The Test might shows that I am controlling gear shift position all the time that I am in the car; the reference for this position is changing (as I go to first, second, third, etc, as necessary) but the behavior (control of gear shift position) is happening when I'm shifting AND when I'm not; one behavioral "segment" is control of perception of gear shift position.

Best Rick

Date: Mon, 24 Apr 1995 15:34:09 -0500
From: Bruce Abbott <abbott@CVAX.IPFW.INDIANA.EDU>
Subject: Re: Classical Conditioning Model, v 1.0

[From Bruce Abbott (950424.1530 EST)]

> Rick Marken (950423.0800) -- Linda and I had a wonderful visit with the Bill and Mary Powers this weekend. The major accomplishment was the translation of my HyperCard classical conditioning program into the Pascal version, which is attached below.

Hey, no fair having the teacher help you with your homework! (Kidding aside, I hope you received a good tutorial on Turbo Pascal programming from Bill, and will be posting more like this one in the future.)

I, too, was busy over the weekend and got my own version up and running, although I am not quite ready to post it. As the model has developed, I've had some changes of mind which I need to address.

My simulation models eyeblink rather than salivary conditioning, and therein lies some of the problems I have to deal with. I envisioned the perceptual variable as the intensity of sensory stimulation of the cornea. A puff of air on the cornea raises that intensity. Because I wanted to stick with a one-level model, I used a counter in the output function, which would trigger an eyeblink when the count reached a threshold value; by changing the threshold you can alter the delay between the beginning of stimulation and the onset of the blink.

The blink itself is modeled as an exponential rise of Vo to a maximum value, followed by an exponential decay. Eye closure becomes complete prior to Vo reaching its maximum value, so the eyeblink itself looks like a clipped version of Vo. By manipulating the rise, decay, and clip constants you can produce an eyeblink with desired characteristics.

As with your model, the perceptual variable is a combination of the CS perception and the current level of the "main" controlled variable (yours is "liquidity," mine is "corneal sensation intensity" or something like that), the resulting perceptual variable is compared to a reference to produce an error, which is multiplied by the system gain and fed to a leaky integrator, whose output is summed to trigger the blink. One difference is that my model currently does not use an exponentially decaying CS perceptual effect; instead I've just cut off the CS when the US starts. The intensity of the US (USint) is multiplied by the proportion of corona exposed to the puff to get the effect of the US on the cornea sensitivity variable.

Although the model performs as expected, I'm not really happy with it in its present form. For one thing, I plan to replace the current output function with a lower-level eyelid muscle control system to handle the eyeblink. I think I can handle the rapid close-open character of the eyeblink as a natural effect of the tear-wiping effect of the blink, which would be expected to rapidly reduce the corneal sensation intensity. With a parameter for tear evaporation the thing ought to blink spontaneously. I also need to model the perceptual input a little better to give appropriate rise and decay times for the sensations; I presently have exponential rise and decay built into the CS and US themselves, but there needs to be something similar done with the effect of the air puff on coronal sensation intensity, and perhaps some lags built in as you have in your model.

After getting the current model running, I decided to check its performance against whatever published results I had handy and was surprised to discover among the secondary sources in my office some rather unexpected data. It appears that the performance of my current model, using its current parameters, more resembles human _voluntary_ responses to the CS than it does the conditioned responses (although I can get something more like the latter by changing paramaters). I'm going to have to get the primary sources from interlibrary loan before drawing any further conclusions, but the graphs I'm looking at for the CR appear to show that the CR eyeblink tends to be partial and pretty much over before the airpuff occurs. If this is true, the CR would not act to shield the

eye from the puff, although it might help somewhat by moistening at least a part of the cornea.

Regards, Bruce

Date: Mon, 24 Apr 1995 19:24:03 -0500
From: kurtzer@UTDALLAS.EDU
Subject: Re: Bridges

B.Abbbot wrote:

> you are still fighting the S-R dragon. That dragon doesn't worry me at all--he's as good as dead, so far as I'm concerned. ...

> In PCT, asking questions about what variables might serve as "cues" or "predictors" of future events is the same as asking what variables might act as disturbances to the controlled perception, and to answer that question, you must first know what perception is being controlled.

but once one found the controlled variable the possible disturbances would not reveal much about the phenoma of concern to any fuctioning organism--namely control. the search for disturbances is equivilant to the search for stimuli. the search is directed to a Baconian compendium of "semi-relevant" facts cataloging all possible ditubances to a controlled variable andthis list is conceivably bordering on the infinite while the number of controlled variables may be intimidating but it is certainly bounded.

i.

End of CSG-L Digest - 23 Apr 1995 to 24 Apr 1995
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Date: Wed Apr 26, 1995 12:39 am PST
Subject: CSG-L Digest - 24 Apr 1995 to 25 Apr 1995

There are 8 messages totalling 701 lines in this issue.

Topics of the day:

1. Hugh Petrie at AERA
2. Bridges (2)
3. Obviousness (2)
4. Simultaneity and anticipation
5. Disturbing Words
6. How Can Words Disturb?

Date: Tue, 25 Apr 1995 02:43:45 -0400
From: DForssell@AOL.COM

Subject: Hugh Petrie at AERA

From Hugh Petrie (950424) Posted by Dag.

Dag, I am not on the net at the moment and probably won't have an opportunity to rejoin until the end of May. Too much to do here. I have appended the material from which I drew my remarks. Basically, it is the proposal for the session. If you want to repost it to the net, that would be fine.

======================

Perceptual Control Theory: A Post-Cognitive Theory of Behavior-
A Demonstration and Workshop/Discussion Strand

The Perspective of Perceptual Control Theory: In the past several decades, an interdisciplinary group of researchers (Powers, 1973, 1989, 1992; Marken, 1992; Petrie, 1981; Cziko, 1992a; Ford, 1989, 1994; Forssell, 1993; McPhail, 1990; Robertson & Powers, 1990) has emerged. They are propounding and applying a new perspective-perceptual control theory (PCT)-to our basic understanding of human behavior. PCT is the product of a long period of exploration and development that follows a line of thought suggested early in the history of cybernetics but leads into quite different territory. At its core is a belated recognition that what people learn to do is not to respond to stimuli or to plan actions and then execute them, but to act on their environments to control what happens to themselves. Human beings, even young ones within their capacities, are active agents, purposive systems with goals and hierarchies of goals. Their actions are not simply push-button responses to stimuli, nor emitted blindly according to precalculated formulae. Instead each action has a purpose, a goal, which is defined in and by the actor and in terms of the actor's perceptions of the world. PCT shows how to recognize control and lack of control in an individual's behavior, how to put oneself in the position of a person trying to learn to control new aspects of the environment, and how to avoid the clashes that always threaten when independent, actively controlling organisms, both adults and children, share the same environment.

Two main conceptions of human behavior, stimulus-response theory and cognitive theory, have traditionally guided educational research and practice. The S-R approach has focused on eliciting the production of appropriate responses through drill, reward, and sometimes punishment. The cognitive approach has focused on teaching the logical organization of ideas and facts so that they can be comprehended at various stages of learning and will generate the appropriate responses. Perceptual Control Theory goes beyond both behaviorist and cognitive theories in accounting for the fact that people can accomplish the same end in an indefinite number of varying circumstances and contexts. PCT theorists have elaborated an underlying generative model of such purposeful behavior that has resulted in predictions which correlate 0.95 and above with actual human behavior in the tasks studied thus far (e.g., Marken, 1990).

Despite its elegance and simplicity, the PCT model is initially difficult to grasp precisely because it turns on its head our common sense and common research wisdom about how to understand behavior. William T. Powers, the major figure in perceptual control theory, captures this new way of looking at behavior in the title of his seminal work, Behavior: the Control of Perception (1973). Instead of viewing behavior as the outcome of stimuli or perceptions (as modified by cognition, emotions, or planning), PCT views behavior as the means by which a perceived state of affairs is brought to and maintained at a (frequently varying) reference or goal state. Traditional theories require the modeling of behavior as planned and computed output, an approach that requires levels of precise calculation that are unrealistic in a physical system and impossible in a real environment that is changing from one moment to the next. PCT, however, provides a physically plausible explanation both for the consistency of outcomes and the variability of means human beings actually employ to reach those outcomes in a constantly changing environment.

The exchange in Educational Researcher between Cziko (1992a, 1992b) and Amundson, Serlin, & Lehrer (1992) illustrates the difficulty of understanding PCT in its own terms. The medium of language allows for significant misinterpretation and talking past one another. Clearly, the Educational Researcher exchange never engages perceptual control theory on its merits. Fortunately, however, researchers in PCT have developed over the years striking demonstrations of the phenomenon of perceptual control and simulations of control systems that are able to keep a sensed variable at a (possibly changing) reference state despite a wide range of external variations and disturbances. These demonstrations, most of them interactive, provide dramatic examples of behavioral phenomena that are extremely difficult or impossible to explain using traditional theories. They are, in the theory of paradigm shifts, truly anomalies (Kuhn, 1970) for behaviorist and cognitive theories but are explicable as a matter of course in perceptual control theory.

Objectives: We will explain and demonstrate a new theory of human behavior in which theory and practice merge, where the principles of the theory are seen at work and used every day in the classroom. The theory is based on universals of human nature that apply across cultural, class, and age boundaries, applying to the challenged and the ordinary as well as to the gifted, and which can be tested and refined even as they are being used to guide events in the classroom, in families, in social service agencies, and in health care organizations.

The objectives of this two-session strand are, first, to present a wide variety of vivid illustrations of PCT; second, to allow the audience to interact with these demonstrations for themselves so that they can get a real feel of the phenomenon of control and how PCT approaches its explanation; and, third, to begin relating these demonstrations to traditional educational research issues such as learning, instruction, motivation, assessment, management and

organization, and school change.

Educational Importance: As with any truly revolutionary theory, a wide range of common phenomena are seen in a new light and a deeper understanding and a range of new phenomena are uncovered.

Perceptual control theory is about human nature and its basic organization. It provides an understanding of principles rather than lists of actions to take under specific circumstances. Teachers who have learned to use these principles find that they are finally true professionals because they know what to do without having to be told, because they understand what is happening. Students experience less conflict among themselves and with their teachers. They are less distracted from learning. Teaching itself, done with an understanding of the learning process as it is experienced and demonstrated, becomes less stressful because conflicts are recognized and dealt with before they escalate.

Teachers come to see that what is learned is neither a set of mechanical responses to stimuli nor a collection of abstract reasoning processes isolated from the real world. Students, and teachers too, learn how human beings perceive, compare, and act, all at the same time, and all in order to increase their control over their own lives. Perceptual control theory deals with the classroom at the level at which we ask ourselves "What on earth are these students up to? Why am I always in conflict with them? What are they learning when they fail to learn what I am trying to teach? How can I find out whether a student is learning or not, and, if not, how can I find out what is wrong?"

The graduates of an educational system organized around the principles of PCT will be neither animals capable of doing clever tricks when systematically rewarded, and otherwise devoid of initiative, nor disembodied intelligences stuffed with facts and incapable of acting without a complete prediction of the future. They will be real human beings with skills and understandings that work together with the world as it is, and with respect for other human beings as equally autonomous agents.

For example, in perceptual control theory an explicit model is available to account for much of the currently metaphorical language on the "construction of meaning." New ways of looking at motivation as essentially intrinsic are suggested. The roles of students, teachers, administrators, and parents as autonomous actors in the educational system are revealed. The near impossibility of "making" people learn or teach or administer or parent in certain pre-specified ways becomes apparent. Strategies for helping the most difficult of students to learn can be derived, at least in broad outline. Common current critiques of standardized forms of assessment are given a deeper underpinning. The centrality of perceptual and experiential learning, along with the necessity for risk-taking and experimentation in the educational process, are straight-forward results of a PCT perspective.

Moreover, PCT provides insights into a wide variety of historical and social phenomena. In particular, events, such as the civil rights movement, which involved individuals who strove against great odds and many obstacles to accomplish important personal goals and make valuable contributions to society are seen as straightforward outcomes of autonomous agents controlling their higher level perceptions. PCT may also serve as a potential antidote to the environmental fatalism that seems so rampant in many of our inner-city schools. A PCT perspective opens up new ways of thinking about how people can find ways of taking control of their lives. New light is shed on issues of diversity and tolerance, both providing a basis for understanding how diversity arises and demonstrating the absolute centrality of tolerance if we are to avoid destructive conflicts.

Session Structures: There will be two sessions in the strand-a demonstration session and a workshop/discussion session, each of two hour's duration.

The demonstration session will consist of a variety of demonstrations of PCT phenomena and theory using everything from rubber band experiments to computer simulations of different individual human behaviors and social phenomena. Some of the demonstrations will be interactive with the audience, although the major "hands-on" phase of the strand for the audience will occur during the second, workshop/discussion session (see below). In the demonstration session, the presentations will illuminate such key educational concepts as learning, instruction, motivation, assessment, school reform, organization, and leadership. However, the major forum for the discussion of PCT and education will occur during the discussion phase of the second session.

The second, workshop/discussion session will engage the audience hands-on activities and discussion of the relationship of PCT to educational issues. The audience will experience the phenomenon of control for themselves, explore hierarchies of control, have their own performance predicted (with 0.95 accuracy) by the model, and experience cooperation and conflict with other control systems. The presenters will be available to answer questions and to explore with the audience further, more complex, issues and research topics suggested by the demonstrations and hands-on activities.

Presenters and Topics:

William T. Powers, Control Systems Group, "Fundamentals of Perceptual Control Theory"

Gary Cziko, Educational Psychology, University of Illinois at Urbana-Champaign, "A New Paradigm for Educational Research"

Edward E. Ford, Ed Ford and Associates, Scottsdale, Arizona, "Using Perceptual Control Theory With Students, Parents, and The Entire School Staff To Implement A School Discipline Program"

Dag Forssell, Purposeful Leadership, "What do we mean by THEORY"

Hugh G. Petrie (Chair), Dean, Graduate School of Education, University at Buffalo, "PCT, Standards, and Assessments"

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Date: Tue, 25 Apr 1995 08:26:56 -0700
From: Richard Marken <marken@AEROSPACE.AERO.ORG>
Subject: Re: Bridges

[From Rick Marken (950425.0830)]

i. kurtzer (950424) --

> the search for disturbances is equivilant to the search for stimuli. the search is directed to a Baconian compendium of "semi-relevant" facts cataloging all possible ditubances to a controlled variable and this list is conceivably bordering on the infinite while the number of controlled variables may be intimidating but it is certainly bounded.

Excellent point, Isaac! Once we know what variable is being controlled we know all the "stimuli" (distrubances) that will influence behavior. For example, once we know that the knot is under control in the rubber band experiment we know all the "stimuli" that will influence rubber band pulling behavior; anything that influences the knot influences behavior. When we don't know what variable is under control (or THAT a variable is under control) then the search for variables that influence behavior, which is the main goal of IV-DV research in psychology, is (to use Isaac's felicitous phrase) "directed to a Baconian compendium of "semi-relevant" facts" or (to use my more blue-collar language) "a damn waste of time and money".

We are trying to build bridges to people who believe in IV-DV research; people who believe that the way to study rubber band pulling (for example) is to manipulate variables that seem to have an effect on this behavior. IV-DV experiments will reveal that moving the table, pulling on the knot, pulling on the other end of the rubber band, etc all have a statistically significant effect on rubber band pulling behavior. However, they won't reveal the variable that is under control; the variable that is disturbed by all these "stimuli". IV-DV research won't reveal the fact that the subject in the rubber band experiment is controlling the position of the knot.

How do you politely tell people that their carefully devised methods for studying behavior have made it possible for them to completely miss the central fact of behavior: controlled variables? I think the only thing to do is keep pointing out examples of controlled variables and hope that they eventually catch on.

Best Rick

PS. Isaac. I will try to answer your modelling questions eventually but some of your posts are coming in MIME code which I can't read.

Date: Tue, 25 Apr 1995 12:42:03 -0400
From: DForssell@AOL.COM
Subject: Obviousness

[From Dag Forssell (950425 0930)]

Musings on obviosness.

It seems obvious that the PCTtexts disk and WWW file with 2.6 MB of PCT discussion is worthless. Not a pipsqueak from any CSGnetter. How about some feedback? Dennis McCracken: Did you get it?

PCT is obvious in the same way it is obvious that the Earth spins on its axis and rotates around the Sun: Not at all!

It took genius to suggest the mechanism of a Sun-centered solar system to explain the phenomena we see in the heavens. Once you have had it explained to you, you can see much evidence for it. The old explanations (of planets and the Sun circling the Earth) became obsolete.

It has also taken genius to suggest the mechanism of perceptual control to explain the phenomena we observe in the behavior of ourselves and others. Once you have had it explained to you, you can see much evidence for it. The old expalanations (of stimulus, response, conditioning etc.) become obsolete.

Was it obvious to an observer in the 1500's that the Earth spins on its axis and revolves around the Sun?

Is it obvious to an observer in the 1990's that every single organism in the world controls its perceptions and that this explains all behavior?

Best, Dag

Date: Tue, 25 Apr 1995 11:27:10 EDT
From: mmt@BEN.DCIEM.DND.CA
Subject: Re: Bridges

[Martin Taylor 950425 10:45]
kurtzer undated (25 Apr 95 04:21:29 EDT in the header)
+Rick Marken (950424.1300)

Answering Bruce Abbott, Isaac Kurtzer wrote:

but once one found the controlled variable the possible disturbances would not reveal much about the phenoma of concern to any fuctioning organism--namely control. the search for disturbances is equivilant to the search for stimuli.

the search is directed to a Baconian compendium of "semi-relevant" facts cataloging all possible ditubances to a controlled variable andthis list is conceivably bordering on the infinite while the number of controlled variables may be intimidating but it is certainly bounded.

This comment seems to go to the heart of the "anticipation" debate. A control hierarchy, or even a set of parallel ECUs consists of a whole lot of control loops, within each one of which there is exactly one perceptual input function, and that PIF defines exactly one Complex Environmental Variable. No changes in the environment of any kind affect an isolated control loop, except for changes in that CEV. As Isaac says, the causes of these changes are unbounded in possible number, and in any case are irrelevant to the actions of the control loop (except insofar as they may be too fast and/or furious for the control loop to deal with).

The CEV defined by the PIF is the only aspect of the external world reflected in the action of the loop. If the PIF is extended in time, we have what Rick Marken says:

> The variable that is controlled by a control system can include derivatives, integrals, convolutions and all kinds of other functions that make the present time value of the perceptual representation of that variable actually represent a time "window" of variations. The width of the time "window" of the perceptual function will have an impact on the dynamics of the control loop. But calling any of these impacts "anticipation" just directs attention away from the fact that some present time perceptual variable (perhaps one defined over a long time window) is under control.

A control system that acts like this cannot "see" or perform any anticipation or prediction, as Rick has been saying all along.

But the control hierarchy contains more than one elementary control unit, and it may be that control unit A has a CEV(A), while control unit B has a CEV(B) that is created by a PIF(B) that has many of the same input units as PIF(A), plus inputs from a major cause of disturbance to A. If the the input seen by B from the CAUSE of disturbance to A is often followed by some delayed effect on CEV(A), then the output of B could influence the reference of A in such a way that the output of A occurs before the disturbing influence on CEV(A).

B does not "see" all the disturbing influences on CEV(A), and sometimes its effect on the output of A may be counter-productive. But on balance, reorganization will eventually work to make such connections have beneficial effects on control more often than not.

This is the two-level hierarchy originally introduced by Rick, and taken up by Bruce. It is, as Isaac says, a view of a "stimulus" to A, but at the same time, it involves perfectly ordinary (non-anticipatory) control by B, as Rick says.

There is a third possibility, and this is the one that I think causes some heartburn. That is the possibility that there is a predictive computation within control loop A itself. Any such prediction HAS to be based strictly on temporal patterns in the values of CEV(A), NOT on the disturbances themselves, as in the Artificial Cerebellum, which produces temporally structured output waveforms from impulse or step changes in the error signal. That temporal structure seems legitimately to warrant the label "anticipation" within the single control loop

A. The Perceptual Input Function may not be extended in time, but the output function is. Indeed, a simple integral output function can be seen as implementing the "prediction" that the disturbance has some coherence over time, so that longer observation provides a more precise determination of the state of the CEV, thus warranting higher effective gain.

If there is "anticipation" or "prediction" in a simple control loop, it is in the output function, not in the perceptual function. But I suspect that more normally, what we see from outside as "anticipation" is either two-level control or control of a perceptual function extended in time.

Rick to:

CHUCK TUCKER (950421) --

> The notion of anticipation and predictive sensing clearly depend upon when one arbitrarily begins an act...let's parse the act first so we have a notion of what action we have under analysis.

> I think that "parsing" behavior is rather arbitrary if we look at behavior as output (as Dewey apparently did). In PCT, behavior is not a "stream of activity"; it is a collection of controlled inputs. The correct parsing of behavior (in PCT) means finding the variables that people are controlling. Parsing the behavior is a natural consequence of doing The Test for controlled variables; the perception controlled defines the relevant boundaries of any behavior. These boundaries will not be temporal;they will be perceptual.

Once again, I agree with Rick (contrary, I suppose, to anticipations of some). When Chuck posted his comment, I did not see what it had to do with anticipation, since everything in the various parses had to do with simple control of some higher-level perception. But if you think of sequence control as representing some kind of anticipation (that the reference sequence will in fact produce perceptions aiding the higher level control), then the parsing is simply a stage in developing the Test for whether there is such a sequence being controlled. As Rick says, the boundaries must be perceptual, since there is temporal overlap even among the controlled perceptions in a sequence.

I had saved up several messages on anticipation, for comment. But the latest interchange between Rick and Bruce seems to make further comment unnecessary.

Are we back in Wonderland yet? Or are we still Through the Looking Glass?

Martin

Date: Tue, 25 Apr 1995 19:16:55 -0400
From: bleach@BIX.COM
Subject: Re: Simultaneity and anticipation

<[Bill Leach 950421.19:50 U.S. Eastern Time Zone]
[Martin Taylor 950420 11:00]

Martin, welcome back.

I am not going to try to get back into the middle of this one again but rather felt it would be useful to restate something that you said in the manner in which I believe that you meant it as opposed to the way that it can be taken to mean...

You said:

> One can and must think in circles, but signals take time to go round and round the circle, even though the whole circle is there all the time.

and I think that you quite specifically mean:

One can and must think in circles, but the effects caused by any change to any signal anywhere in the loop (for any reason) takes time to go round and round the circle, even though the whole circle is there all the time.

Maybe I'm "anticipating" but I am just about certain that you did not mean for your statement to almost sound like a token ring type operation (if for no other reason, one almost has to come to that conclusion based upon your other related statements).

-bill

Date: Tue, 25 Apr 1995 19:23:37 EDT
From: mmt@BEN.DCIEM.DND.CA
Subject: Re: Obviousness

[Martin Taylor 950425 19:10] Dag Forssell (950425 0930)

Musings on obviosness.

> It has also taken genius to suggest the mechanism of perceptual control to explain the phenomena we observe in the behavior of ourselves and others. Once you have had it explained to you, you can see much evidence for it.

Yes, precisely. It takes cleverness to elaborate and to describe things in a way that works and with which people can agree because it extends what they "knew." It takes genius to take an elaborated and agreed scheme and replace it with something simple that works a lot better and can be understood only if you change the basis of your thinking.

I have said here and in many other places: I think Bill Power's eventual effect on psychology will be akin to that of Newton on physics.

Which doesn't stop me from arguing with him on points of detail!

> It seems obvious that the PCTtexts disk and WWW file with 2.6 MB of PCT discussion is worthless.

I have my own 30+ Mbytes of PCT discussion, so I haven't looked at your collection. But what I have been doing with Allan Randall as a contractor, is using my set as a base corpus for an attempt to devise a method of automatic

hypertexting, that would allow people interested in a theme to find it through the variegated discussions, regardless of "Subject:" lines. No success as yet, but some interesting possibilities--the automatic analysis clusters as very similar the following words (note the bracketing): (((Bill Rick) Martin) Powers). Perhaps I should add another bracket :-)

We have run out of money for that project, but I have hopes of getting more from another source. If a tool comes of it, I hope that the tool would be made available for archive browsing. But it may become proprietary, if it ever exists, so I can't guarantee anything.

Martin

Date: Tue, 25 Apr 1995 19:18:41 -0700
From: Richard Marken <marken@AEROSPACE.AERO.ORG
Subject: Disturbing Words

[From Rick Marken (950425.1915)]

What we say makes a difference. For the last several years there has been a continuing din of hateful speech in this country. The _enemy_ has been "liberals", "immigrants", "welfare mothers", "government", etc. The bombing in Oklahoma has shown that "government" is not just faceless "ATF agents"; it's individual human beings: men, women and children.

Nobody questions the "right" of people to say almost anything they want in the media; I am all for free speech myself. But I want to join with President Clinton in his plea for a voluntary cessation of the hateful speech that passes for political dialog in this country. The fact that most of this comes from the political "right" is irrelevant; wherever it comes from, it is not a good approach to solving control problems.

I think that I myself may have been guilty of a kind of hateful speech on this net and I want to apologize for it now. Whenever a category of people (like "conventional psychologists") is referred to as "idiots", "the problem" or "the enemy" it is hateful speech. I have been guilty of hateful speech and I am profoundly sorry for it. Oklahoma will always remind me of the human consequences of hateful speech.

It would be nice if some of the people who have exercised their "right" to hateful speech in the media would not only change their ways but would also apologize publicly for their past contribution to the tone of the political dialog in the country. If they did, I think it would go a long way toward lessening the hatred that exists in many of the people who admire them. It would also mean that the children who died in Oklahoma will not have died in vain but, rather, for the most precious gift of all; peace.

Here's a prayer for a future of peaceful political debate; we can get there if we can get beyond our hate. And we can get beyond our hate if can go up a level and see our hate as something we are doing; something we are controlling for. If we can see what we are controlling for we can stop controlling for it

Best Rick

Date: Wed, 26 Apr 1995 03:24:21 -0600
From: CZIKO Gary <g-cziko@UIUC.EDU
Subject: How Can Words Disturb?

[from Gary Cziko 950426.0305 GMT}

Rick Marken (950425.1915) wrote:

> What we say makes a difference. For the last several years there has been a continuing din of hateful speech in this country. The _enemy_ has been "liberals", "immigrants", "welfare mothers", "government", etc. The bombing in Oklahoma has shown that "government" is not just faceless "ATF agents"; it's individual human beings: men, women and children.

Rick, I don't think I like hateful speech anymore than you do and I was moved by the sentiment evoked by your words.

But it also got me thinking of how it is that "words can make a difference." If certain words "disturb," then we have control systems to compensate for the disturbances. When I hear hateful words, I am not moved to join in the hate, but rather I become suspicious of the judgement and motives of the producers of the hateful words. But you must feel that hateful words can cause at least some of the people who hear the words to hate, tool, or else you probably wouldn't be concerned about the hateful words. But how does the suspected effect of hateful words mesh with PCT? How can words cause one to hate or do anything if hateful reference levels are not already there? This looks like an input-output (S-R) view of behavior to me.

This question also came up in my mind when preparing for my talk at the American Educatinal Research Association at which Hugh Petrie, Bill Powers, Ed Ford, and Dag Forssell participated. I argued against an independent variable - dependent variable approach to educational research, but then Ed Ford shows some dramatic reductions in school violence and discipline problems in a school that uses his "responsible thinking" program. But this looks like the old-fashioned IV (responsible thinking) - DV (reduced discipline problems) approach to educational research.

Perhaps the answer is that IV-DV doesn't work for well established control systems, but that IV - DV can be used to understand reorganization.

Any thoughts that anyone wishes to share concerning my conundrum would be appreciated.--Gary

End of CSG-L Digest - 24 Apr 1995 to 25 Apr 1995
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Date: Thu Apr 27, 1995 3:15 am PST
Subject: CSG-L Digest - 25 Apr 1995 to 26 Apr 1995

There are 12 messages totalling 476 lines in this issue.

Topics of the day:

Date: Wed, 26 Apr 1995 01:11:52 -0400
From: MLazare910@AOL.COM
Subject: CSGnet help

This is Mark Lazare and I'm trying to get to the net thru AOL.

My Screen name is MLazare910. SOMEONE LET ME KNOW IF I MADE IT
M A Lazare

Date: Wed, 26 Apr 1995 05:27:00 EST
From: Hortideas Publishing <0004972767@MCIMAIL.COM>
Subject: Rick's revelation about hateful words

[From Greg Williams (950426)]

Congratulations, Rick, on realizing the potential influence of name-calling and other "hateful" language. Hard as it has been, I've vowed to stay off the net ever since I decided that my presence contributed to the outpouring of hateful words in your posts. Now that you've seen the light, I'm tempted to return -- but I think I'll wait a bit to see how thorough your recantation actually is. ;-}

No, I don't try to phone the Rush Limbaugh Show, either.

As ever, Greg

Date: Wed, 26 Apr 1995 07:42:40 .
From: "Joel B. Judd"
Subject: Disturbing Words

SUBJECT: Disturbing Words

{from Joel Judd 950426.0730 CST}

Gary (950426):

I'm not sure I understand what you're asking about the effects of words. Are you suggesting that words are stronger evidence for an IV -> DV view of behavior than a red rose or a bullwhip? Human beings are pretty much language-based for communication, but it seems that it's the "post-linguistic" perceptual levels that run the show. If you control for "Freedom of Speech" regardless of the speech, then particular words won't be a significant disturbance, will they? On the other hand, if you're functioning under a "Government's a Conspiracy" POV, then warmongering and similar sentiments confirm your perceptions.

In either case, the language stems from and contributes to our Systems Level perceptions.

Date: Wed, 26 Apr 1995 12:58:47 -0600
From: CZIKO Gary <g-cziko@UIUC.EDU>
Subject: LISTSERV Mail Lost

[from Gary Cziko 950426.1250 GMT]

From approximately April 19 through April 23 about 100 messages were received at my account cziko@vmd.cso.uiuc.edu and due to factors beyond my control these messages were all lost.

I do NOT use this account for my personal mail, but use it to receive messages generated by LISTSERV. I suspect that almost all these messages were notifications that CSGnet mail did not get through to the intended recipients, with other messages informing me of who has left and joined CSGnet.

So if you recently subscribed to CSGnet adn did not receive an intro document, let me know and I will send you one. If you sent me personal mail to this address during this time period, you will have to send it again.

I regret any inconvenience this may have caused.--Gary

Date: Wed, 26 Apr 1995 10:25:18 -0400
From: MILLERD@DAYTON.BITNET
Subject: Re: How Can Words Disturb?

[Dan Miller (950426)]

Gary Cziko and Rick Marken,

Rick,

Your ideas about hate speech were much appreciated. Over the past few years I have had the misfortune of driving across country to see to an ailing parent. During these trips I have listened to the radio (my tape player is fried). I hadn't listened for some time, and to my amazement I was treated to some of the most frightening talk I had heard since overheard some Iowa vigilantes talking about teaching a lesson to the commie peaceniks down at the university.

Rush Limbaugh is a pussycat compared to Gordon Liddy (Isn't he a convicted felon?) and a couple of others I heard. If the sentiment they espoused is widespread, then we are in for some "interesting" times. Interspersed throughout their interpretation of the news is antigovernment, antiSemitic, xenophobic, racist, and militant rhetoric. Certainly, this promotes the idea in listeners that these ideas are widespread and reasonable. I hope that they are not.

Last summer, after a few hours of particularly disturbing speech, I told a friend that I wouldn't be surprised if some of the crazies who take this seriously begin to act on their hateful reference signals. They have. Only now is the generalized public becoming aware of right wing militias and other fascist paramilitaristic organizations that have proliferated in recent years. How many? Dozens of organizations (often linked with FAX, short wave and broadcast radio, computer bulletin boards, mailing lists, and numerous magazines and newsletters), tens of thousands of members in varying degrees of involvement, and lots of supporters. These people are dangerous not just because of their intolerance, hatred, and ignorance, but more so because they are organized. We know what these people and organizations represent. If we think that we got rid of fascism fifty years ago, then we were wrong.

Gary,

In your post you questioned Rick's allusion to the causality of hate speech. I agree that hate speech does not cause violent behavior. However, I do not see why we must immediately reduce influence to mechanical causality. It might work in this way: The hate speech might support ideas (even clarify them) already held by people, thus creating an illusion of popular support and movement. Also, hate speech might make people aware of ideas and plans of action. That is, when we listen to music, read books, etc. we often come into contact with ideas that we had not considered previously. It is possible to adopt those ideas/plans of action as reference signals and take purposive action.

Kids, hearing about killing cops, are aware of alternative plans of action. They may adopt these plans. Young people reading the Kama Sutra may adopt the alternative approaches to love making therein. Being aware of alternatives does not cause one to behave in a certain fashion. Should the government, or "we the people" pull the plug on the hate speech dominating broadcast radio? Probably not. However, it is our responsibility to call them by their name, inform people about who they are and what they are up to. It is a good excuse to discuss fascism and the consequences of such ideas in our classes, church groups, community associations, labor unions, and radio broadcasts that are truly democratic.

Later,
Dan Miller
millerd@udavxb.oca.udayton.edu

Date: Wed, 26 Apr 1995 08:22:18 -0700
From: Dennis McCracken <dennis@COMMUNITY.NET>
Subject: Re: Obviousness

[Dennis McCracken 950426.0810]
[Dag Forssell (950425 0930)]

>>Musings on obviosness.

Don't give up on me yet: My new copy of netscape doesn't have the URL for CSGnet FTP's in it yet and I haven't got round to adding it to my bookmark file. I still plan to send for the update disketts with the sorted threads. You should be getting a mail order soon.

Dennis

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Date: Wed, 26 Apr 1995 09:04:37 -0700
From: Richard Marken <marken@AEROSPACE.AERO.ORG>
Subject: Re: How can words disturb? Revelations

[From Rick Marken (950426.0900)]

Gary Cziko (950426.0305 GMT) --

> **how it is that "words can make a difference."... you must feel that hateful words can cause at least some of the people who hear the words to hate, too, or else you probably wouldn't be concerned about the hateful words. But how does the suspected effect of hateful words mesh with PCT? How can words cause one to hate or do anything if hateful reference levels are not already there? This looks like an input-output (S-R) view of behavior to me.**

It did to me too, when I wrote it. So let me try to clarify my point.

I don't think one person's words can cause another person to hate. I think "hate speech" is a "level of perception" problem. "Hate speech" points to possible causes of control error at too high a level of abstration. For example, people who are uncertain about their jobs have a real control problem. "Hate speech" identifies the cause of the problem at the category level of perception; "government". "liberals", "welfare mothers". People can easily be convinced that these categories are the source of their problem, especially when they see individual instances of these catagories (the tax collector, the lawyer defending a criminal, the person on welfare getting a guaranteed wage). I define "hate" as a chronic error in a category control system; the reference for the existance of the category is set to "zero" so the perception of instances of the category creates error. Hitler and his buddies managed to get in a position where they were actually able to systematically control for the error created by the exitance of "jews", "homosexuals", "gypsies" and "communists"; they actually got rid of them, physically. The Oklahoma bomber tried this on a smaller scale.

Hatred is error in a control loop that can only be eliminated by 1) killing millions of innocent people or 2) ceasing one's control of the category perception. I prefer option 2) and I think that hateful people can be encouraged to move in this direction if they can be encouraged to go up a level and see their hatred (control for the existence of categories of individuals) as somethong they are doing. Hate speech in the media just keeps hateful people's consiousness at the level of their existing hatred -- and implicitly approves of

it (which is why the audiences for this stuff are so large). I suggest that people like Rush Limbaugh might say something like the following before every show:

"We have real problems but they are not caused by "liberals" or "government"; we have problems because some liberal ideas are wrong, some government policies are wrong; but the people who have these ideas (like the President) are people like you and me; individuals who are just trying their best to live their lives. From now on let's challenge the ideas and policies but lets remember that the people who disagree with our ideas are still good people."

If he said something like this I think the level of hatred in this country would suddenly go down several dB, not becuase it was "caused" to but because many people would be able to go up a level and stop trying to control their problems by controlling categories.

The IV-DV model of behavior is still wrong. Hate speech does not cause hate; it just sustains it.

Greg Williams (950426) --

> **Hard as it has been, I've vowed to stay off the net ever since I decided that my presence contributed to the outpouring of hateful words in your posts. Now that you've seen the light, I'm tempted to return -- but I think I'll wait a bit to see how thorough your recantation actually is. ;-}**

I have been trying, since last year's "exile" to avoid what I consider "hateful speech". Bill Powers showed me (by doing a database search on the keyword "idiot") that I would often refer to groups of people ("conventional psychologists", "republicans", "religious believers") as "idiots". I didn't consider this "hateful speech" at the time but it fits my definition now; it implies that I have a reference of "zero" for these catagories of individuals. I have not only tried to eliminate such hateful speech from my posts, I have also tried to remember that it's the ideas, not the individuals who espouse them, that I don't like. But I think talking (and thinking) in categories is very seductive (for me, anyway) so I always will have to "watch myself" when I post; but I think it's worth it, given the downside of what I had thought of as relatively "innocent" hate speech.

I think that the level of what I consider to have been "hate speech" has declined markedly in my posts since last year. If you have still been perceiving a lot of hate speech in my posts over the last few months, then I think you might be perceiving things as "hate speech" that I don't perceive as such. So you might end up being disappointed by the thoroughness of my "recantation". I will still, for example, feel free to act as the "PCT policeman"; if someone says that a model or theory is consistent with PCT (when it's not) I will say that it's not and explain why. I don't consider giving an accurate description of PCT (including pointing out when someone is wrong about PCT or the nature of behavior) to be "hate speech". If you do, then I guess you will just have to endure it in silence or expose it for all to see.

Best Rick

Date: Wed, 26 Apr 1995 08:36:36 -0700
From: Dennis McCracken <dennis@COMMUNITY.NET>
Subject: Re: Disturbing Words

(Rick Marken (950425.1915))

>What we say makes a difference.

I echo your sentiments completely and support your aim reduce what you perceive to be hateful speech. But, please don't hold back on clarifying speech. I have found posts on "anticipation" and related matters extremely useful in clarifying my thinking. All the efforts to insist on the precision helps me realize how far I have to go in really "getting it. " It works though.

Lurkin' and learnin'

Dennis.

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Date: Wed, 26 Apr 1995 12:38:45 -0500
From: Richard Robertson <urrobert@UXA.ECN.BGU.EDU>
Subject: Re: CSGnet help

[from Dick Robertson] 950426.1240CDT
Yes, Mark, you made it. Glad to see you here.
best, Dick R

Date: Wed, 26 Apr 1995 10:57:59 -0700
From: Richard Marken <marken@AEROSPACE.AERO.ORG>
Subject: Scarey Haters

[From Rick Marken (950426.1100)]

Dan Miller (950426) --

Great to hear from you!

>Rush Limbaugh is a pussycat compared to Gordon Liddy

I just heard a report about him on NPR -- yikes!

> Only now is the generalized public becoming aware of right wing militias and other fascist paramilitaristic organizations that have proliferated in recent years.

I was happier when I was ignorant.

There is a funny(?) side to this. I seem to recall that one big complaint from many cold war conservatives (like Limbaugh and Liddy) about "commies" was that they advocated the violent overthrow of the government. Now we've got paramilitary groups that are openly preparing for the violent overthrow of the govenment and many of these same conservatives are defending them. Maybe it wasn't the "government overthrow" thing that was the real problem (disturbance) after all? Gives one a better idea of what some of these cold warriors were controlling for.

> The hate speech might support ideas (even clarify them) already held by people, thus creating an illusion of popular support and movement.

My point exactly. Well put!

Mark Lazare -- Your post was received.

Best Rick

Date: Wed, 26 Apr 1995 17:20:41 +0200
From: Clark Mcphail <cmcphail@UX1.CSO.UIUC.EDU>
Subject: Re: Disturbing Words

>[re Rick Marken (950425.1915)]
>
>What we say makes a difference.

What we say does make a difference. It makes a difference in the assertions of presidents, in the assertions of talk show hosts, in the writings of scholars, and even in the instructions experimenters give to subjects as they are asked to participate in a rubber band exercise or a tracking task. What is "said" is not a stimulus that evokes an automatic response. There are no intrinsic stimulus properties in any language behavior. That does not mean, to me, that language behavior is unimportant or nonconsequential. So I agree with you that "what we say makes a difference."

> Oklahoma will always remind me of the human consequences of hateful speech.

As a native Oklahoman, a resident of Oklahoma City for 24 years, a regular visitor over the subsequent 33 years, a childhood friend of the architect who designed the Federal Bldg, I know many people who lost friends, colleagues and acquaintances in that tragedy. I do not write here to diminish that tragedy in any way.

> Nobody questions the "right" of people to say almost anything they want in the media; I am all for free speech myself.

As a student of first amendment activities (political demonstrations) over the past three decades I share those opinions. It is clear to me that the phenomena I study cannot occur independent of the language behaviors that planners and organizers employ to engage others' assistance in mobilizing people and other resources for these predominantly nonviolent gatherings of people and the various collective actions in which some members of those gatherings engage. From my study of both terrorist and police manuals it is equally clear that complex

sequences of violence - whether bombing attacks by enemies of the state or SWAT raids by agents of the state - cannot take place without the use of verbal utterances and writings and nonverbal gestures. Speech or language behaviors were inextricably involved in both the planning and execution of the tragedy in Oklahoma City as well as in the rescue efforts which we have witnessed in the aftermath. What we say makes a difference.

> **Here's a prayer for a future of peaceful political debate; we can get there if we can get beyond our hate. . . . If we can see what we are controlling for we can stop controlling for it**

What we say makes a difference. My request for the future of productive scholarly debate is that we take the phenomena of spoken, written and gestured language seriously and come to better understand this in terms of PCT, including the labels (symbols) of categories of experience upon which we draw in order to communicate to others that we presume (?anticipate?) share those symbols and perhaps some of the experiences to which those symbols refer. One of the most useful PCT discussions I have heard of symbol acquisition and communication came from Martin Taylor at the CSG meeting in Durango in 1993. I will not attempt to summarize his position here but hope that Martin will take this occasion to reintroduce his PCT analysis of language acquisition and use. What we say makes a difference.

Clark

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Date: Wed, 26 Apr 1995 15:43:33 -0700
From: Richard Marken <marken@AEROSPACE.AERO.ORG>
Subject: Re: Disturbing Words

[From Rick Marken (950426.1540)]

Dennis McCracken (950426) --

> **I echo your sentiments completely and support your aim reduce what you perceive to be hateful speech. But, please don't hold back on clarifying speech. I have found posts on "anticipation" and related matters extremely useful in clarifying my thinking. All the efforts to insist on the precision helps me realize how far I have to go in really "getting it. " It works though.**

Thank you. Thank you. What a wonderful, heartening post.

As I said to Greg Williams in an earlier post, I will certainly continue to insist on precision in matters PCT. What I will try to avoid (and what I hope I have avoided for some time) is giving the impression that I think anyone who doesn't understand something about PCT is anything other than a wonderful person who (from my perspective) doesn't understand something about PCT.

Nice to have you here, Dennis.

Best Rick

End of CSG-L Digest - 25 Apr 1995 to 26 Apr 1995
****

Date: Fri Apr 28, 1995 5:39 am PST
Subject: CSG-L Digest - 26 Apr 1995 to 27 Apr 1995

There are 5 messages totalling 853 lines in this issue.

Topics of the day:

1. **Disturbing Words, Anticipation**
2. **Eyeblink Simulation**
3. **Darwin on page 76**
4. **Rick Marken's "Disturbing Words"**
5. **Predictive stimuli, etc.**

--

Date: Thu, 27 Apr 1995 10:26:50 -0700
From: Richard Marken <marken@AEROSPACE.AERO.ORG>
Subject: Disturbing Words, Anticipation

[From Rick Marken (950427.1030)]

Clark Mcphail (950426) --

> My request for the future of productive scholarly debate is that we take the phenomena of spoken, written and gestured language seriously and come to better understand this in terms of PCT

We have had some excellent (in my opinion) discussions of language on this net but it seems like the last sustained discussion od langauge happened a couple years ago. It would be great to have more of these discussions; language is certainly a very important human activity; just look at all the blabbing we do on the net! But all you have to do to have a productive scholarly debate about language on this list is to start talking about language; the discussion won't happen on its own.

Martin Taylor (950425 10:45) --

> Once again, I agree with Rick (contrary, I suppose, to anticipations of some). When Chuck posted his comment, I did not see what it had to do with

anticipation, since everything in the various parses had to do with simple control of some higher-level perception. But if you think of sequence control as representing some kind of anticipation (that the reference sequence will in fact produce perceptions aiding the higher level control), then the parsing is simply a stage in developing the Test for whether there is such a sequence being controlled.

I think sequence control does come close to being consistent with my everyday notion of anticipation. But I still think that there is no actual anticipation here because there is no expectation or prediction that one event will follow another.

In sequence control, a perceptual function is determining whether a sequence of elements, like A,B,C is _in progress_. If each element occurs in sequence the perceptual signal remains "high" indicating that the sequence IS in progress; if an element occurs out of sequence (A,C,B) the perceptual signal goes "low" indicating that the sequence is not currently in progress. If the control system's own outputs are contributing to the production of this sequence it will seem like the output that produces, say, C after B has anticipated the occurance of B. But this is not really an anticipation because the control system doesn't assume or predict the occurance of B after A; the perceptual function is LOOKING for B after A and C after B but it is not really anticipating it. The perceptual funciton is just reporting the status of the sequence perception.

I think the only time we really "anticipate" is when we produce an expected future perception as a present imagination. I would say that I am anticipating C if, after B occurs, I imagine C. Then, whether C happens or not, I would say that I have anticipated C. I guess I am saying that what we refer to, in everyday life, as "anticipation" is what I would call "control of imagination" in PCT; anticipation happens when we purposely replay, via the "imagination connection", a perception that completes a perceptual sequence.

Best Rick

Date: Thu, 27 Apr 1995 13:23:11 -0500
From: Bruce Abbott <abbott@CVAX.IPFW.INDIANA.EDU>
Subject: Eyeblink Simulation

[From Bruce Abbott (950427.1320 EST)]

No, it's not classical conditioning (YET!), but it _is_ an attempt to model some of the control systems involved in the eyeblink response. The program appended below represents a two-level hierarchical control system. The lower-level system controls the sensed length of a muscle that determines the position of the eyelid. The upper-level system controls the intensity of sensation arising from corneal sensors by manipulating the eyelid position reference value of the lower-level system. The system diagram looks like this:

```
              rc    LEVEL 1: corneal sensation intensity
               |
       pc      V     ec
    +------>[comp]-------+
    |                    V   slowc * gc *ec
```

```
 [input]              [output]
    ^                   |  +------- r2
    |                 r |  |
    |             p     V  V      e    Level 2: eyelid position
    |         +-------->[comp]---------+
    |         |                        |  slow * -g * e
    |         |                        V
    |   ki [input]                  [output]  ko
    |         ^                        |
    |       x |                        | o
    |         +-----[length sensa]-----+
 ic |                                  |
    |                          [muscle length]
    |                                  |
    |                                  V ml
    |                          [eyelid position]
    |                                  |
    |                                  V expose
    |                     [tear evaporation/replacement]
    |                                  |
    |                                  V
    +---------------------[corneal sensation intensity] kc
```

The diagram has been simplified somewhat: the leaky integrator functions and the functions that translate eyelid exposure to corneal sensation intensity are not represented.

When the eye is open, tears evaporate from the corneal surface, causing the corneal sensation intensity to increase exponentially at a rate determined by constant kc. This intensity is compared in the Level 2 system to the corneal intensity reference. As the signal rises above reference, the comparator generates an error signal which is applied to a "leaky integrator" output; this output becomes part of the reference level for the Level 1 system. As the reference moves toward the "closed" eyelid position, the lower-level system follows the reference, limited by its gain and slowing factor. A slowing factor in the corneal intensity integrator was included to mimic the delayed effect of eye-closure and and tear evaporation on corneal intensity. Because of this delay, the change in intensity does not keep up with the change in eyelid position, so the upper-level intensity system keeps the eyelid position reference moving toward closed. When the eyelid closes the intensity reduction finally "catches up," then falls below reference, leading to the lower-level reference being set toward the open eyelid position. The result is a blink.

The sensation intensity function uses a faster slowing factor during eyelid closure than when the eyelid is stationary or opening, to simulate the effect during closure of tears being wiped across the cornea (rapidly reducing sensation) and the slower, incrementing effect of evaporation. The rate of change takes into account the proportion of the cornea exposed to air. This arrangement seems to work, but I would like to replace it with something closer to the actual physics. The problem is that I haven't seen how to do that as yet. Any suggestions?

The reference level for eyelid position can be changed during the run to simulate voluntary control; this reference is summed with the signal arising from the intensity control system. Thus you can simulate what happens if the person tries to keep the eye half open, etc.

You can experiment with different intensity reference levels and change the slowing factors for the "tear wiping" and "evaporation" intensity effects. You can also alter the gain and slowing factor of each control system.

In the classical conditioning procedure, a puff of air is directed at the cornea. The next step is to add this effect to the model. The puff would directly increase the intensity signal (via a leaky integrator), but this effect would be diminished in proportion to the degree of cornea exposure at the time of the puff. The model should not have to be altered otherwise.

The program is written in Borland (Turbo) Pascal 7.0. To compile the program you will need the grUtils and setparam units. Source code for these units has been posted on CSG-L; if you need the source code, send me an e-mail message to that effect and I will send it to you.

Regards, Bruce

```
program Blink;

{   Simulates a two-level control system regulating the intensity of sensation
    arising from the surface of the cornea. As tear fluid on the corneal surface
    evaporates, intensity increases. The intensity control system reacts by
    changing reference to a lower- level eyelid position control system (actually
    controlling the perception of eyelid muscle length). The lid begins to close,
    wiping tear fluid on the cornea, thus reducing sensation intensity. However,
    the sensation changes slowly relative to the speed of the positioning system,
    leading the upper-level system to overshoot and then undershoot the intensity
    reference. The result is a blink. Bruce Abbott

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   950427
}


uses DOS, CRT, graph, grUtils, setparam;

var
  MaxX, MaxY, x1, x2, y1, y2: integer;
  p, r, e, g, slow, o, x, ki, ko, ml, maxl, t, dt,
  nmax, kic, koc, kc, k1, k2, ic, pc, rc, ec, oc, gc, slowc,
  r2, expose: real;
  param: paramlisttype;
  ch : char;

procedure InitScreen;
begin
  ClrScr;
```

```
  InitGraphics;
  MaxX := GetMaxX; MaxY := GetMaxY;
end;

procedure LoadParams;  { Set up parameters }
begin
  with param[1] do    { cornea intensity reference }
    begin
      legend := 'ref cor';
      kind := 'r';
      rvinit := 50.0;
      rvmin := 0.0;
      rvmax := 100.0;
      rvstep := 1.0;
      rv := @rc;
    end;
  with param[2] do    { cornea intensity control gain }
    begin
      legend := 'gain cornea';
      kind := 'r';
      rvinit := 100.0;
      rvmin := 0.0;
      rvmax := 200.0;
      rvstep := 1.0;
      rv := @gc;
    end;
  with param[3] do    { cornea intensity control slowing factor }
    begin
      legend := 'slow corea';
       kind := 'r';
       rvinit := 0.025;
       rvmin := 0.000;
       rvmax := 1.000;
       rvstep :=0.001;
       rv := @slowc;
    end;
  with param[4] do    { tear evaporation rate }
    begin
      legend := 'evap rate'; { rate of intensity change due to evap. }
      kind := 'r';
      rvinit := 0.005;
      rvmin := 0.0;
      rvmax := 1.0;
      rvstep :=0.001;
      rv := @k1;
    end;
  with param[5] do    { cornea wetting during eyelid closure }
    begin
      legend := 'wet rate'; { rate of intensity change due to wetting. }
      kind := 'r';
      rvinit := 0.90;
      rvmin := 0.0;
      rvmax := 1.0;
      rvstep := 0.01;
      rv := @k2;
```

```
    end;
  with param[6] do    { eyelid contraction reference }
    begin
      legend := 'ref lid';
      kind := 'r';
      rvinit := 100.0;
      rvmin := 0.0;
      rvmax := 2000.0;
      rvstep := 50.0;
      rv := @ r2;
    end;
  with param[7] do    { eyelid contraction gain }
    begin
      legend := 'gain lid';
      kind := 'r';
      rvinit := 100.0;
      rvmin := 0.0;
      rvmax := 200;
      rvstep := 1.0;
      rv := @ g;
    end;
  with param[8] do    { eyelid contraction slowing factor }
    begin
      legend := 'slow lid';
      kind := 'r';
      rvinit := 0.025;
      rvmin := 0.0;
      rvmax := 1.000;
      rvstep := 0.001;
      rv := @ slow;
    end;
end;

procedure InitSim;
begin
       t := 0.0;    { initial time }
      dt := 0.1;    { time integration constant }
    nmax := 2000;   { max neural current }
       r := 0.0;    { eyelid position reference}
      ki := 100.0;  { eyelid position input constant }
      ko := 0.001;  { eyelid position output constant }
       o := 0.0;    { eyelid position output }
       x := ko * o; { eyelid position signal to muscle}
      ml := x;      { eyelid muscle length, mm }
    maxl := 15.0;   { max eyelid contracton }
  expose := 1.0;    { % eyeball exposed }
      ic := 1.0;    { corneal sensation intensity }
     kic := 100.0;  { corneal sensation input constant }
     koc :=  10.0;  { corneal sensation output constant }
end;

procedure RunModel;
var
  oldexp: real;
begin
```

```
{ ***** eyelid position control ***** }

  p := -ki * x;
  if p < 0.0 then p := 0.0;
  e := g*(p - r - r2);
  o := o + slow * (-e - o) * dt;
  if o < 0.0 then o := 0.0;
  x := -ko * o;
  ml := x;
  if ml < -maxl then ml := -maxl;

{ ***** cornea sensation control ***** }

  oldexp := expose;
  expose := (maxl + ml)/maxl; { proportion of cornea exposed }
  if (expose < oldexp)  then kc := k2 else kc := k1;
  ic := ic + kc * (expose - ic) * dt;
  pc := kic * ic;
  ec := gc * (pc - rc);
  oc := oc + slowc * (ec - oc) * dt;
  r := koc * oc;
  if r < 0.0 then r := 0.0 else if r > nmax then r := nmax;
end;

procedure Plot;
var
  v1, v2, v3, v4, v5, ypos, time: integer;
begin
  t := t + dt;
  time := round(t);
  if time > X2 then
    begin
      t := 0.0;
      time := 0;
      ClearViewPort;
    end;
  v1 := round(0.5 * rc);
  v2 := round(0.5 * pc);
  v3 := round(50.0 * (1.0 - expose));
  v4 := round(0.03 * r);
  v5 := round(0.03 * p);
  ypos := (y2-y1) div 5 - 3;
  putpixel(time, 3 * ypos, white);
  putpixel(time, 1 * ypos - v1, lightred);
  putpixel(time, 2 * ypos - v2, yellow);
  putpixel(time, 3 * ypos - v3, lightgreen);
  putpixel(time, 4 * ypos - v4, lightred);
  putpixel(time, 5 * ypos - v5, yellow);
  if time mod 100 = 0 then line(time, 3 * ypos + 3, time, 3 * ypos - 3);
end;

begin
  ClrScr;
  InitSim;
  InitScreen;
```

```
  LoadParams;
  x1 := 1; x2 := 475;
  y1 := 101; y2 := MaxY-1;
  setcolor(lightgreen);
  outtextXY(175, 0, 'EYEBLINK SIMULATION');
  setcolor(yellow);
  outtextXY(10, 20, 'This simulation models a two-level contol system .');
  outtextXY(10, 35, 'The bottom-level system controls eyelid muscle length.');
  outtextXY(10, 50, 'The top-level system controls corneal sensation');
  outtextXY(10, 65, 'intensity.');
  setcolor(lightgreen);
  outtextXY(30, 90, 'Cursor up/dn: select     +/-: change      Esc: Quit');
  setcolor(lightred);
  outtextXY(x2 + 5, y2-313, 'r corneal intensity');
  setcolor(yellow);
  outtextXY(x2 + 5, y2-241, 'p corneal intensity');
  setcolor(lightgreen);
  outtextXY(x2 + 5, y2-169, '% eye closure');
  setcolor(lightred);
  outtextXY(x2 + 5, y2- 97, 'r lid contraction');
  setcolor(yellow);
  outtextXY(x2 + 5, y2- 25, 'p lid contraction');
  setcolor(white);
  SetUpParam(x2, 10, 8, param);
  SetViewPort(0, 0, MaxX, MaxY, clipoff);
  setcolor(lightgray);
  Rectangle(x1-1, y1-1, x2 + 1, y2 + 1);
  SetViewPort(x1, y1, x2, y2, clipon);
  setcolor(white);
  repeat
    RunModel;
    Plot;
    if keypressed then
      begin
         SetViewPort(0, 0, MaxX, MaxY, ClipOn);
         ch := changeparam(param);
         SetViewPort(x1, y1, x2, y2, ClipOn);
      end;
  until ch = #27;
end.
```

Date: Thu, 27 Apr 1995 17:21:28 -0400
From: DForssell@AOL.COM
Subject: Darwin on page 76

[From Dag Forssell (950427 1430)]

I found this article in NEWSWEEK, May 1, 1995

Seems to me we have discussed these issues from time to time. What is missing from this article is an explanation.

Gary, how about a sneak preview of how you treat this in your book. By the way, what is the deal on ordering your book from you. We get CSG discount, yes?

DARWIN, CALL YOUR OFFICE

Science: Evolution is smarter than he knew

Isn't it lucky that some proto-giraffe, eons ago, developed a random mutation or two that produced a long neck? And that moths in l9th-century England spontaneously developed a mutation that made them as dark as the soot-covered trees where they alit (and could then be camouflaged)? Darwin thought that such random mutations, combined with nature selecting the "genetically fit,'' fully explained evolution. In other words, "mutations arise independently of biological needs," explains James Shapiro of the University of Chicago in the current issue of the journal Science. "The evolutionary watchmaker is blind." But for about a decade there have been hints that the watchmaker may, in fact, see. In experiments with bacteria, mutations that are useful arise more frequently than mutations that are neutral or deleterious--almost as if the microbes "know" which novel traits it would be good to have. To biologists, this un-Darwinian notion has been borderline heresy. But now two papers in Science go a long way toward making "adaptive mutations" respectable.

The researchers, one team at MIT and one at the University of Utah, examined _E. coli_ bacteria, the kind that live in the human gut. The bacteria lacked a gene that digests lactose--milk sugar--so the researchers put the microbes in a dish containing nothing but. Rather than dying of starvation, the colony of E. coli developed, 100 times more quickly than Darwinian evolution allows, a mutation that enabled them to eat lactose. That much had been reported last year, too. But the latest experiments give skeptics an explanation of how the bacteria seem to anticipate desirable mutations. It has to do with what passes for sex in the microscopic world: the transfer of a circle of DNA called a plasmid, in this case containing a gene that digests lactose. It happens that the systems controlling bacterial sex rev up when a bug is starving, the researchers report--exactly the situation of bacteria unable to digest lactose. Adaptive mutations probably don't work this way in higher organisms like giraffes and moths. But if primitive organisms preferentially acquire traits that help them survive, biologists will have to admit that, 136 years after "The Origin of Species," evolution still holds surprises.

SHARON BEGLEY

Best, Dag

Date: Thu, 27 Apr 1995 17:56:10 -0700
From: RUNK@OREGON.UOREGON.EDU
Subject: Rick Marken's "Disturbing Words"

[From Phil Runkel on 950427]

TO Rick Marken in reply to his post of 950425.1915:

I am very grateful to you for saying what you did on the 25th.
You enabled me to weep for the dead.

Date: Thu, 27 Apr 1995 22:03:16 -0600
From: "William T. Powers" <POWERS_W@FORTLEWIS.EDU>
Subject: Predictive stimuli, etc.

[From Bill Powers (950427.1720 MDT)]

I'll be catching up for a few days, a little at a time.

IMPORTANT NOTE: NEW ADDRESS. When I got back I found that Fort Lewis has changed its email address; it now acts as its own postmaster. So everyone who sends me direct messages should note my

NEW EMAIL ADDRESS:

powers_w@fortlewis.edu

I have re-subscribed with the new address, so it should be correct when you "review csg-l" the list.

Bill Leach, Bruce Abbott:

RE: anticipation, prediction, etc.

What I'm hoping is that sooner or later we will start considering these terms to be quaint. When we speak theoretically and in terms of models, I maintain that we should speak as literally as possible, not in metaphors. I agree with Bill Leach that we should give some consideration to terms in common use, figuring out whatever equivalents we can come up with. But with care.

Anticipation, as far as I can see, doesn't literally happen: it is impossible to base any action on things that haven't happened yet. So when we speak of an anticipatory response, we can be pretty sure that whatever is giving an impression of anticipation, the underlying process uses only present and past information. The net result is something that LOOKS LIKE anticipation, but some other process must really be responsible. It seems right now that one way to give the appearance of anticipation is to define a perceptual input function that reports the state of a variable composed of several environmental variables, some of which occur prior to others.

There are, of course, cases in which literal prediction and anticipation are carried out. These involve formal pencil-and-paper calculations or equivalent symbol-manipulating processes in which current trends are extrapolated to yield representations of imagined states of affairs. Goals and subgoals are chosen in the light of their predicted consequences, creating a situation in which the future seems to have an effect on present actions.

I guess I'm a deconstructionist. When people present models that purport to show certain phenomena like conditioning or anticipation or reinforcement, I always look at the model and ask "Now what does this thing actually DO?" In a lot of

cases, the fancy phenomena are gratuitous interpretations; if you understand what the basic model does in its own terms, you understand all that there is to understand about the model. Why dress up a simple phenomenon in fancy terms?

Greg Williams (day after Easter '95 - II)--

RE: New View catalog and PCT credibility

As Mary said, the term "control theory" is in the public domain. I'm uncomfortable with having PCT appearing in the middle of all that other stuff, mostly because Glasser is grabbing credit for a half-hearted attempt to incorporate PCT into his own work. On the other hand, I don't want to be in the position of saying that nobody but the Central Committee has a right to think about control theory and its applications to human nature; that would be a little too much like the way that Glasser passes on the purity of anyone who aspires to be a Reality Therapist. I'm happy that a bunch of people who haven't been exposed to anything but Glasser's version will now see that there is more to it, and will have access to another kind of source materials.

In any group of people hearing about PCT for the first time, there will be some who become intrigued and look further into it. Those are the people who eventually join the effort. This happens in all venues, whether "respectable" or not; in fact, my impression is that it is more likely to happen when the audience has no prior stake in some antithetical belief system.

> I hope that PCTers won't be contributing to the Dumbing [of America] by ignoring the problem, selectively.

Before we can explain how PCT applies to human behavior, we have to have somebody listening. I trust you're not suggesting that any of the authors would tailor their explanations with an eye to increasing book sales rather than making sure the basic ideas were understood correctly. I know that when I proposed writing a book called something like "Sex, Violence, and Riches through PCT" I did not get an enthusiastic response from my colleagues.

I echo Rick's remarks in one regard: it doesn't do much for book sales when professors call me up asking if they can Xerox a chapter from one of my books for a class to use. ------------------------------

Bruce Abbott (950417.1300 EST)--

> For example, a bell ringing just prior to dry food powder being placed into a dog's mouth would allow the dog to get the saliva flowing and thus have its mouth in an optimal state for the receipt of the food. This is, of course, the way Bill P. described his model as working, although he used the term "action" rather than "response" to describe the change in output due to the disturbance provided by the bell, so I am justified in suggesting that Bill's (Wayne's) model falls into the "preparatory response" category.

What keeps getting overlooked here is that the US is a disturbance of a controlled variable that never gets mentioned in the classical analysis. Wayne's pregnant insight was that all unconditional responses are really actions designed to keep certain variables under control, with the US playing the part of a

disturbance. When you look at this control process under some very limited conditions, you get the impression that the UR is simply caused by the US.

As Rick pointed out, there is no such thing as a controlled response or action in PCT. The output simply varies as a function of the error signal. You can arrange all sorts of special situations to produce errors, but this doesn't mean there's a special phenomenon going on for each special condition.

I can see what you're trying to do by finding PCT explanations for the basic concepts of classical and operant conditioning. In the best of all possible worlds, it should be sufficient to show how special theories are supplanted by a general theory that explains them as special cases. Perhaps in this paper you're (I suspect) working up to, you will be able to do this in a more convincing way than I've ever succeeded in doing. But don't be too disappointed if those to whom you address these arguments take them in a direction that's exactly the reverse of what you intend, by claiming that you've shown that the classical approaches already handle the phenomena.

> I am not defending Pavlov's theory, mind you, but Pavlov's methods and his ability to create a theoretical framework of the highest scientific caliber, one that elegantly fit the available data. Yeah, we can see where he went wrong, but we have the benefit of 20-20 hindsight.

My cryptic remark about Pavlov's theorizing was a knee-jerk response to terms like inhibition and disinhibition. Such terms are not only stabs in the dark, but make special phenomena out of things that systems modelers handle just as examples of addition and subtraction. We've learned a lot about how to deal with complex systems since Pavlov's day. While this may excuse Pavlov's limited capacity to think up a working model, it doesn't make his efforts look any more impressive to me.

I think that Dick Robertson has some information about Pavlov's experimental methods; I based my comment on what I remembered. Most scientists, when you get to know a bit about them, turn out to put their pants on one leg at a time just like anyone else.

--

Bruce Abbott (950417.1550) --

RE: predictive cues

This whole argument consists of statements whizzing past that fail to connect at a point.

ME:

>> However, even though I have been watching this coffee cup for over 12 hours, on and off, so far it hasn't predicted anything. Is there something wrong with it?

> This is the point at which I finally understood that you guys thought I was talking about cues being used to calculate an output.

What I was saying was that the coffee-cup failed to predict ANYTHING, whether it be an output, an input, or anything else. Coffee cups don't predict. They just sit there full of coffee, or empty as the case may be.

The power to predict lies in the observing system, not the thing being observed. Behavioristic psychology is awash in terms that attribute mental processes to nonliving variables or events. The reason is simple: we need those mental processes if we're to make any sense of what happens between physical variables when an organism is present. Yet under a system that forbids us to speak of processes internal to the organism, there is no way to use such terms correctly, as descriptions of an organismic process. Thus we talk about reinforcing stimuli, discriminative stimuli, choice-triggering stimuli, anticipatory stimuli, predictive stimuli, and so on and on.

I'm simply trying to point out that predicting or anticipating is something you do, not something that coffee-cups do.

> Let's say that promptly at 3 pm each day Mary comes into the room bearing a tray containing a steaming pot of hot coffee and two clean cups. By mutual agreement, this is your "break" time; you put aside the writing or program and the two you enjoy each other's company and the coffee for a few minutes.

It took a while to get Mary to stop laughing at this scene of domestic order. But on to your point:

> It is now five minutes to three. Even though you are on the brink of getting that last subroutine to work on ARM3, you click the "save" button and begin to clear the papers off the coffee table to make room for the tray. But Mary's not here yet. Is there something wrong with you? Or more to the point, what does a clock reading of 2:55 indicate about Mary?

If something were to happen every day at 3:00, a clock reading of 2:55 would predict nothing at all -- unless there were a person there who was expecting something to happen and was using the clock as a means of being ready for it. Take away the people and their goals and expectations, and the clock would just sit there ticking over its numbers, anticipating nothing and predicting nothing. It is, after all, just a counting device.

I'm sure you're quite aware that if we allow the language of sentience and purpose into the argument, all these problems of usage go away. The only reason that organisms behave is to control what happens to them. All the phenomena of classical or operant conditioning fall into place if we just recognize that organisms want things and act intentionally to bring them about, or to keep things from happening that they don't want. This language is clear, simple, intuitive, and in terms of PCT, right. There are no separate "conditioning" phenomena. There is only control and learning to control.

The language of behaviorism has come to be as it is because people have tried to explain behavior without reference to what the organism wants, to the organism's intentions. If behavior occurs that anticipates some regular event, the behaviorist doesn't have the option of speaking of a predictive process inside the organism. Instead, the prediction must be made to appear as part of the environment: as a "predictive stimulus," a stimulus that simply by virtue of its temporal relationship to other events is capable of eliciting a response at just the required time.

Perhaps it seems to you that I'm just being nit-picky about words. I don't think so. I think this subject brings us up against a real division between PCT and the pursuits that others call the "sciences" of behavior.

Wayne Hershberger (950417)--

> A conditioned reflex appears to be an endogenous disturbance which offsets an exogenous disturbance. If the exogenous disturbance does not materialize, the reflex is dysfunctional; it serves as a self-generated disturbance. If a cup of liquid is not as heavy as anticipated, I may throw the liquid into my face.

That is, this is what we would expect in a thought-experiment assuming that there is actually such a thing as a goal-less conditioned reflex. In fact, you regularly pick up coffee-cups the weight of which you do not know, and neither drop them nor fling the contents into your own face. This is what we would expect from a position-control system, but not what we would expect from an open-loop reflex system of the kind imagined by Pavlov et. seq. ad naus..

We have to be careful when using hypothetical behavioral examples to make a theoretical point. It is obviously possible to set up an experiment with a CS and a US and train an animal or a person until that person regularly reacts to the CS. When you then omit the US, it is quite likely that the reaction to the CS could be counterproductive (especially if some accurate adjustment is involved). This is the sort of situation that one has in mind when speaking of "throwing the liquid into my face."

However, if there is any significant number of cases in which responding to the US causes more error than it corrects, we would expect learning to _continue_. You might throw the coffee into your face once, or twice, or perhaps even three times (if the trials were sufficiently spaced within runs of "normal" trials), but eventually you would revise your control systems to accomplish the goal without this unfortunate side- effect. If you began with primitive force control, you would end up with a more capable position-control system, which doesn't need advance information about the weight of the cup.

When we set up experiments deliberately designed to fool an organism, we can often succeed -- at first. But to see what the organism is really capable of, we can't just stop there. The question is, are we seeing something fundamental about behavior, or are we just seeing an intermediate stage of organization as the organism progresses from no control at all to the best control it can achieve?

As became evident in some earlier discussions, measuring the response to the CS alone is not a simple matter. If, after initial training, you just present the CS over and over, pretty soon there will be no response at all. If you always present the US after the CS, over and over, other changes can take place depending on whether the response to the CS _when the US occurs anyway_ makes the error worse or doesn't matter (if the response is to depress a key, it probably wouldn't matter, but if the response must be to move a key by a specific amount, no more and no less, the response to the CS might well be thrown off by the occurrance of the US). If you omit the US on every 5th trial, the organism might ignore the CS for 4 of the trials and respond to it on the 5th.

In short, it seems to me that doing experiments intended to measure classical conditioning is a continual process of trying to outwit the organism as it learns more and more sophisticated control over what matters to it. You have to randomize the CS-only trials and keep them rare. You have to maintain some reason for the organism to respond to the CS and not simply ignore it (for example, by making the effect of the US more and more noxious until finally you get a response to the CS again). You have to avoid all patterns, lest the organism start taking them into account.

So instead of looking like a simple demonstration of a basic automatic reflex phenomenon, an experiment in classical conditioning begins to look more like a battle between two intelligences, one of them continually trying to learn the rules and use them to control its world, and the other continually changing the rules in the attempt to create some behavior that is basically inappropriate. The experimenter is setting up a paradox: the animal learns to press a bar or jump in order to keep from being affected by a shock; but if it succeeds in learning the rule, there is no shock to affect it, and thus no reason to produce the behavior.

> If reference signals always specify the intended value of some input, not output, the conditional reflex is not an intended output. But, neither is the consequence of the reflex an intended input--otherwise I should be pleased with the acceleration of the cup, if not the wetness of my face. If a classically conditioned reflex is mediated by a reference signal it is an odd sort of reference signal-- which Rick hasn't modeled yet. This is what I thought you were driving at.

It's not the reference signal that is odd, but the perceptual signal, reflecting the oddness of the environmnent. If the perceptual signal originally represents the degree of pain-signal caused by a shock, the response to the instantaneously-appearing shock is always too late to prevent at least a tenth of a second or so of shock. That alone is unusual -- an environment in which large painful effects appear instantaneously without any warning.

But the environment is also unusual in that there is a precursor event that appears instantaneously some fixed time prior to the instantaneous shock event. Thus it becomes possible for the organism to construct a different controlled variable -- not pain alone, but pain plus the first derivative of the precursor event. If the resulting perceptual signal can be kept close to zero, the total experienced pain from the shock can be lessened or even eliminated. This perceptual signal is caused to become nonzero either by a pain-signal or by the precursor event. If the shock alone occurs, the organism responds delta-t seconds too late, and it experiences the shock. If the precursor event alone occurs, the organism still responds delta-t seconds too late, but now the response is in time to prevent the effect of the shock. If the cost of the response to the precursor event is low enough, the organism will keep the average pain signal at a very low level by this means.

We have to remember what this sort of experiment was supposed to prove. It was supposed to prove that supposedly purposive or goal-oriented behavior was really just a matter of reflexive responses to stimuli. At the time Pavlov did his experiments, there was no other way to explain how an organism could produce behaviors that prevented bad things from happening. The time-delay involved was,

I think, secondary to the main point being made. Once the classical-conditioning phenomenon had been observed, it was simply assumed that this was how ALL behavior of that kind was produced: the environment kindly provided a timely signal that caused a behavior that had the right effect on removing a noxious stimulus or whatever.

In fact, for most disturbances of controlled variables there are very few precursor stimuli of any reliability or accuracy, and in most cases none at all that reach the senses. So the assumption that there must always be a CS is simply false -- it applies only in a few cases, most of them artificial and concocted for laboratory experiments. So while I'm game to spend some time on modeling classical conditioning, just to see how a control process might handle it, this line of enquiry is basically a dead end, because so little behavior will be explained by it.

That's enough for tonight -- more tomorrow.

Best to all, Bill P.

End of CSG-L Digest - 26 Apr 1995 to 27 Apr 1995
****

Subject: CSG-L Digest - 27 Apr 1995 to 28 Apr 1995
There are 7 messages totalling 822 lines in this issue.

Topics of the day:

1. **Dumbing down**
2. **Darwin on page 76**
3. **Eyeblink Simulation**
4. **Misc catching up**
5. **On Parsing Problems**
6. **Interview You?**
7. **Disturbing Words**

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Date: Fri, 28 Apr 1995 06:40:00 EST
From: Hortideas Publishing <0004972767@MCIMAIL.COM>
Subject: Dumbing down

[From Greg Williams (950428)]

I deleted Bill Powers' come-back post in which he replies to my comments on the NVP catalog awhile back, so I don't have his exact text. But he was considering the possibility that I might be accusing CSG authors of contributing to the dumbing down of Americans [and others :-)] by thinking about writing watered-down literature. In reply, no, I don't think CSG authors (those currently known to me, at least) would want to do that. What I meant about PCTers potentially contributing to the dumbing down is that if we don't speak out about misleading and incorrect (to use Rick's term) presentations of "all one Control Theory" (as

in the NVP catalog), we will be part of the problem. I am still hoping that the CSG authors represented in the NVP catalog will work with NVP staff to remedy the problem. I can see no excuse for complacency about future NVP publicity given the incorrectness regarding Control Theory in the catalog. Bill Williams has suggested to me that the authors might not care much about that incorrectness because it doesn't pose a threat to their references, and I agree that it doesn't. But I'm talking about a threat to the perceived (by outsiders) integrity of PCTers if they are seen as accepting and even being associated with such dumbing down as is promoted by NVP.

As ever, Greg

Date: Fri, 28 Apr 1995 16:41:20 +0000
From: CZIKO Gary <g-cziko@UIUC.EDU>
Subject: Re: Darwin on page 76

[from Gary Cziko 950428.1513 GMT]

> Gary, how about a sneak preview of how you treat this in your book.

Below is a short extract from Chapter 16, "Universal Selection Theory: The Second Darwinian Revolution" about the possibility of "intelligent mutations" in biological evolution. Basically, I argue that if some mutations are smart (which I understand has not yet been clearly demonstrated), then this smartness itself had to be the result of previous blind (unsmart) variation and selection.

"However, it may well be that this and other explanations for directed or "instructed" mutation are not necessary after all. Australian microbiologist Donald MacPhee and his colleagues provided evidence that, when placed in a medium of lactose, the mutations produced by glucose-metabolizing E. coli are indeed produced blindly. What seems to happen under the stressed condition of a glucose-poor environment is not a specific increase in the rate of adaptive mutations, but rather a general increase in the overall mutation rate due to inhibition of the mechanism that usually checks and repairs the genetic errors that arise during the normal functioning of the bacterium. So while mutations continue to be produced blindly, the higher rate of genetic change allows the bacteria to stumble on the adaptive genetic change more quickly than they would if left in their normal glucose-rich environment.

"But let us continue to imagine for a moment that a bacterium was able to change just those genes regulating metabolism in just the right way to allow for the digestion of a foreign sugar. If this were the case, it would be yet another example of a puzzle of fit demonstrating that the bacterium had somehow acquired the ability to sense a new sugar in its environment and alter its genome to digest it. But then we would be led to ponder how this adapted complexity could have originated in the first place, with cumulative blind variation and selection as a prime candidate to explain the source of this remarkable ability that somehow permitted the bacterium to instruct its genome to make the required changes to digest the new, strange food that was being served."

> By the way, what is the deal on ordering your book from you. We get CSG discount, yes?

The book will have to be ordered from MIT Press. And since they don't give me a CSG discount, I doubt that they would give one to anyone else.

It should be available around July for about $29 (a bargain for close to 400 pages of undiluted wisdom plus my photo on the jacket!). I will provide ordering information when it is published.--Gary

*** * * * * * * * * * * * * * * * * * ***
Coming to a bookstore near you this summer--a book by me!

WITHOUT MIRACLES
Universal Selection Theory and the Second Darwinian Revolution.
Cambridge: MIT Press (A Bradford Book).

Gary Cziko
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Date: Fri, 28 Apr 1995 11:38:50 -0500
From: Richard Robertson <urrobert@UXA.ECN.BGU.EDU>
Subject: Re: Eyeblink Simulation

[From Dick Robertson] 950428.1136CDT

It was gratifying to me to see your program Bruce and your analysis of the so-called "eyeblink reflex." It seemed to me a sophisticated expansion on the explanation that I and my students developed after running some experiments on it - all of which I reported in the textbook. Might I also suggest that the name we proposed for it - corneal moisturization control system - might be substituted for the old s-r term?

Best, Dick R

Date: Fri, 28 Apr 1995 15:30:16 -0600
From: "William T. Powers" <POWERS_W@FORTLEWIS.EDU>
Subject: Misc catching up

[From Bill Powers (950427.1038 MDT)]
Continuing catchup --

Greg Williams (day after Easter '95 - III) --

> After myriad complaints down the years on CSG-L about R. Beer, Carver and Scheier, various control engineers and human factors researchers, conventional psychologists, popular-press authors, etc., etc. not getting

> **their PCT-facts straight, silence about the NV catalog's representation of Control Theory would surely beg the question from anyone so heavily critiqued: I wonder how much I'd have to pay for them to stop criticizing _me_?**

There is nothing in the New View catalog amounting to misrepresentations of the PCT view. Such things may exist inside the pages of books presented for sale there; when they come up I expect we will offer our usual criticisms of them. What I have read of these publications (not much) leaves me with the feeling of seeing a kindergarten version of a grown-up subject, not serious proposals about human nature that call for comment one way or the other. I don't feel any vigilante's urge to cruise around looking for violations of PCT ideas, although I'm perfectly happy to take a stand when challenged. When I get my own copy of the catalogue I may have some comments to make to the Goods. But I'll make up my own mind, thanks.

You imply that I have sold out my scientific integrity in return for the promise of increased book sales; that anyone who could come up with my price could get me to withhold criticism. If that's your assessment of my character after our ten years of acquaintance, I'd say you need more practice in making judgments.

Bruce Abbott (950417.2035 EST)--

One of the problems with modeling anticipatory phenomena is just what you say: "'The' phenomenon of anticipation is really a number of different phenomena, for which different control models will need to be developed." But another and deeper problem comes from allowing the conventional experiments with classical (or any other kind of) conditioning to confine our picture of behavior to an artificially narrow set of conditions.

We agree that beneath the surface phenomenon there is always some sort of control process. This tends to be obscured, however, when we imagine situations in which one and only one action is available for accomplishing control -- a highly unnatural situation. It's this unnatural constraint that encourages us to go on talking about "the conditioned response," as if control is always associated with the same action. That is what the conventional model leads us to expect, because the conventional model sees the action as being dictated by the unconditional stimulus. Whether it is realized or not, the experimental setups used for demonstrating classical conditioning are constructed to encourage this interpretation.

If the response to a US is a UR consisting of a movement to the right, we would expect the CS also to produce a movement to the right -- not to the left or in some other direction. But we can easily set up an experiment in which both the CS and the US can lead to any direction of response at all.

Suppose that on the screen you have a row of spigots along the top, each with a growing drop of water at its tip. From time to time, one of the drops separates and falls. The subject moves a bucket to the left or right using the mouse, the object being to keep the water drops from reaching the bottom of the screen. The drops take about 0.3 seconds to fall to the bottom of the screen.

After this task has been learned, we will find that the subject can intercept some, but not all, of the drops. What we find is that when any drop starts to

fall, the response is a movement either to the left or the right, by any amount between zero and some maximum movement. So the "UR" to the "US" of a falling drop is not any particular movement of the mouse; it is simply whatever movement will get the bucket as close as possible to a position under the falling drop.

Now we can add a signal -- a flash of light over the spigot that is going to release a drop 1/2 second later. Now the subject is able to intercept all the falling drops. Omitting the falling drop on some occasions will still produce the movement of the bucket to the right position. If the wrong drop falls, the bucket will move to the signalled position, not the right position.

Both the US and the CS now produce responses -- but they produce responses of all possible sizes and directions. You can argue about how "U" the stimuli and responses are, but we are obviously seeing the basic phenomenon called classical conditioning. Even more obviously, however, there is no longer any such thing as "the" conditioned response. The basic mental model, in which a stimulus input simply becomes "reflexively" connected to a motor output, breaks down.

The breakdown happens when we construct an experiment which recognizes the basic fact of control: that it is a perceptual consequence, not an action, that is controlled. When the environment includes variations in initial conditions, we find that the actions change from instance to instance, while the perceptual outcome remains controlled. Once this principle is recognized, we can construct innumerable "classical conditioning" experiments in which the actions show completely arbitrary relationships to the supposedly controlling stimuli. In fact, we are forced to recognize that so-called classical conditioning is only an artifact of a special experimental condition, and has no general significance.

Chuck Tucker (950418) --

> If we parse any act we have to begin somewhere in that stream but we should not forget that the organism was doing something before we decided where we started the act to parse. So it we start with a "cup being lifted toward the mouth" (for an act of drinking) we should not forget that prior to that there was "a hand grasping the handle of the cup" and prior to that "a hand reaching toward the handle of the cup" and prior to that "the forming of the hand to grasp the handle of the cup" and prior to that ...... You should get the point. All of these parts of the act can be parsed in PCT terms. If you don't wish to label the action prior to when you start the act 'anticipation' then call it by another name but let's parse the act first so we have a notion of what action we have under analysis.

Bravo. A voice of simple sanity.
--
Bruce Abbott (950419.1550 EST)--

Language is a uniquely human invention designed to prevent communication. So much for evolutionary progress.

RICK:

> But this is just another way of saying that o = -d and that d(t) = f(d(t-dt).

BRUCE:

> Which is to say that it generates a perception, based on current perceptions, of the likely state of the variable in the next time- cycle, and begins to act on that perception in this time-cycle.

This is OK, but your way of stating it is needlessly complicated as well as misleading (there is only one process under way -- the next time cycle is irrelevant. And there is no computation of "likelihood"). In some cases of explicit prediction/anticipation, those involving higher levels of perception and control, your way of stating it would be appropriate and Rick's would not. I think it is an error to try to see lower-level processes as instances of higher-level processes, as if the same mechanisms were somehow involved in either case.

> the advantage of adding target velocity to the perceptual input function resides precisely in the predictiveness of the equation.

I think you have it backward: it is the sum of position and velocity that leads you to see predictiveness, but the fact that you call this predictiveness has no influence on the way the system works. You can solve the differential equations analytically or in simulation to see the effects of this arrangement in complete detail, without ever bringing in the abstract notion of prediction.

> Humpty Dumpty doesn't wish to call p + dp/dt a predicted target position, but most of us, I suspect, wouldn't have the problem he does with doing so. Saying that control of p + dp/dt is "just control" fails to communicate how p + dp/dt improves control (when it does) relative to p alone. Saying that p + dp/dt predicts where p likely will be at time tn+1 makes it abundently clear, which is why I prefer the latter.

In general, rate feedback from a controlled variable v is introduced as p = A*v + B*(dv/dt). If B is made very large, the approach of the whole system to equilibrium will be very slow -- there can be hundreds of "time cycles" before p approaches its final state of equality to the reference signal. It is difficult to specify the time at which the "prediction" is supposed to apply as a prediction of v, particularly as dv/dt is changing at the same time that v is changing toward its final state. What seems simple and clear when stated in words turns out to be less so when considered in terms of continuously-varying quantitative relationships.

As something to chew on, consider how a control process works when the perceptual signal is simply p = A*(dv/dt). With a constant reference signal, p will be changing at a constant rate, and dv/dt will be maintained at the value r/A. You can certainly interpret dv/dt as making a prediction of the next value of v after a time A, but this will hardly reveal what this control system is actually doing.

Martin Taylor (940419.1930)--

> "Information" was and is defined in one way and one way only, as reduction in uncertainty. Uncertainty has its own precise and technical definition, based on a subjective probability distribution.

And subjective probability turns out to be undefineable, so where does that leave us?

> ... if the real world were totally predictable there would never be a need for ANY control system.

I've let this pass too often. I think what you should say is that if the real world were totally predictable, the control-system solution to producing behavior would no longer be the only choice. One could, after all, sense all the variables that were affecting a controlled variable (or simply build their predicted values into an appropriate computer), solve the inverse kinematic and dynamic equations, and compute the output needed to create any selected result.

However, in most cases the control-system solution would remain the fastest and least complex way of assuring that an actual consequence of actions would follow a changing specification for the desired consequence. The Little Man Version 2, after all, works in a completely predictable world; there are no disturbances and all relationships are exact to the limits of the computations, with no noise. Yet it works much better and faster, with less computing power, than the alternative open-loop model.

Even if the world were actually predictable, it would be far more practical to ignore most of the disturbances that could in principle be predicted and compensated for, and simply rely on a good tight control system to eliminate their effects. The savings in computing time and computing power would be immense in most cases, even for very simple behaviors like preparing and eating breakfast. Why compute the exact conformation of your scrambled eggs so you can issue precisely the commands that will get a prescribed portion onto your fork, when you can just dig in and take whatever you come up with as a load?

> In (Bill Powers 950408.0715 MST) he talks about controlling a signal that varies upward from DC with a high-frequency cutoff, and compares it with controlling a signal that is a slow modulation of a high-frequency carrier. At the end of it, he says:
>That was aimed more toward Martin Taylor than Bruce Abbott.

> I am puzzled as to why. Bill, did you believe I thought you were unaware of the difference between bandwidth and high-frequency cutoff? I assure you that I assumed you to know the difference.

I knew that you know the difference. However, in early discussions you were talking about control systems which controlled by slow modulation of a high-frequency carrier as if they could produce better control than could a system with the zero-based bandwidth. I didn't comment at the time, but in fact when you switch from moment-by-moment tracking (zero- based bandwidth) to pattern-matching (matching phase and amplitude of generated output to an oscillating target) your control is worse, not better. Or to state that more exactly, the switch takes place at a disturbance frequency where the zero-based system is losing control, and what is accomplished is that the errors are kept from increasing as fast as they would if the switch were not made. However, the bandwidth of the pattern-matching process is narrow, and correction of disturbances of phase and amplitude is slower than correction of position control errors. Don't know if that's relevant to anything any more.

> There is, in this passage, some discussion of a "pattern-matching system" as opposed to a "moment-by-moment" system. I did not follow the argument, because EVERY perceptual input function can properly be called a "pattern matching system" whose output is the degree to which the incoming data matches the pattern defined by the input function.

I meant specifically matching an oscillating pattern in phase and amplitude.

>> The pattern-matching system actually has a much narrower bandwidth than the MBM system, although the center of its bandwidth can be at a higher frequency. The PM system is designed to control the match between regular recurring patterns, but it can correct errors only slowly.

> If any PIF is defined to have a narrow bandwidth, then of course it can correct errors only slowly. That's what narrow bandwidth means: low rate of variation, or, in other words, low information rate. A control system with a narrow-band PIF is a slow control system.

I wasn't questioning your theoretical knowledge; only pointing out that in real behavior, the pattern-matching types of control behavior do in fact have narrower bandwidths and hence control more slowly. In general systems theory you can of course have pattern-matching systems in which the center frequency is very high (megahertz or terahertz) and the bandwidth also respectably wide (kilohertz or megahertz), but these systems don't occur in organisms.

Bruce Abbott (950420.1105 EST)--

Let me horn in on your discussion with Rick again, with an observation about closed loops that bears frequent repetition.

There is a strong temptation to think

Perceptual event
THEN
Comparison
THEN
Error signal
THEN
output act
THEN
feedback effect
THEN
next perceptual signal

This is what makes "prediction" seem like a viable interpretation. But in fact, what is going on is (forgive the mess...)

```
Perception    Comparison    Error         Output        Feedback
Comparison    Error         Output        Feedback      Perception
```

Error	**Output**	**Feedback**	**Perception**	**Comparison**
Output	**Feedback**	**Perception**	**Comparison**	**Error**
Feedback	**Perception**	**Comparison**	**Error**	**Output**
Perception	**Comparison**	**Error**	**Output**	**Feedback**

Each line represents processes taking place at a given instant of time. As you scan your eye around the control loop you're moving your eye from an item in one line to an item in the next line directly below. If you're attending to "feedback" in the middle of the third line, you can see that the current feedback came from the output (above), and is leading to a perception (below) -- but out of the corner of your eye in the same line you can see that output and perception are going on (left and right) while you're looking at feedback, and farther toward the periphery of vision, error and comparison are also going on at the same instant.

The whole control loop is active at the same time. The transport lags we know are there do not turn this into a sequence of processes -- they just change the time stamp (slightly) on the information that each process is currently receiving. The only way to create the appearance of a sequential process is to start with a situation in which none of the functions is active and all the variables are zero. Then you can define a starting event and trace its effects once around the loop. But as soon as you've completed the first loop, on all successive trips you begin to run into effects that began one loop ago, two loops ago, and so forth, messing up the simple relationships you thought you saw the first time around. And to make matters worse, in the real system what we draw as a single feedback loop is actually a large number of superimposed loops operating through redundant parallel pathways -- with different transit times around each one. So the sequential appearance very quickly disappears. When you realize that all actions are normally continuous with actions that preceded and follow them, the sequential approach becomes simply untenable.

In our computer models we have one lonely CPU that has to do everything. We can't literally run all the functions at the same time, as we could do with an analog computer or a multiprocesser system. The nearest we can come is to compute our way forward around the loop, using the output of one function as input to the next, until we arrive where we started. But then there is an inevitable one-iteration glitch: we can't make the initial variable receive the output of the output variable in the same iteration. This leads to computational oscillations when we try to raise the loop gain too high, oscillations which are completely an artifact of digital computation.

Wolfgang Zocher's "SIMCON" is an analog computer simulated on a digital computer. The sequentiality problem is solved by giving each function an "old" and a "new" output. First all the "new" outputs are computed for each function using the "old" inputs ("old" outputs of contributing functions). Then, in a separate step, all the "new" outputs are transferred to the "old" outputs and another iteration takes place. This converts the single-processor computer effectively into a multi- processor, with one processor per function in the simulation. Now we can have simultaneous operation in all functions. The penalty for doing this is that we have to use much smaller values of dt, because effects propagate only from one function to the next during a single iteration, not all the way around the loop.

That is the only proper way to simulate a closed-loop control system on a digital computer, the only way that allows all functions to operate literally at the same time (even if they contain delays). Fortunately, the results of using this "pure"

modeling approach are indistinguishable from the way we normally do it, with the one-iteration glitch, as long as no variable changes a lot in one iteration. If we run into computational oscillations, we just reduce the size of dt until they go away.

But the "pure" method is still important, because it forces us to deal with the simultaneity of operations in a control loop, and shows us very directly that we can't analyze the system properly as a sequence. Just by considering this problem, we make it clear that control systems do not operate in a sequence like "input, perception, comparison, error, output, feedback, input..." with just one event happening at a time.

Maybe Rick has posted my program for classical conditioning by now (I'm not reading ahead). When you see it, you will find exactly the arrangement I proposed, with a perceptual function that combines the value of the controlled variable with the first derivative of the value of a signal (the signal actually decays after its onset). So this system looks as if it's doing some sort of prediction. But you'd find, if you converted the model to Zocher's "pure" form of simultaneous operations, that the control system itself always operates in present time, with current values of all variables everywhere in the loop.

What gives the appearance of prediction is not anything inside the control system: it is the fact that the signal occurs prior to the disturbance of the controlled variable. The control system would work exactly the same way if the signal didn't occur, or if it occurred after the disturbance of the controlled variable. Of course we would see different relationships between its behavior and the variables involved -- we wouldn't see "prediction", for one thing. But the control system would still be the same control system with exactly the same properties. It would still not be doing any predicting. All its parts would still be acting simultaneously. But because we have changed the environmental relationships, we would see new relationships among the actions of the system and the various variables.

Let's take that a little further. Suppose, using the same model, we decided to pulse the "signal" variable four times before the controlled variable was disturbed. Suppose we decided to apply two successive disturbances of the controlled variable. Suppose we halved the effect of the action on the controlled variable, or introduced a constant disturbance of the controlled variable that was always present. In each case, of course, the model would do something; it would do whatever a model with that organization would do under those circumstances. Its behavior would show new relationships to the various variables.

BUT WE HAVEN'T CHANGED THE MODEL AT ALL. We're seeing all sorts of new behavioral relationships, but we aren't really discovering anything new about the behaving system. As far as the behaving system is concerned, it's acting exactly the same way it always acts: perceiving in a certain way, doing continual comparisons, and producing output actions based on the behavior of the error signal. If it's not predicting in one situation, it's not predicting in any situation. We only see prediction if we arrange the environmental events in a certain way that suggests prediction or anticipation to us.

When we model organisms, we aren't trying to model specific situations. We're looking for a model that will fit a specific situation, but which will also imitate behavior when we change the conditions, change the way the environment is arranged or the way it behaves, WITHOUT ANY CHANGE IN THE MODEL. If we had to change the model to fit every new environmental situation, we wouldn't have any model at all -- just a lot of ad-hoc curve-fitting. I'm hoping that when you start running the rats, we will find that a simple control-system model will reproduce their behavior no matter what type or degree of schedule they're on, with no changes in parameters or organization. I'm conjecturing that when we find a good model of classical conditioning, it will predict behavior under many oddball circumstances that a traditional experimenter would never have thought of. It will predict operant conditioning, too. Classical and operant conditioning look different to us because we arrange the environment differently in order to see them. But those are just changes in the environment; they aren't changes in the organization of behavior.

Martin Taylor (950420 11:00)--

Rick sez:

> **but its present time perceptions are also (and simultaneously) based on it present time actions.**

And you say:

> **Sorry. Should say "based on the actions at such past times as now are affecting the perceptual signal." Its present time actions have not yet affected the perceptual signal. You are mixing up the fact that all functions in the loop are acting simultaneously (which they do) with the notion that all signals in the loop have simultaneous effects all around the loop (which they don't).**

And you have missed the fact that Rick's statement is literally true: present-time perceptions are also and simultaneosly based on present- time actions. If all functions in the loop are acting simultaneously, then it follows that at the time any perception is varying, it is subject to the effects of present-time actions that are going on.

> **One can and must think in circles, but signals take time to go round and round the circle, even though the whole circle is there all the time.**

Yes, this is true, but present-time perceptions are still being affected by present-time actions. To focus too much on the loop delays is to miss the important fact, which is that all perceptions are affected by actions all of the time. In real human control systems, lags are relatively unimportant because inertial effects and integrations that would be there anyway take care of most of their deleterious effects. Given the physical properties of the neuromechanical systems, behavior would not be perceptibly improved if all the true transport lags were reduced to nanoseconds, or zero. And even if the lags were measured in minutes, it would still be true that perceptions are always being affected by present-time actions.

As to further discussions of information about the disturbance being in, or passed through, the perceptual signal, I pass.

Bruce Abbott (950421.1325 EST)--

Excellent summary of where we stand with respect to modelling various types of anticipation, prediction, conditioning, etc.. Fortunately, our models can't hear us arguing about the words we use: they just work as they work.

I'm up to the 21st and saturated. Until next time.

Best to all, Bill P.

Date: Fri, 28 Apr 1995 08:43:13 EDT
From: "CHARLES W. TUCKER" <N050024@UNIVSCVM.BITNET>
Subject: On Parsing Problems

#%#%#%#%#%#%#%#% FROM CHUCK TUCKER 950428 %#%#%#%#%#%#%#%#

ON PARSING PROBLEMS (RE: Rick Marken (950424.1300)

My main purpose for parsing controlled variables into a temporal sequence was not to "explain" an act but rather to encourage persons to supply an example of some "doings" when discussing certain questions. How do I know if "anticipation" is involved in an activity w/o having an example of the activity? It may be easy for persons who limit their examples to computer programs or tracking acts but for me it is not that simple. In fact, if you wish to convince most people that I know that PCT is useful for understanding their own acts, computer programs and tracking acts are least convincing [I gather much "evidence" on this every semester].

The parsing I presented comes from Bill's 1979 chapter in Ozer's book where he used it to show how the S-R formulation does not account for "behavior" (which we know as a "controlled variable" or "the control of perception") [So far as I know John Dewey never parsed an act!] The specification of the "behavior" ("controlled variable") is arbitrary because it can be a small as a "neuron firing" and as large as a total body movement (e.g., "getting into the truck"). The temporal arrangement is, for the most part, arbitrary since there is rarely only one set of actions in a particular sequence which is necessary for an act to be accomplished (e.g., there is A way to get orange juice). But it should be noted that is would be difficult to "light a cigarette" without first having a cigarette or "get into a truck" if the truck was not present. So, both of the complaints that you have, Rick, are valid. But there is another matter you don't mention that is a problem w/ parsing. It is multiple intentions or controlled variables in any act.

All organisms are doing a number of activities at the same time. As I write this on a piece of paper I am controlling sitting, breathing, hand movement, gaze direction, vision and covert verbalization to mention a few of my activities. Each of these can be parsed and could be arranged in a "temporal sequence" or "time line" and if all the activities I am performing now were parsed the chart would be quite complicated but I think wouuld be very useful. Some of these activities involve higher levels in the hierarchy while others don't go very high. But such a chart would illustrate that humans are controlling a number of variables simultaneously both horizontally and vertically. If this were done more often it would influence those folks who have an interest in PCT so they might see its relevance for their activities.

None of the computer programs or tracking demos incorporate multiple controlled variables ("CROWD" has several but not enough of them to simulate action in the non-simulated world; Spread sheet and Little Man have several levels) People who do the demos do use multiple controlled variables when they do them but no one has ever described these activities (I don't even present the traces just the r's). So what is needed is a program which more closely simulates what a person does in everyday activities which is to simultaneously control many variables.

A question: What disturbance could be introduced while you are driving that would indicate that you are controlling for "gear shift position" while you are also controlling for "the picture in the windshield" or the "picture in the side view mirror" or the "picture in the rear view mirror" especially when you don't have your hand grasping the knob at the end of the gear shift stick (you have both hands [one at the 9AM position; the other at the 2PM position] on the steering wheel)?

Date: Sat, 29 Apr 1995 00:58:25 GMT
From: Michelle Martin <martin.611@OSU.EDU>
Subject: Interview You?

I am a student at Otterbein College in Ohio (this is my wife's account). I need to interview a few people for a paper I will write soon. I will either focus on affective, anxiety or psychotic disorders depending on who I interview and what their experience is.

If you have some practical experience with any of the above and would not mind either a phone, chat mode or even a newsgroup interview, PLEASE e-mail me at martin.611@osu.edu. Maybe we could get a few people to do this over the weekend or early next week on a newsgroup so that others could partcipate and give input. That might be interesting for everybody as well as being invaluable to my paper. I will check these newsgroups tonight if you would prefer to reply this way. Thanks.

Chris Martin

Date: Fri, 28 Apr 1995 14:02:45 EDT
From: mmt@BEN.DCIEM.DND.CA
Subject: Re: Disturbing Words

[Martin Taylor 950428 12:00]
>Clark McPhail Wed, 26 Apr 1995 19:16:18 -0400 (in header)
+Rick Marken (950427.1030)

> One of the most useful PCT discussions I have heard of symbol acquisition and communication came from Martin Taylor at the CSG meeting in Durango in 1993. I will not attempt to summarize his position here but hope that Martin will take this occasion to reintroduce his PCT analysis of language acquisition and use. What we say makes a difference.

Well, thank you. It was easier in Durango, because I could talk with pictures. As I remember, I had previously said all the same things on CSG-L that I later said in Durango 93, but on CSG-L, without the pictures, it must have been harder to understand. I'll try to develop a posting that restates the case, but it may take some time.

We have had some excellent (in my opinion) discussions of language on this net but it seems like the last sustained discussion od langauge happened a couple years ago. It would be great to have more of these discussions;

I agree. But first...
Now For Something Completely Different...

(me)

> But if you think of sequence control as representing some kind of anticipation (that the reference sequence will in fact produce perceptions aiding the higher level control), ...

In sequence control, a perceptual function is determining whether a sequence of elements, like A,B,C is _in progress_. If each element occurs in sequence the perceptual signal remains "high" indicating that the sequence IS in progress; if an element occurs out of sequence (A,C,B) the perceptual signal goes "low" indicating that the sequence is not currently in progress. If the control system's own outputs are contributing to the production of this sequence it will seem like the output that produces, say, C after B has anticipated the occurance of B. But this is not really an anticipation because the control system doesn't assume or predict the occurance of B after A; the perceptual function is LOOKING for B after A and C after B but it is not really anticipating it.

All well and good, but when I suggested that sequence control could be seen as "anticipation" or "prediction" I was looking at the other side of the control system--the reference that indicates that "this" sequence is desired and not "that" sequence. The control of sequence, if it means anything, is the control of perceptions such that a particular sequence DOES happen, not a passive observance that it IS happening (or not). And that particular sequence is desired because if it happens, then there is an anticipation that some higher level perception will be brought nearer its reference (I take that higher level probably to be program level, where there can be control in imagination involving a choice of which sequence is "anticipated" to produce the desired effect).

The reference sequence is present AS A REFERENCE, before any of the sequence events have occurred. And it is in that sense that I said that sequence control could be taken as a form of anticipation or prediction.

Now back to the Spanish Inquisition, as everyone expected (didn't they?)...

My thesis in Durango was that "language" can be considered an artefact, a perceptible object in the same sense that a rock is an object, despite the obvious fact that "language" is never physically present other than in specific instantiations of particular utterances. In other words, "parole" may be directly observed, but nevertheless "langue" is an equally valid object of perception.

Language is seen as a set of conventions that come to exist by means of the reorganizations of interacting control hierarchies. If you want to see what I mean by that, ask Dag for the videotape of Durango 93, or wait until I have some time to try to put it into writing. For the time being, though, the word "convention" may cause a problem. A "convention" is not a perception on the part of a person who uses it, any more than the output implementation of a control action is a perception on the part of the actor. But a convention IS a perception on the part of an outside observer of patterns of communication. It is a kind of "ideal" in the same sense that one never actually sees a true circle, even though one has a very good idea of what a true circle is. One never actually perceives an ideal convention in the use of language, but one sees language acts that serve the perceptual control purposes of the talker/writer, in ways arbitrarily close to the "ideal" convention.

One may note that a lot of political "facts" are included with, and are generated in the same way as, language conventions. They exist because of the interactions of independent control hierarchies, whose reorganizations minimize conflicts. One way of reducing conflicts is to agree on "facts," without reference to evidence. Such "facts" are relatively stable against counter-evidence, since to accept the counter-evidence would be to increase conflict in the interaction with other people, leading to reorganization which is likely to restore the prior lower-conflict situation.

I'll see about trying to produce a posting on the language issue, based on the Durango talk. Thanks for asking.

Martin

End of CSG-L Digest - 27 Apr 1995 to 28 Apr 1995
****

Date: Sat Apr 29, 1995 10:39 pm PST
Subject: CSG-L Digest - 28 Apr 1995 to 29 Apr 1995

There are 6 messages totalling 763 lines in this issue.

Topics of the day:

Date: Sat, 29 Apr 1995 07:27:00 EST
From: Hortideas Publishing <0004972767@MCIMAIL.COM>
Subject: Perceived integrity

[From Greg Williams (950429)]

I didn't delete this one. Reorganization works!

>Bill Powers (950427.1038 MDT)

> You imply that I have sold out my scientific integrity in return for the promise of increased book sales; that anyone who could come up with my price could get me to withhold criticism. If that's your assessment of my character after our ten years of acquaintance, I'd say you need more practice in making judgments.

I have been trying to say that if you and the other CSG authors with books in the NVP catalog are complacent about NV's characterization of "Control Theory," it won't surprise me if some _other_ scientists -- who do not enjoy my benefit of long personal acquaintance with your high standards of scientific integrity -- perceive that the scientific integrity of some PCTers is for sale and even generalize that the scientific integrity of PCTers generally is low. It matters not what _I_ believe to be the case; what matters is whether non-PCT scientists becoming interested in PCT are "turned off" because of _their_ beliefs about the scientific integrity of PCTers -- beliefs influenced by NVP publicity.

It is certainly reasonable for you to act indignantly about even the slightest hint that someone might think PCT scientists can be bought after seeing the NVP catalog -- but I hope you _also_ will act to defuse even the slightest hint that such is the case. Having good models isn't going to matter to someone who doesn't even look at the models because he or she thinks PCT appears, as per NVP publicity, akin to the many self-help systems being hawked far and wide. Sure, a class act is occasionally discovered in a dive. But only occasionally, because most of the talent scouts are uptown. Those undiscovered class acts on the wrong side of the tracks aren't any less classy, just undiscovered.

When you make up your own mind on this, I hope you will consider those who know little about your mind. You can press on regardless (that is, be complacent about NVP advertising), but in my opinion, that could slow the acceptance of PCT significantly. I believe that from our long acquaintance you appreciate my passionate committment to aiding that acceptance, even if it sometimes requires disturbing you.

As ever, Greg

P.S. Final typesetting files for Gary Cziko's important (in my opinion) _Without Miracles_ book go out today. If the MIT Press imagesetter works, it should arrive in stores in July or August.

Date: Sat, 29 Apr 1995 12:49:52 -0400
From: DForssell@AOL.COM
Subject: PCT @ AERA

[From Dag Forssell (950429)]

When I post INTROCSG.NET Monday, it will include a new offering. The video tape PCT @ AERA featuring Hugh Petrie, Bill Powers, Gary Cziko, Ed Ford and Dag Forssell will be listed with other offerings from Purposeful Leadership. The color was corrected somewhat by the duplicator who converted from my 8mm original to VHS to remove some of the red caused by incandescent light in ceiling. You can adjust color further on your TV set, until it looks quite normal. The tape is 120 minutes, VHS, NTSC and I ask for $10 plus $5 shipping.

Anything else for INTROCSG.NET????

Best, Dag

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Date: Sat, 29 Apr 1995 13:46:43 -0600
From: "William T. Powers" <POWERS_W@FORTLEWIS.EDU
Subject: More misc catchup

[From Bill Powers (950429.0915 MDT)]

Oded Maler (950424) --

> How about: "the attractors of the dynamical system of neuro- chemical networks can be considered as control of perception"?

Got it backward: the control of perception is a literal description, the appearance of attractors is a metaphor (nothing is actually "attracted by" or "pulled to" a reference signal).

Martin Taylor (950420.1820)--

Remarks to Rick:

> Actually, I stopped believing in cause-effect relationships long before I heard of PCT, perhaps 10-15 years before. .... A "cause" is something like, for example, a waveform generator that puts a signal on a line, or a gust of wind that pushes the proverbial car. The "disturbance effect" is the influence of that cause on something that IS perceived. The cause is not.

> You insist on saying that I talk about the information "in" the perceptual signal when I have tried several dozen times to correct that misconception. The perceptual signal in a functioning control loop never "contains" information about the disturbance. The use of that information by the control system destroys it, so far as the perceptual signal is concerned. ....

> The argument is that the perceptual signal passes (not contains) information about the disturbance effect, and indeed that it is ONLY this fact that allows the control system to operate at all.

Martin, you often appear to speak in self-contradictions. If you do not believe in cause-effect relationships, why say that a waveform generator that puts a signal on a line or a gust of wind pushing on a car is a cause? If information in the perceptual signal about the disturbance is "destroyed," it must have existed there first in order to be destroyed. And if it is "passed" by the perceptual signal in order to be "used" to allow the control system to operate, it must not, after all, have been destroyed -- else there would have been no information to pass or use. It is hard to conceive how information could be "passed" by the perceptual signal without also, during transit, being "contained" in the perceptual signal. It is even more difficult to grasp how a control action could "destroy" information in the perceptual signal while "using" that information to achieve the destruction. I simply can't follow your reasoning processes.

I think we can agree that the perceptual signal is a representation of the state of a controlled quantity (that an external observer could see or infer). Since any particular state of a controlled quantity can be established by an infinity of combinations of physical variables which contribute causally to that state, it is impossible for the perceptual signal to indicate which combination of external effects is actually responsible for the state of the controlled quantity. You seem to agree with this, yet you also seem to insist that the perceptual signal can, somehow, indicate what part of the controlled quantity's magnitude is due to one cause or another one.

In fact, the control system needs only two pieces of information in order to operate: the current state of the controlled quantity as represented by the state of the perceptual signal, and the current state of the reference signal. Its actions are driven by the difference, and tend to change the controlled variable in the direction that reduces the difference. It is not required that the reason for existence of a difference be known -- whether it exists because of unknown external influences, spontaneous fluctuations in the output, changes in the reference signal, or all of these. All that matters is the difference itself. THAT is what makes a control system work.

The reason that Rick and I, to your great annoyance, keep returning to a feeling that you have not abandoned S-R lies in your continued attempts to find some way for the action of a control system to be based on information about the disturbance that is contributing to differences between perception and reference. My impression is that you feel in your bones that SOMEHOW the environment must play a part in guiding control actions to have the right result.

You have often referred to properties of the disturbance such as bandwidth and spectrum as having determining effects on control behavior, as if it does make a difference to the operation of a control system just how disturbances originate and what their temporal properties are.

If you could just accept that a control system needs NO information about the world except about the state of its own controlled quantity, you would see that control depends on properties of the closed loop and on nothing else. Even the dynamics of control can be optimized using that information alone. When you start worrying about designing control systems for particular kinds of disturbances, you miss the whole point that control systems work for _all_ kinds of disturbances, from steady disturbances to those that tax the physical capabilities of the system. A "random" disturbance includes every kind of disturbance, from ramps to square waves to constants to multiple superimposed sine-waves -- all of which can occur in reality from one occasion to another, and all of which a good control system can successfully counter without any change in its design -- within its physical limits.

RE: prediction

> There's no way to tell the difference between a computed prediction and a prediction built into the parameters of the control loop.

Yes, there is: in a computed prediction, the predicted value of a variable appears explicitly as a separate variable. The remainder of the control action then follows from the value of this predicted variable as if it is an ordinary present-time variable.

I think you are confusing the way an observer could classify the operation of a control system with the actual operations performed by the components of that control system. When we think in terms of a general category of operations, we can see many processes as "having the same essential characteristics" even when the processes are completely different. This is the well-known property of abstraction that discards differences in favor of seeing similarities.

I am quite sure that we will need models that really do predict and really do anticipate. I think these models will consist of symbolic operations, program-like operations, in which all the individual processes involved appear explicitly in different parts of the models. There will be no choice but to see the processes as involving explicit prediction and explicit and intentional acts calculated to occur in advance of other events. All this is quite appropriate for a high-level control system.

However, if we allow ourselves to impose our own perceptual modes of classification on all processes regardless of their differences, we will find that we can see prediction and anticipation occurring in all control systems at all levels of organization, even the spinal reflexes. The stretch reflex involves a proportional-plus- derivative length- difference signal. Are we to call this, too, prediction or anticipation? If so, then we are no longer talking about levels of perception and control, functional levels in the whole model, but only about dynamical details of the design of any control system. Prediction and anticipation then lose their character as identifiable modes of behavior, and become just informal names for derivatives and temporal relationships we can see in systems of all kinds, even non-controlling systems.

> Adaptive control is all about modifying the parameters so that they better predict.

No, it is about modifying parameters so that perceptual signals come closer to reference signals. If you make the predictions better but the errors become larger, you have not achieved "adaptation." Prediction is not an end in itself, is it?

> **When you talk (as you have) about the "predictive" changes in a reference signal from a higher level as being only the results of that higher level's control of its own perception, you are absolutely correct. But that "only" makes the result no less predictive, from the more restricted viewpoint of the lower-level control that receives these apparently magical advance warnings of disturbances yet to happen.**

This is word-salad. The lower-level systems don't know why their reference signals have changed. Sometimes they change in anticipation of disturbances to keep the controlled variable from changing, and sometimes they change in order to alter the value of the lower-level controlled variable. The lower systems receiving these signals can't know whether they're involved in a predictive or anticipatory process.

Martin, if you want everything to be "predictive" you can certainly find language that, if not looked at too closely, will make it seem so. I don't see any point in doing that.

Alice (950421.1045 EST)--
Humpty Dumpty (950420.1930) --

There are some ways of describing experiences that simple describe them. But there are others that not only describe the experiences, but make assertions about other things at the same time. This is a good part of what we are arguing about.

A word like "cue" is an example. In front of me on the table is a ball- point pen. Is this a ball-point pen, or is it a cue? To call it a ball- point pen is simply to name what I am looking at, without implying anything about its relationship to other things, the implications of its being there, or its ability to make marks.

If I call it a cue, I am really talking about more than the object I see before me. I am asserting a relationship between this object and other objects or events. I am asserting a _function_ of this object; it is there in order to indicate something else. That is what makes it a cue and not just a pen.

Similarly for other S-R words. To call the pen a "stimulus" is to assert something that the pen can do to sensory inputs of an organism, and by custom to assert that it is at least potentially capable of causing some "response." To call it a "reinforcer" is to assert that it is not only a stimulus, but has an effect on the way I behave relative to other stimuli (for example, it might be a reward to me for good behavior). To call it a "discriminative stimulus" is to assert that it signifies that some rewarding action, if now taken, will succeed. To call it a "predictive stimulus" is to assert that of all the succeeding events that regularly follow the appearance of the pen on the table, some particular event of importance to me is about to occur.

Yet through all of these descriptions, the fact remains that I am still looking at a ball-point pen and nothing more.

Words like cue, stimulus (various flavors), and reinforcement are S-R words precisely because they do NOT serve merely as names for observable phenomena. We have plenty of words for the observable phenomena, but these special words carry

hidden theoretical assertions peculiar to S-R theory: they put the simple phenomena into categories that are the basic elements of S-R theory. A bell that sounds five seconds before a puff of food powder is administered is no longer just a bell that sounds when it does; it is a conditional stimulus. That puts the bell into the same category as all other events that are considered to be conditional stimuli, and says that it shares the properties of all other events in that category: particularly, the ability to cause a conditional response (another category, a category of actions).

There are, of course, also PCT words: disturbance, controlled variable, action, feedback connection, and so forth. If I say that the ball-point pen is a disturbance, I am asserting that its presence is tending to alter some variable I am controlling, and implies that I will take some action to restore that variable to a particular state, a reference state. These PCT words also make theoretical claims in terms of categories, just like the S-R words. The PCT words go beyond simple description just as the S-R words do.

If we understand that there are PCT words and SR words, we can be sensitive to the theoretical assertions that are part of their normal usage. We can become aware that the specific phenomena before us are not the only things being described: we are hearing the names of theoretical categories which imply whole systems of understanding.

Most important, we will not give either set of terms any special ontological status (there, I knew I would be able to use that word some day). That is, to call a movement a "response" is not just to name what we observe, but to assert a theoretical framework in which movements are caused by external processes and represent the end-point of a behavioral episode. To call the same movement an "action," on the other hand, is to assert that it is produced by the organism as a means of causing a change in something else -- in PCT, a perception.

Whatever theoretical framework we are using, we always have available another level of description that contains no theoretical assertions. We can say that the animal pressed the bar in some temporal pattern, and as a result food pellets periodically dropped into a dish and were eaten. We can say that over time, the rate of bar-pressing came to an average value, and the rate of pellet-delivery and consumption came to some other average value. With care, we can offer a completely noncommittal description of what is observed, never using a term that implies any theoretical interpretation that goes beyond what we can see. When we do that, we can truly say that we are simply reporting our observations.

However, the moment that we begin to describe our observations with words like reinforcement, response, cue, stimulus, disturbance, controlled variable, or reference condition, we have begun to overlay the observations with theoretical interpretations. At that point, to claim that one is merely describing phenomena is simply incorrect. This is not description, but interpretation.

What's important when we are comparing theoretical points of view is to realize that the theoretical terms we use can become so familiar that they seem like simple descriptions when in fact they are heavily laden with theory. This is particularly true when most of the people we interact with believe in the same theory -- we point to the ball-point pen and say "Ahah, there's the perception I was controlling for," and everyone else knows what we mean, or thinks they do. The behaviorist says "I observed 5000 responses for every reinforcement," and his colleagues know what he means, or think they do. The theory starts to be taken

for granted, and the theoretical entitities seem to take on a solid existence out there in the perceived environment.

Wow, look at all that phlogiston coming out of that bonfire!

Cue is an SR word. Disturbance is a PCT word. When all else fails, try just naming the thing instead of categorizing it. Maybe it's just a ball-point pen lying on the table.

Almost there in catching up.

Best to all, Bill P.

Date: Sat, 29 Apr 1995 16:56:50 -0400
Subject: neurosemantic dynamics; PCT Biology

[From John E. Anderson (950429.1530 EDT)]

Rick Marken (950404.2120 PDT)

John E. Anderson (950404.0630 EDT)--

> I have been developing a theoretical model of the brain called "neurosemantic dynamics" (NSD).

>> What is NSD a model of? That is, what phenomena does it explain?

Sorry it's taken me so long to respond. NSD is a biological model of synaptic activity in the brain. It is interesting that the CSG-L discussion of prediction came along just now, because at the heart of NSD is a mechanism by which an organism could anticipate how its perceived surroundings will change. I think the ability to predict such changes would be very important for the organism's survival, because it would facilitate a quicker reaction, which in critical situations could make the difference between life and death, and probably wouldn't hurt in less critical ones. For this reason, the ability to predict could have contributed over the course of evolution to the development of some of the features of existing nervous systems, including our own. I believe the predictive mechanism proposed in NSD could have contributed to the evolution of functional localization in nervous systems, especially in more central regions like those involved in language, concepts, etc, whose functional topography would be less constrained by the physical anatomy of sensory and motor organs than more peripheral regions.

The prediction mechanism in NSD is likely to be UNlike any that most CSG-L people have had in mind during the thread on anticipation. It is based upon the fact that, at least in my own experience, changes in perceptions of the physical world are usually continuous. This "continuity constraint" means that the perceived surroundings at one moment are only slightly different from those at the next moment. One of the hypotheses of NSD is that neurons whose activities elicit similar functional responses are clustered together in the brain. This is localization of function, which is usually exemplified by the organization of the brain into visual cortex, somatosensory cortex, language areas, etc, but I mean

functional clustering on a much finer scale. When functionally similar neurons are clustered together, the neuronal activity corresponding to the perceived surroundings at one moment will occur near the neurons whose activity corresponds to the perceived surroundings at the next moment, because the neuronal responses to similar perceptions should themselves be similar.

Let me emphasize that the responses I am talking about here are _neuronal_ responses, NOT the overall responses of the organism. As anyone familiar with PCT knows, the whole organism might respond in very different ways to the same perceptual input on different occasions, depending on which reference signals are active and what their values are. The responses of the neurons receiving the input, though, will be essentially the same each time.

NSD proposes that when a neuron is active, it releases a neuroactive substance, which diffuses into the surrounding brain tissue to form an "envelope" around the active neuron. The neuroactive envelope transiently increases the sensitivity of any other neurons it comes in contact with. I have simulated the effect of such an envelope on single neurons (using, by the way, the GENESIS neural modeling program Bruce Nevin posted about here on 950104), and have shown that it makes them respond more quickly to subsequent input. Due to the continuity constraint and functional clustering mentioned above, initial perceptual input representing the surroundings will be followed within milliseconds by activity induced in adjacent neurons by new input from the surroundings. Because the adjacent neurons are also sensitized by the neuroactive envelope from the initial activity, their output will occur sooner. The net effect will be that the animal will be able to decide what to do more quickly; it can predict the state of the surroundings in the immediate future. It does not predict it specifically, and it could be wrong if there was an explosion or some other abrupt change, but most of the time, changes will be slight and the right neurons will be sensitized. I can email to interested persons a more detailed synopsis of NSD (25522 bytes), though with no connections drawn to PCT, as I am just now beginning to do that; email me at jander@unf6.cis.unf.edu if you'd like to have one. (Rick and Bill P., I am going to email a copy to you as soon as I post this to CSG-L.)

I said:

>> (The name [neurosemantic dynamics] stems from the theory's suggestion that on the most fundamental level, the meaning of a neural signal is the neural signal it induces.

And Rick commented:

> This sounds strange but maybe it is similar to the PCT model. In PCT, the meaning of a neural signal is determind by the perceptual function that produces the signal as output. If the inputs to the perceptual function are neural signals, then a particular value of perceptual signal is "induced" by these inputs, but the "meaning" of perceptual signal variations, regardless of the value of the signal at any moment, is determined by the function that tranforms inputs into outputs.

> For example, suppose that p = x+y where p is the perceptual signal, x and y are input neural signals and "+" is the function transforming inputs into outputs. Then the "meaning" of p is "sum"; the particular value of that "meaning" at any instant depends on the value of the inputs to the perceptual (summation) function.

Our definitions of meaning seem to literally have opposite orientations, maybe because I think about meaning in biological terms while you think in mathematical terms. My view is that the meaning of a particular input neural signal is determined by what is done with it by the part of the organism's nervous system that receives the signal. The input signal induces activity in the receiving neurons, whose output becomes the input for another region, and so on. Sooner or later, the activity occurring downstream as a result of the original input signal, together with all the rest of the neural signals in the brain at the same time, including reference signals, contributes to some action the organism carries out. A different input signal would induce a different local output, which would eventually combine with the rest of the neural signals to produce a different action by the organism. This means that the different input signals have different meanings to the organism; one means do this, the other means do that. If we call the neurons receiving an input signal a perceptual function, then I agree that the meaning is determined by the function. But I think that the meaning in question is the meaning of the input signals, and that their most fundamental meaning is the output signal. When you understand something, when you can assign it a meaning, you understand what to do with it. For example, when you hear and understand a sentence, you know what to say in response to it. Of course, your response depends on the global state of your brain, what point you are trying to get across, etc; in PCT terms it depends on your reference signals as well. In your specific example, I would define the meaning in the other direction: the meaning of the input signals x and y is the output signal p.

I said:

>> a lot has been learned about nervous system structure and function in the 22 years since it [BCP] was published. Has there been any further development of its relationship to the PCT control hierarchy since B:CP?

Rick answered:

> Not nearly enough. Perhaps you could mention some of the more important things that have been learned about the NS in the last 22 years and we can kick around ideas about what these findings suggest about possible architectures for the PCT model (and vice versa).

A lot more is known about the connectivity between regions, especially in the neocortex. For example, David van Essen, a neurobiologist who used to be at Caltech and is now at Washington Univ in St Louis, makes connectivity maps between regions of monkey cortex. I remember seeing a comparison (can't remember where, but if I can locate it I'll post the source) between a map made around 1985 and one made around 1990; the increase in complexity of the map was incredible. As I mentioned in my original post, I think PCT offers a unique way to look at this kind of extraordinary complexity. It seems to me that correlating NS structure to PCT control function would be a novel and potentially very useful way to look at mental illness, as well as to understand the functioning of normal brains. So I think an important piece of work would be to try to relate current knowledge about nervous system anatomy to the PCT control hierarchy, and I am thinking about putting together a grant proposal to do just that; advice and criticism is welcome. If it can be shown that nervous system anatomy is in fact consistent with its being a hierarchy of perceptual control systems, and if the known function (from brain lesions, etc) of certain regions

can be shown to be consistent with their proposed position in the hierarchy, this should go a long way towards converting the legions of PCT nonbelievers.

Regards to all,

John

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Date: Sat, 29 Apr 1995 17:11:47 -0600
From: "William T. Powers" <POWERS_W@FORTLEWIS.EDU>
Subject: Re: Hate talk

[From Bill Powers (950429.1540 MDT)]

Rick Marken (950426.0900) --
Gary Cziko (950426.0305 GMT) --

RE: Hate talk

Gary asked the following about Rick's powerful post on hatred, and Clark McPhail's commentary on it:

> **...how it is that "words can make a difference."... you must feel that hateful words can cause at least some of the people who hear the words to hate, too, or else you probably wouldn't be concerned about the hateful words. But how does the suspected effect of hateful words mesh with PCT? How can words cause one to hate or do anything if hateful reference levels are not already there? This looks like an input-output (S-R) view of behavior to me.**

While Rick's answer denies that one person can cause another to hate (and I agree), I think we need to look into this further, because a lot of hatred seems to be spread through words -- and not only hatred, but fear, love, plans, and knowledge. I've been puzzling about this for several days, and not for the first time. Clark McPhail and Chuck Tucker have told us often in their writings that "words make a difference," but we've given only a superficial treatment of just how they make a difference. There's a lot more to this subject that I can deal with, but a few ideas have occurred to me.

One of the ideas came about through reading a story, one of the mysteries I like to enjoy as an alternate reality. Reading a well- wrought story is an interesting experience because the reading itself quickly turns into a complex and detailed world that unfolds itself in imagination under the direction of the words that one takes in by eye. The mind's eye is not on the printed page, but on a village scene, a conversation between two characters, a journey by train through a half-familiar landscape, a stroll through an old churchyard, a participation in

someone else's consciousness as he or she puzzles over some problem, enjoys some experience, suffers a shock, solves a mystery. For a while one is somewhere else and often someone else, learning about human nature through another's point of view, learning facts and attitudes as if discovering them for oneself.

Vivid writing is not just a narration of sequential occurrances. It describes details of sight, sound, smell, touch, pain, and pleasure so that experiences of many levels are evoked in the reader. The reader creates, from these low-level experiences, higher-level perceptions that are not explicitly described but which fill in the more abstract descriptions and give them life and presence. Not "He sympathized with her plight," but "He reached for her hand and found it cold and trembling; he looked into her small sad face and ached with sorrow for her."

So it seems very clear that words can create experiences very much as if they were real experiences coming from the normal world. And it is equally clear that these experience do not actually have to happen in order to be believed in, at least temporarily. This is a power that one human being has over another: the power to create experiences in the other person merely through the medium of words.

We can't, of course, interpret those experiences for the other person. But by creating experiences we can give the other person something to think about, and we can often make an educated guess as to what the other person might decide to do about these experiences. I am finally coming to my point.

When you hear hate talk or read hate literature, what do you find? Do you find statements like "Black people ought to hate Jews" or "White people ought to hate black people?" Not at all. What you find are stories about things that happened or are happening now. Jewish biologists created the AIDS virus in order to infect black people with it. Once a white woman has been raped by a black man, she is so enslaved by the sensations from his enormous penis that she can no longer be satisfied by a white man. The FBI, the CIA, and the White House are planning to make common citizens register all their weapons so that when the time comes to install the Clinton Dictatorship, federal marshalls and troops can swiftly seize all private weapons and prevent resistance to the takeover. If you have read the Protocols of the Elders of Zion or the literature promulgated by the John Birch Society or the KKK or other such groups, you will be familiar with dozens more stories like these as well as others more extreme.

Just suppose for a moment that you were innocent and ignorant, and heard these stories for the first time from a person you had no reason to disbelieve. Any one of them _could_ be true, as far as you know: that is the art of story-telling, to create a believable narrative. And what if they were true? What if it never even occurred to you to wonder if they were true -- what if you just took them as given, as if you had known these things from your own experience. What then would you want to do? I think that a lot of people would feel full of anger, outrage, or fear, and would wish to do something to counteract these horrible things that are going on. How did you feel, when you first heard the descriptions of the scenes discovered in Buchenwald and Auschwitz by the liberation troops? Was there not horror, and hatred for those who did such things, and a desire to exact justice, however futile it might be after the fact? Remember, very, very few of us actually saw those scenes ourselves, in our own present-time perceptions. We saw pictures, we heard stories, and we translated them into experiences of our own, almost as if we had actually experienced them. And we had feelings about these indirect experiences, and we wanted to do something about them. We spoke of

the damned Nazis just as others might speak of the damned blacks or the damned Jews or the damned Government.

So maybe this can give us some insight into what it would be like to be a member of a so-called hate group and believe in all the things such hate groups say and do. These are not particularly hate groups; they are fear groups, and outrage groups, and escape groups, and self-protection groups, and justice groups. All we have to do is realize that they take the stories literally and seriously, as real things being done by real people, now, to other real people. All we have to do is ask ourselves what we would think and do if, by some monstrous chance, some or all of these terrible stories were actually, literally, true: if we had actually perceived for ourselves what the words describe.

When we approach the problem from this angle, the real problem would seem to be not hatred, but gullibility, an inability to judge what is and is not a likely story. To this, of course, we have to add questions about what could make some people at certain times in their lives more willing to suspend disbelief and swallow stories with obvious flaws and contradictions in them; what could lead some people to trust their imaginations more than their senses. But basically we aren't talking about crazy people, about people behaving in inhuman or incomprehensible ways. We are talking about people who are fearful, or outraged, or offended by certain events, and who are trying to maintain control of their lives despite these events. The fact that the events never happened is almost a side-issue -- except, of course, that in the final analysis it is only the realization that these events never actually happened that can persuade the hate groups to abandon their pitiful efforts.

The next time you hear discussions of the hate groups, listen carefully. They don't preach hate. What they do is tell stories, and leave the hating up to the listener who believes the stories.

Best to all, Bill P.

Date: Sat, 29 Apr 1995 16:58:12 -0700
From: Richard Marken <marken@AEROSPACE.AERO.ORG>
Subject: Anticipation, Evolution, NVP Science

[From Rick Marken (950429.1700)]

Chuck Tucker (950428) --

> if you wish to convince most people that I know that PCT is useful for understanding their own acts, computer programs and tracking acts are least convincing

I think a number of non-tracking behavioral examples of apparent anticipation have been given. Classical conditioning is one. Shock avoidance (Bruce A. has done this kind of research, I think) is another. Real world examples are rife; I prepared a room in _anticipation_ of the Powers' visit. I took an umbrella to work in _anticipation_ of rain. I am putting money into a retirement account in anticipation of retiring (well, I was until my kids started college;-). I am turning right off Beverly Glen in anticipation of reaching the 405.

> None of the computer programs or tracking demos incorporate multiple controlled variables

The Excel version of the spreadsheet control hierarichy controls 18 variables simultaneously using six output varirables. The little man controls several variables simultaneously (I forget the number).

> A question: What disturbance could be introduced while you are driving that would indicate that you are controlling for "gear shift position" while you are also controlling for "the picture in the windshield" or the "picture in the side view mirror".

As the passenger you could throw the gearshift into neutral while turning the steering wheel counterclockwise. It would take some technical innovations to test whether the driver was really controlling the position of the gear shift or something related to stick position.

Gary Cziko (950428

> So while mutations continue to be produced blindly, the higher rate of genetic change allows the bacteria to stumble on the adaptive genetic change more quickly than they would if left in their normal glucose-rich environment.

Nice! Did you mention that the "stress" that drives this process is the discrepency between a specification for the state of a variable and a measure of the actual state of that variable? Even if you didn't, it looks like "Without Miracles" (a best-seller title if I ever saw one) will go right up there on my PCT shelf. I can't wait to buy a copy; I'm saving up already.

Greg Williams (950429)--

> what matters is whether non-PCT scientists becoming interested in PCT are "turned off" because of _their_ beliefs about the scientific integrity of PCTers -- beliefs influenced by NVP publicity.

I don't think many scientists will come into contact with PCT via NVP. I suppose, then, that your main concern is the scientist who gets interested in PCT via articles in academic publications and journals and subsequently gets "turned off" when he discovers that PCT is advertisted in NVP. But I don't think there is much chance that such a scientist would have ever gotten PCT anyway. If his main concern is fitting into the scientific establishment then NVP will be the LEAST of his problems with PCT.

Best Rick

End of CSG-L Digest - 28 Apr 1995 to 29 Apr 1995
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Date: Sun Apr 30, 1995 8:58 pm PST
Subject: CSG-L Digest - 29 Apr 1995 to 30 Apr 1995

There are 4 messages totalling 174 lines in this issue.

Topics of the day:

1. Willing suspension of belief
2. Word Of Appreciation!
3. Classical Conditioning Model v 1.0
4. Closed Loop Analysis

Date: Sat, 29 Apr 1995 22:35:19 -0700
From: Richard Marken <marken@AEROSPACE.AERO.ORG>
Subject: Willing suspension of belief

[From Rick Marken (950429.2230)]

Bill Powers (950429.1540 MDT)--

> When you hear hate talk or read hate literature, what do you find?...What you find are stories about things that happened or are happening now.

When we approach the problem from this angle, the real problem would seem to be not hatred, but gullibility, an inability to judge what is and is not a likely story.

To this, of course, we have to add questions about what could make some people at certain times in their lives more willing to suspend disbelief and swallow stories with obvious flaws and contradictions in them

My personal experience is that it is often difficult to disbelieve what I hear, especially when the story evokes a great deal of familiar detail. It is often an act of will for me to be skeptical. When I was a kid I just accepted many stories that were told to me; I still believe far too much of what I am told. I think this might be true of others too; I have noted a tendency for many people to believe something simply because it has been said (especially if it has been said well); and to believe it even more if it has also been written down.

It's much easier for me to be skeptical, even when a story is wonderfully rich with familiar detail, when I can see obvious internal inconsistencies or conflicts with experience. But skepticism is not an easy stance for me; I'm lucky that I managed to muster enough skepticism about the stories told in conventional psychology to see that they didn't match my experience. I have learned a great deal about skepticism from PCT. But what I have not learned yet is why it is apparently so difficult for people to be skeptical.

It seems that some people actually want to believe; they see belief in untested stories as a good thing; skepticism as a bad thing. My step-father (an avowedly deeply religious man) once asked me, in anger, "don't you believe in anything"?. I'm proud to say that it took me a few moments to think of some things I did believe in (number one, of course, being that "behavior is the control of perception"; that left him a bit cold). But what I should have said is "What is more important to me than the few things I believe are the many things about which I am skeptical".

There is obvious entertainment value to be derived from "willing suspension of disbelief"; but it seems that there is something about human nature that makes it a little too easy to suspend disbelief. I would like to know what we can tell people to improve their ability to willingly suspend belief.

Best Rick

Date: Sun, 30 Apr 1995 08:13:00 EST
From: "Edward E. Ford" <0005913466@MCIMAIL.COM
Subject: Word Of Appreciation!

Ed Ford 950430.early morning, AZ time

I think we all owe Hugh Petrie a word of appreciation for all his effort in bringing about our (Powers, Cziko, Forssell, myself) chance to present PCT at the AERA conference in San Francisco. The amount of work that goes into that sort of thing is immense and Hugh, who led the presentation, did a great job. It is this kind of effort that gets PCT known and respected. Thank you, Hugh. A job well done.

Ed.

Date: Sun, 30 Apr 1995 17:15:52 -0500
From: Bruce Abbott <abbott@CVAX.IPFW.INDIANA.EDU
Subject: Classical Conditioning Model v 1.0

[From Bruce Abbott (950430.1710 EST)]

Here's just a couple of brief comments about the model of salivary conditioning posted by Rick Marken, as co-developed by Bill Powers and him.

The exponential decay built into the "CS" perceptual input function helps to prevent the effect of the CS from summing with the effect of the "US" (food placed in the mouth). As far as I know, the evidence would not support such an input function, but I would want to have a much closer look at actual conditioning data before passing judgment. Early in conditioning, salivation is likely to occur throughout CS presentation, suggesting that its ability to act as a disturbance has not attenuated as the decay function would imply. (With more experience the UR tends to occur only near the end of the CS-US interval.) Clearly, the ordinary perceptual experience of the CS does not attenuate, which would seem to be at odds with the decay function; it would have to be a specific component of this control system's PIF rather than a property of the sensory receptors. This function would also seem to be at odds with the ability of CS effects to summate when two separately conditioned CSs are combined into a compound CS. I guess what we need to know is whether the CS and US disturbance effects summate when the two disturbances overlap. I have no experience with appetitive classical conditioning; perhaps someone on CSG-L does and can help us out.

In the simulation, food, once placed in the mouth, acts as a constant disturbance to the controlled perception, so that a constant rate of salivation eventually sets in, as if the food is "drying" the mouth at a constant rate. This simplification is probably sufficient for the model's purpose but it would be more accurate if the amount of food placed in the mouth were specified and a degree of "liquifaction" based on the food and saliva volume computed. The food would remain in the mouth but its disturbance value would diminish over time as salivation continued, being proportional to its current state of wetness.

One more comment: the "strength" of response to the CS is always some fraction of the "strength" of response to the US (at least for salivary conditioning) Thus, if food is capable of eliciting, say, 30 drops of saliva over the course of 15 seconds, then a fully conditioned CS would always produce less than 30 drops in the same time period. This limits the ability of the CS to act as a disturbance to the controlled variable.

Regards, Bruce

Date: Sun, 30 Apr 1995 17:17:22 -0500
From: Bruce Abbott <abbott@CVAX.IPFW.INDIANA.EDU
Subject: Closed Loop Analysis

[From Bruce Abbott (950430.1715 EST)]

Bill Powers (950427.1038 MDT) --

> _The whole control loop is active at the same time._ The transport lags we know are there do not turn this into a sequence of processes -- they just change the time stamp (slightly) on the information that each process is currently receiving.

I believe I follow you. In an ideal control system (one with infinite gain and zero lag), a step disturbance would instantaneously induce a step action on the part of the control system exactly equal in size to the disturbance but of opposite sign. There would be no change in either the perceptual signal or the error signal. With a continuously varying disturbance, the signal variance injected by the disturbance would appear instantaneously as signal variance in the output -- and nowhere else.

In a system with finite gain and zero lag, a disturbance would induce an instantaneous and simultaneous change in perceptual signal, error signal, and output signal. The changes would be exactly those necessary to establish an equilibrium involving just the right level of error to nearly counter the disturbance so that the perceptual signal would change just enough to produce the right error signal.

In a system with finite gain, finite lag, and having variables subject to finite rates of change, the disturbance changes propagate around the loop at finite speed. The transformations get complex as the uncancelled remnants of the disturbance signal recirculate like reflected and re-reflected waves, producing a complex pattern of interference with their own "reflections" and the current disturbance signal. However, these circulating "remnants" appear at any point in the loop merely as indistinguishable components of the current value of each variable; from the point of view of the control system, there are only current

values, which result from current values and in turn produce current values, both of themselves and of other variables (as each function currently "sees" them).

Have I got it right?

Regards, Bruce

End of CSG-L Digest - 29 Apr 1995 to 30 Apr 1995
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